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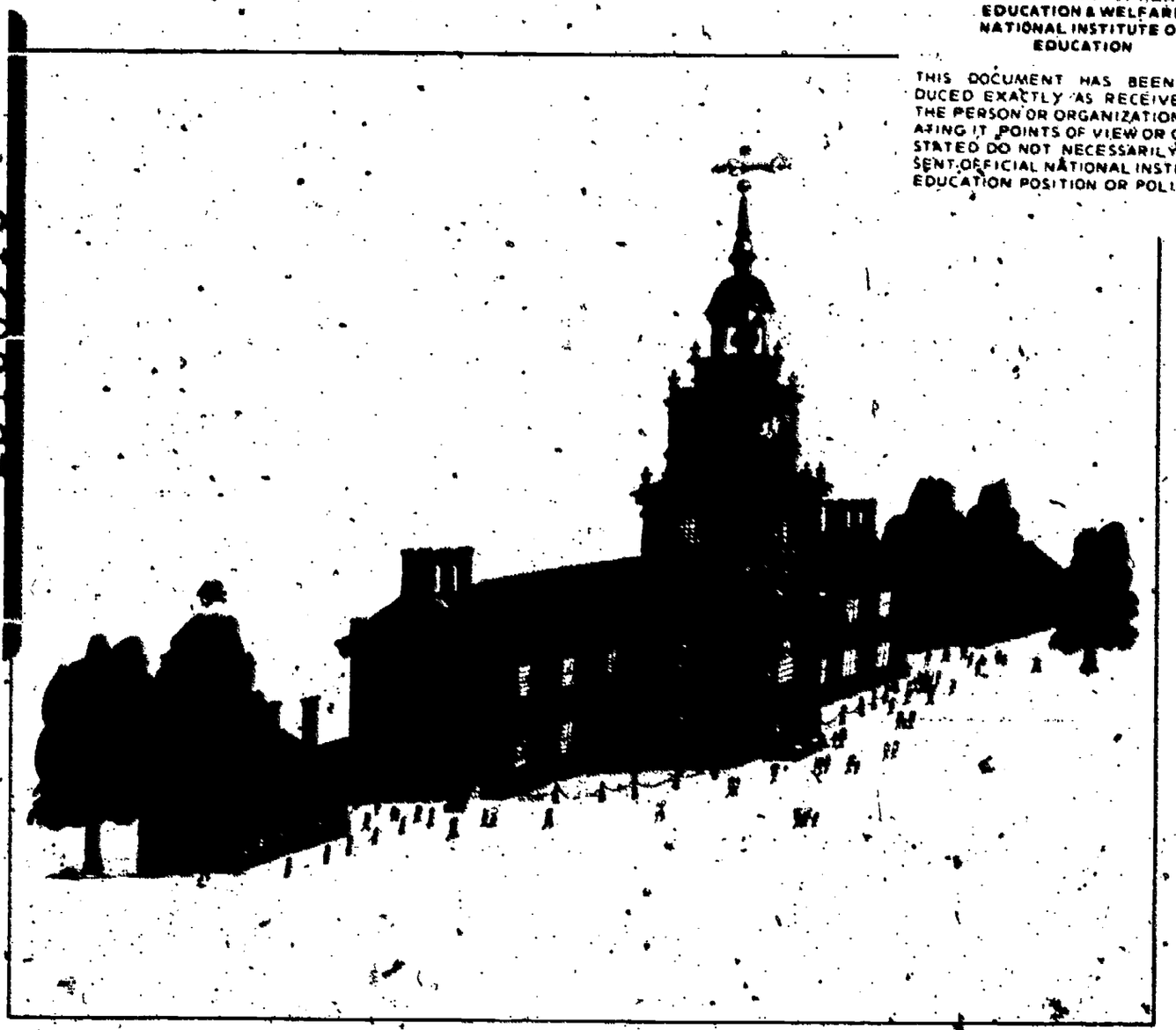
ABSTRACT

The Stanford-Berkeley Seminar on School Finance and Organization was convened to reexamine the problems that underlay the educational finance reform movement, to consider the new problems caused by that movement, and to recommend research topics and study areas that could help alleviate those problems in years to come. The eight papers discussed and revised during the five months of the seminar and presented in full in this document (1) assess the role of the courts in the development of the educational finance reform movement; (2) consider the relationship between equity and fiscal neutrality in the light of state actions; (3) argue that the business model for understanding school organization is inadequate and its application hazardous; (4) point out the dangers in assuming larger schools and districts are more efficient; (5) examine the implications of changing fertility rates; (6) build on the literature in economics of time, time budget analysis, and child pyschology to explore how a child's use of time outside school affects academic performance; (7) reject the human capital model for understanding the relationship between schooling and work; and (8) urge adoption of "recurrent education" as a technique for keeping the society economically and educationally vital. (Author/PGD)

Education Finance and Organization Research Perspectives for the Future

Program on Educational Policy and Organization



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Preface

This collection of essays on the financing and organization of American education is the product of a joint Berkeley-Stanford Seminar supported by the National Institute of Education. The seminar was designed to capitalize on the wealth of talent and experience resident in those two institutions, and to encourage collaboration and cooperation between them. We believed that a joint venture, focusing attention on the interconnection between finance and organization, would significantly advance our thinking about these important issues. The timing of this seminar, and the collection of essays that grew out of it, are particularly important, for the enterprise of public education itself is at a crucial juncture point.

It is now apparent that the radical critiques of the sixties were but a preview of the larger public concerns of the seventies. Those concerns, while still fragmented and uneven, are directed at the whole of the educational enterprise: its purposes, its goals, its structure, and the financial and organizational arrangements that undergird and define it. Declining public school enrollments, increasing private school enrollments, debate over court-ordered bussing, declining test scores, voucher initiatives, tax and expenditure limitation movements are but part of a long and controversal list of problems. Each has a significant financial and organizational component.

Our support for the Berkeley-Stanford Seminar was based on our belief that the school finance reform movement is reaching a plateau, that school financing and organization are intimately bound together, and that it is time to reflect on the near-term future of these two important issues. Historically, the movement toward school finance equalization was the major education reform of the sixties and early seventies. Its necessary emphasis on questions of taxpayer equity through judicial proceedings, however, had begun to obscure rather than reveal other important equity issues and other non-judicial concerns and side effects. School finance research and implementation strategies had become, as Burke said about the law, sharp by virtue of being narrow.

In short, we were convinced that it was time to enlarge the scope of school finance research to include questions of school organization. Schools are, after all, financed for a purpose, and that purpose is achieved through organizational arrangements. To separate the one from the other, while analytically appropriate, provided only a one-dimensional picture of the structure of education.

These essays, then, present an emerging two-dimensional picture dealing with both the financing and organization of education. The missing third dimen-

sion, of course, is the purpose of education itself; unfortunately, that important subject is beyond the purview of these essays. But it is clearly the next question to be asked by students of educational finance and organization, and it must be interwoven into the dialogue in the near future.

It is our hope that this book will stimulate and encourage that dialogue, for it is in the free give and take of ideas that the educational enterprise is most at home. If these essays help to focus that dialogue and force important issues to the surface, they will contribute to the solution of the pressing educational problems of the seventies and eighties. In that case, the Berkeley-Stanford seminar and this volume, its offspring, will have demonstrated their utility and value.

David Mandel
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Chapter 1

Overview

Charles Benson and Louise Stoll

Finance and organizational policies in American education are in a tumultuous state. Called to task from all sides for not meeting what often appear to be contradictory standards of equity and choice, schools function in the narrow spaces permitted by policies imposed by local school boards, state legislatures and departments of education and, to an increasing degree, the federal government. The great swells of the "state of the nation", such as a changing economy and shifting job market, the rise in teacher militancy, matters of social conscience which cry for redistribution of resources, are all experienced by school districts before the issues are recognized and defined as trends and new problems. The lag is critical, for bound by the policies born of a different perception of the facts, education languishes, and schools and school districts apply bandaid techniques that often compound the difficulties of long range solution. When policies are altered in response to new situations, they often reflect only partial understanding of the issues and hence either worsen the scene or bring on a new set of serious problems, for there is little systematic information gathering or research on educational policy with respect to finance and organizational questions.

The education finance reform movement, for example, undeniably directed toward benefitting students with respect to equal educational opportunity, has proceeded in the absence of a clear definition of objectives and has paid only casual attention to unintended consequences. This has resulted in diluted returns for the intended beneficiaries, a situation which has finally aroused the attention of educational policy planners. This volume, which represents the efforts of the Stanford-Berkeley Seminar on School Finance and Organization, constitutes a modest but important first step to deal more thoroughly with issues of educational resource allocation.

In early 1976, officers of the National Institute of Education held a series of informal meetings with members of the Stanford and Berkeley faculties on the desirability of a joint seminar on educational finance and organization. There was a feeling among all parties that, notwithstanding the dilution of results noted above, considerable attention had been drawn to problems of finance and organization during the course of the "educational finance reform movement." Actually, the discussion between NIE and Berkeley-Stanford could be seen as a logical progression from a series of meetings held around the country from 1972 onwards and sponsored by such organizations as Ford Foundation, Education Commission of the States, etc. Further, those who were pondering the future of the proposed seminar shared a feeling that the content of ideas in public policy toward finance and organization of education had reached a kind of plateau. This is not to say that there had not been technical and analytical advances, but it was believed that the seminar might help to stimulate thoughts to guide future improvements in analytical capacity.

The first requirement for gathering data that would assist educational policymakers to cope effectively with the rapid changes in society was to identify the areas for needed research. The theme of the seminar thus became "emerging educational policy issues for the next decade," with emphasis on research directions for financial and organizational matters. A list of tentative topics included equality issues in school finance, "optimal size" issues of administration and decision making units, demography, child care and preschool, out of school environment of young people, labor markets and social mobility, recurrent education and educational technology. Scholars recognized for work in these areas were solicited to write papers on facets of the topics which they considered important for future research emphasis. The authors were expected to develop themes of interest to themselves within the parameters of the seminar, but specifically to answer the following three questions:

- What is the problem being addressed by the paper, and why is it important?
- If existing policies in education prevail, what is the likely development in 5-10 years' time of the problem(s) chosen for investigation?
- If the decision were yours, what specific research topics or studies would you recommend to deal with the problem(s) and future policy agendas?

Thus, a common format focused on predictive insight was developed for presentation of the issues. The content and emphasis of the papers varied widely, however, and the style of presentation ranged from discursive to tightly argued paragraphs. While the "placing" of the problem naturally required an understanding of present educational policy, a comprehensive review of the literature was not requested of the authors for two reasons: first, the readers of these papers are presumed to be knowledgeable with respect to educational policy and, therefore, familiar with the literature and related research; second, the papers would have become too long for a single volume, and been diverted from the creative mission of the seminar which was to develop *emerging* research issues for the next decade. To the extent that a critique of related research was deemed necessary to clarify

analysis or proposed research agendas, the authors provided it. The final selection of topics depended on the interests of seminar participants. For example, no researcher had sufficient interest in pursuing educational technology, though this is deemed an important area for future work.

The Stanford-Berkeley Seminar followed a straight forward format. Beginning in November 1976 and ending in March 1977, the Seminar convened nine times. The meetings were held in the Stanford Board Room in downtown San Francisco, midway between the Stanford and UC Berkeley campuses. In addition to the authors of the papers and the project staff, a select group of graduate students, professors and other persons who had particular expertise in matters of school finance and organization were invited. Average attendance was 15. Papers were distributed and "talked through" by the author. Discussion first clarified assumptions and arguments and then centered on research topics provoked by the paper. The discussion generally resulted in specific suggestions to the author for expansion or reemphasis of the paper. A record of each discussion was kept which constituted the basis for written suggestions for revision in some cases. Following each seminar, the paper was sent to the participants who had not attended and to a broader mailing list which included officials of NIE. Written critiques were solicited from those who had not attended the presentation to assist the authors with revisions. Final drafts of the papers were collected prior to August 1977 and, with approval of the Project staff, appear in full in this volume.

The seminar papers segregate themselves into two main categories:

- Those which deal with effecting change in school organization and resources already internalized to a substantial degree in the system.
- Those which deal with outside social forces that have been recognized and responded to in only slight measure by the educational system.

Out of the eight papers, a focus for the future is developed based on an understanding of present trends relating both to educational institutions and the broader social context.

The first set of papers addressing School Organization and Resources discuss four topics: law, finance, optimal size of educational units and theories of change.

"Law, Politics and Equal Educational Opportunity: The Limits of Judicial Involvement," by David Kirp assesses the role of the courts in the development of what has come to be called the "Educational Finance Reform Movement."

Kirp asserts that the decade of heavy court involvement in educational issues, beginning with Brown vs. Board of Education, can be traced to two important institutional shifts: procedural reforms which relaxed requirements for class action suits and a judicially construed expansion of the meaning of the equal protection clause. He argues that the history of school finance parallels that of other equal opportunity movements in that a three-stage process has occurred: first, after political reform failed (school boards and legislatures "did nothing"), the equality claim was given *constitutional definition* (Serrano vs. Priest, Hobson vs. Hansen, etc.). Second, a minimal, rather than egalitarian, version of the claim achieved judicial recognition (San Antonio vs. Rodriguez, Lau vs. Nichols, etc.). Third, federal and state legislation has transformed the distributive justice issues

into issues amenable to political resolution—new State finance legislation. He develops two related hypotheses to explain the current (post Rodriguez) situation of apparent minimal court involvement in school finance equity issues: (1) that judicial involvement has declined because many of the issues are extremely complex and essentially nonjusticeable or beyond the court's competency to deal with; and (2) that the role of the courts is best appreciated as a catalyst to political resolution of complicated policy dilemmas rather than as a source of definitive policy decisions.

The discussion of legal cases presented in the paper illuminates the impossibility of the courts creating operant language out of such terms as "adequate" or "thorough and efficient" as applied to school funding. The present state of research in the impact of educational and school finance policy and on alternative mechanisms for resolving disparities is an inadequate foundation on which the courts can rely—ambiguous as to what the terms mean and what the remedies ought to be. In addition, the courts are unwilling to decree trade-offs in which one group in the population benefits at the expense of another. The courts, Kirp argues, have—given the research available—gone about as far as they can, and, while possibly continuing a dialogue with state legislatures on the matter, are likely now to restrict their concern to gross disparities and broadly defined minima. This is not necessarily a loss, Kirp argues, because it has, indeed, spurred response in the political arena—the appropriate arena to tackle the trade-offs required by public policy—as witness over 30 states have thus far enacted school finance reform laws. It is this joint undertaking of the courts and the legislative branch, the courts declaring minimal constitutional guarantees and the law makers giving substance to these declarations, that best suits the resolution of questions of educational equity in this society. We are directed, then, not only to examine educational and finance policies, but also to explore what new lines of research might facilitate the interplay of judicial and legislative action in the future.

In this volume, one such direction is indicated in the paper by Lee Friedman and Michael Wiseman: "Toward Understanding the Equity Consequences of School Finance Reform." Grounded in research and analysis which will be familiar to those interested in school finance, the authors point out that (1) definitions of "equal opportunity" are diverse and sometimes inconsistent, and (2) little data exist regarding the effects of *recent* policy changes addressing unequal opportunity. They go on to argue that an important gap in current understanding is the scarcity of data concerning how *people and localities* act with respect to changes in school finance policies. Friedman and Wiseman suggest that the progress toward the goal of "fiscal neutrality"—the apparent preferred response of states to the demand for equity—be monitored closely, particularly with respect to unintended negative consequences such as possible increased residential clustering by income class.

The paper is concerned with expanding the understanding of the concepts frequently bandied about in the school finance reform movement, in order to refine the tools for monitoring the impact of fiscal legislation. They distinguish between 1) "simple neutrality," meaning, with respect to educational resources, absolute equality in dollar amounts, and 2) "conditional neutrality," a "fair pro-

cess in the distribution of wealth." The latter occurs when the distribution of inputs or outputs would be the same, all other things being equal. It is the thesis of this paper that the fundamental equal education issue is a "simple neutrality" issue—that is, poor children do not have the same educational inputs *or* outputs as rich children. In contrast, the California court, still the leading court in school finance, appears to accept a "conditional neutrality" solution. Quoting language from the Serrano II decision, they show the court's apparent concern with actual disparities in school expenditures caused by wealth and, at the same time, willingness to allow district power equalizing (DPE) as a solution. DPE is specifically intended to avoid a "simple neutrality" solution. The same tax rate would bring in the same dollars when applied; however, no one coerces a particular district to levy a tax rate sufficient to provide equal input, much less equal educational output. It would appear highly unlikely under DPE that the frequency distribution of school district spending would be identical when sorted by wealth or income class of residents.

Interestingly, however, in an examination of the impact of school finance reform in Illinois since 1973, Friedman and Wiseman find considerable progress toward "simple neutrality." They point out that the recent establishment in Illinois of a major Title I ESEA-like revenue distribution to districts counters this progress toward "simple neutrality," but it also counters the interest of the DPE reform legislation. All of this points up the complexity and conflicts inherent in the equity issue, the ease with which it is muddled, and the need for increased research with respect to the impact of reform legislation implementation. Once again, it is the problem of relating judicial intent to legislative action. Giving voice to the law requires clarity of ideas and a grasp of the practical implication of action; both need continued examination.

The examination of judicial-legislative interplay is of interest with respect to school finance, primarily as it impacts local school districts and the education provided in the schools themselves. The questions that must be addressed here are: (1) as a result of shifts in the law, what changes occur in the organizational and financial condition of school districts? (2) How does this affect the educational inputs and outputs for various categories of students? And, (3) how can it all be measured? Two seminar papers address these issues, one developing a methodology for understanding change in a school district, or school, and the other focusing on optimum size of educational units.

"Toward a Contingency Theory of Organizational Change in Education: Structure, Processes and Symbolism," by Terry Deal and Brooklyn Derr focuses on a methodology that is tailored to schools for both examining impact and implementing change. The paper rejects the "OD"—organizational development—movement which was transported largely unaltered from the business world to education in the sixties, as being adequate to effect school district institutional change.

Schools, Deal and Derr maintain, while behaving like other organizations in some respects, are extremely complex kinds of institutions that share a number of characteristics that make change difficult. They lack clear goals and developed technology, are inter-penetrated and controlled by their environment, operate

under a myth of "professionalism" which makes employees believe they are autonomous but have a civil servant mentality operating alongside. Thus, a three-pronged consideration emerges which, with respect to any change, demands examination of the *structure*, *processes*, and *myths* of the school, or school district. Viewed from this perspective, the impact of Serrano on the education opportunities of children in low wealth districts, or districts which will find themselves with less money, is *not* explained by the dollar differences at all, but is highly contingent upon how the district retains its structural patterns, technical processes and symbolic elements such as values and beliefs. Since school districts are "loosely coupled" or "organized anarchies," Deal and Derr maintain that there are few guidelines to follow to minimize deleterious effects or enhance the possibility of success. And it is in understanding the dynamic interactions of these dimensions that they urge a research agenda—for they believe that insensitivity to these internal needs and a lack of integration among the three aspects of the organization have contributed to the high failure rate of previous change efforts. In essence, they are saying, the best laid plans in school finance and organization reform may be doomed to failure if means of implementation that really work aren't developed and carried out, based upon the reality of how schools and school districts operate.

This cautious reminder concerning methodology for examining the impact of laws as well as implementing change finds no more ongoing concern than in the question of optimal size of schools and districts.

"Organizational Scale and School Success," by James Guthrie, tackles the issue of district and school size, initially from an historical perspective. The trend of the last 35 years is clear: while the number of students has increased since 1930 from 25 million to 50 million, the average school district size has gone from 200 to 3,000 and the average school from 100 to 550. Theories touting the economics of larger school units, legislation encouraging consolidation of school districts, and research claiming increased educational outputs from this have come under attack. In recent years, many studies, for instance, ignored important structural considerations of consolidation, such as transportation. With respect to achievement and integration, the research is ambiguous since the socioeconomic status (SES) level of students seems often not to have been taken into account.

Guthrie argues, that citizen participation seems adversely affected by large schools, and also that both citizens and school board members lose power when state control is increased, as is generally the case when school finance reform legislation is enacted. The consequences of this loss of lay control on education is not well understood beyond expressed feelings of citizen alienation. It likely will not be understood until more refined measurement techniques for cost-benefit analysis of schools are operational.

The trend toward school districts and schools of ever increasing size continues to be encouraged by legislation based on simplistic and possibly erroneous cost-benefit analysis. In addition, it appears to be a public policy which is in apparent conflict with an increasingly important public insistence upon additional (or renewed) community participation in schools, an insistence which also has acquired

legislative favor as more federal and state programs require parent advisory committees. This collision course of increased parent expectation of control and the increasing size and bureaucratization of the educational institution is far more than an ironic commentary on the apparent impossibility of managing public education in this country. It is superimposed on a structural shift of an enormous magnitude—the sharp decline in the birth rate of the country—which forced attention on both the use of facilities and the need for staff.

The importance of understanding the impact of declining enrollment of students on the financial and organization features of American public education dominates the current scene. While demographers were quick to note the sharp shift in fertility rates, schools took a while to adjust. The revelation came to school districts operating under a per/pupil revenue limit law that their student population wasn't raising sufficient revenue to pay the same staff, and it became obvious to all districts that the cost per student of running their schools had skyrocketed! The consequences for school district organization and governance are substantial: teacher lay-offs, which create conflict with teachers' unions over jobs and seniority provisions and threaten to leave school districts staffed only with senior people; school closings which cause enormous dissension in a community; the demands of governing boards to have increased power to "transfer staff" to "cover the ground," to list a few. Declining enrollment is now understood to be a nationwide phenomena, but an industry which has been for a century predicated on growth does not make the transition to stability or contraction overnight. Particularly since urban school districts which most need attention with respect to educational equity are most affected, the implications for finance reform and district/school organizations are important to understand.

By the same argument, the Seminar arrived at the decision to examine other aspects of the context within which schools operate. Prior school finance reform research has paid little attention to either the interrelatedness of school and home or changes in the world of work. A failure to examine assumptions in each of these areas, it is believed, could result in new policies being adopted that result in a series of adverse and unanticipated side effects. Thus, the following papers address the second major area for research: outside social forces influencing education.

The Seminar commissioned a paper by Harriet Fishlow, a demographer, and one by Charles Benson whose recent work has focused on early childhood nutrition and studies of how children of different SES backgrounds spend time outside of school. These two papers by no means exhaust considerations essential to understanding the "context" within which schools operate, but rather, are examples of the kinds of considerations which must be taken into account if sweeping reform in the technical areas of finance and school/district organization are to be effective.

In "Demography and Changing Enrollments," Harriet Fishlow, examines the fertility rates of the last decades, projects future fertility rates and suggests the implications of these for schools.

Fishlow points out that the number of births has fallen nearly every year in the last 18 years and that the percentage of decline over that period is nearly 28%. This has resulted, already, in a 10% decline in elementary school enrollment and will mean a further decline of 7% or 8% by the middle 1980s. Secondary enrollments reached their peak in 1976 and, over the next 15 years, can be expected to decline by 25%. After the mid 1980s, however, the children of mothers belonging to a large cohort—born before the decline in birth rate of the late 1960s—will enter school so enrollment will once more turn upward; however, enrollments are not likely to move all the way back up because the level of fertility is expected to stay comparatively low. Fishlow spends considerable effort to defend her chosen size of future completed families of 2.1, or what is known as "replacement" per woman. She bases this selection on: multiple considerations of career aspirations and rising labor force participation for women, manifestation of female individualism, the availability of contraception and legal abortion, environmental concerns, and the short run economic state. The babies born prior to this current period of declining fertility rate, between 1946 and 1964, create a "bulge" in the age structure of the nation which will have implications for the job market as well as the delivery of social services for the next decades. It is, then, not decline or growth, per se, but the cyclical fluctuations in elementary and secondary schools that the Fishlow paper causes us to consider. It points to needed research to develop flexibility with respect to facilities, retraining of surplus teachers, and creation of a "pool" of certificated people to move back in as enrollment rises again without an agonizing period of overcrowding and powerful teacher wage demands, etc.

Insofar as the Fishlow paper sharpens an understanding of the "magnitude of the clientele" schools serve and are likely to serve, it urges institutional developments that will minimize the buffeting to education from swells or declines of population. The question of whether the social benefits of education *can* be independent of demographic considerations—or any other significant social phenomena—is, of course, not answerable with certainty.

The paper by Charles Benson, "Time and How It Is Spent," argues for a continuing examination of one of the most stable findings of social science research: that the school performance of children is related to their home background, and, indeed, that variation in family background accounts far more for variation in school achievement than do variations in school characteristics. Benson notes that whatever else the term "home background" means, it certainly includes consideration of family characteristics and structure—number of children and ordering, whether one or two parents is in the home, whether or not there is a non-working parent, income and education level of the parents, and attitudes on important issues relating to child rearing. Everyone agrees that what happens outside of school affects what happens inside of school, and vice versa. Schools should, ideally, develop the human capital built up by the family.

But it is not well understood how human capital is built up in the family. Various studies indicate a significant relationship between time investment in children by parents and the children's performance in school, so time spent with children is considered "good." But results seem to vary with respect to the

"quality" of that time. Time, it seems, may be "poor time," just as salary may be "poor income," and each, presumably, has an adverse effect on children's development. In addition, it appears that children in different socio-economic classes have differing amounts of contact with non-parent adults, and that these "neighborhood effects" might play a role in understanding student achievement in school. The trade-offs are not well understood when quality, parent attitudes, neighborhood contacts, education level, and income level qualify the "time spent with children." The goal is to bring the best possible equipped child to the school. The challenge is to understand the critical components of "home background" with an eye, possibly, toward developing socially acceptable interventions that would enhance school performance. Examples of the unanswered questions pertinent to school finance reform considerations are: can "good" child rearing practices in low SES families, including good diet and sleeping practices, help with homework, control of TV watching, etc., enable their children to compete successfully in school with upper SES families who pay no attention to these practices or even do the opposite? Or are the best efforts of poor families to improve the life chances of their children eradicated by neighborhood effects or family structure effects? Is the quality of family investment so different that all the "good practices" of the poor fall before the unmeasurable components of the rich? How does much time from an uneducated mother compare with little time from a highly educated mother with respect to children's school achievements?

Benson's paper suggests it may be possible to disentangle the effects of parental characteristics, family structure and neighborhood by analyzing, in the first instance, the use of children's and parents' time. This proposal builds on the growing literature in three fields: economics of time, time budget analysis, and child psychology. It also builds on research into intra-familial distribution of time.

Two papers in the seminar direct research to the connection between work and education. It has long been accepted that schools prepare people for work and that this is their primary function. The equity problem has been to assure that the opportunity and the rewards of schooling fall equally on all classes of people. The human capital model, which assumes that the market works efficiently and that individuals respond rationally to rates of return to schooling, has been the model generally used to explain the school-work connection. Norton Grubb, in "Schooling and Work: The Changing Context of Education," rejects the human capital theory as providing an adequate model for understanding this relationship and explores three topics which could serve as the foundation of a more adequate model: over-education in America, credentializing, and career education. He argues that the long-held myth that more education means better jobs should be laid to rest in view of the large college age cohort of the 1960s which could not find jobs with salary and status commensurate with their education. The demographic and technological causes of this, he argues, are not unexpected in a capitalist society where benign neglect rules public policy with respect to the connection between work and schools. This approach ultimately challenges the school finance reform movement and all other educational equity movements by suggesting that the possibility of success will remain elusive as long as the "system is what it is."

Grubb's development of the credentialing theory addresses other disturbing phenomena: the degradation of jobs, the setting of unnecessarily tough requirements for employment in the presence of an abundance of workers, the use of education as a non-rational sorting device rather than a place to acquire needed job skills. The proliferation of career education programs grounded in assertions about "over-education" and "credentialing," speaks primarily to reform in school curriculum and content.

It is the view of those arguing for career education that the present mode of schooling has become irrelevant to adult/work life. Grubb believes that if career education cannot obtain the support of business and labor, it will remain merely a reform internal to the education system and have no impact on the way schools interact with labor markets. He develops a research agenda which departs from hypotheses centering around the human capital model in its attempts to understand the variety of contradictory roles schools have come to play. The common myths have said one thing about the relationship; reality insists that something different is happening. Lurking in the paper are the unassailable assumptions that schools, in what they teach and how they do it, "support" the present capitalist structure of the country and that, as an agent for change, schools are likely to fail.

The final paper in the volume, "Recurrent Education and Employment" by David Stern also examines the apparent phenomena of "over-education," but with the goal of utilizing it for development of a more equitable society rather than preventing it. The key for Stern is "recurrent education" which he defines as "any activity in which someone intends to learn something but which is not undertaken as part of an unbroken sequence of schooling, from compulsory schooling to post secondary, to final degree." This includes urban power programs, night schools, veterans programs, institutes for managers and union members, etc. Stern sees recurrent education as a means of reducing rates of unemployment and providing more jobs to workers of all educational backgrounds. It would avoid bottlenecks in certain occupational categories as well as get workers interested in their jobs by permitting mid-career retooling and thus raising production, both with likely anti-inflationary effects. Stern's research agenda is directed toward discovering how recurrent education can increase both motivation and productivity.

Stern appears to reject Grubb's thesis that the phenomena of over-education renders inadequate the human capital model to explain the relationship of work and school. Rather, he views "over-education" as the consequence of the "baby boom" and as causing major but predictable dislocations in the labor market. He believes that recurrent education provides a practical solution to dealing with both the sheer numbers of workers who will be in mid-career in the years 1980-2010 and the likelihood that their skills will have deteriorated.

Stern's paper departs from the papers already discussed in providing an intriguing and original means for accomplishing what he advocates: a plan for financing recurrent education by permitting individuals to borrow against their Social Security or pensions, thereby creating no new public fiscal liabilities. The spillover benefits to the public from a program directed essentially at education is a healthier economy in ways already described as well as alleviation, in part,

of the potentially horrendous problem of intergenerational distribution as the large baby boom cohort begins to retire and demand Social Security benefits from the payrolls of the smaller cohort of their children and grandchildren.

The papers in this volume, then, address a varied and provocative list of issues pertinent to school finance and organizational reform. The reader is urged not to be bound intellectually by the order of presentation which has been chosen. The subjects are inter-related and the papers presented in a format focussed on the common concern for developing emerging research proposals in school finance and organization for the coming decades. The implications for policy and research raised by any one paper in the volume may well illuminate many of the other topics considered. The collection should not be assessed as an end in itself, but as a spur to considerations and research important to the development of an equitable educational system in this country.

Chapter 2

Law, Politics, and Equal Educational Opportunity: The Limits of Judicial Reform

David L. Kirp

"By itself, the court is almost powerless to affect the court of national policy."[1]

"[T]he Supreme Court of the United States is really the schoolmaster of the Republic and if it cannot command, it can at least educate the American people about what they need to do to improve the educational systems of the country."[2]

Introduction: The Rise (and Fall?) of the Constitutional Revolution in Education

With respect to questions of educational policy, courts have historically been the "least dangerous"—and the least involved—branch of government. Policy was traditionally framed by legislatures and school boards, largely unconstrained by the command of the constitution. The *Brown*[3] decision significantly altered this allocation of authority, but only where issues of race were concerned; defining "equal educational opportunity" in other spheres remained essentially the province of politicians and educators.

The experience of the past decade has been very different: to ignore the possibility of judicial intervention in educational policy calculations risks subsequent review and upset of those calculations. The judges have thrust themselves into the vortex of debates over the procedural and substantive rights and liberties of students and teachers: decisions concerning, for example, students' and teachers' rights of free speech and peaceful protest, and the procedural protections available to students and teachers whom school districts wish to discipline or dismiss, have had noteworthy effects on the management of school affairs.[4] Of even greater moment, courts have assessed claims based on the assertedly inequitable treatment of particular classes of students: those residing in "poor" school dis-

tricts[5] or attending minimally funded schools within a particular district,[6] the handicapped,[7] the non-English speaking,[8] and women.[9] In each instance, normative arguments rooted in understandings of distributive justice have been rendered into issues cognizable under the constitution.

This development may be traced to several interrelated sources, both inside and outside the judicial system. Within the judiciary, various procedural reforms pertaining to litigation, most notably the relaxation of requirements for a "class action" lawsuit—a case whose result affects not only the individual litigant but also all others who share the litigants' grievance—made possible the bringing of cases whose impact would be widely felt; class justice, not just individual justice, could now be obtained.[10] Paralleling this procedural reform was a judicially-crafted expansion of the substantive meaning of the equal protection clause.[11] That metaphor of equality—"No State shall . . . deny to any person within its jurisdiction the equal protection of the laws"—had once been derided by Justice Holmes as the last refuge of constitutional argument. No longer: during the Warren Court era, poll taxes, fees for transcripts and appellate attorneys in criminal cases, electoral apportionment, and welfare residency rules were assessed in light of the equal protection guarantee. In each instance, because the court could identify a constitutionally fundamental right in jeopardy, the state-created differentiations were reviewed to determine their "necessity," not merely their "reasonableness"; none of these differentiations survived scrutiny under this new standard. The Supreme Court also became increasingly attentive to classifications which disadvantaged the poor, a group which—like blacks—could plausibly claim that differentiations adversely affecting them were presumptively invidious. The landmark equal protection cases of the time fused *both* kinds of concerns: the state's classifications trenched upon constitutionally vital interests and employed wealth as the basis for distinction-making.

This new equal protection had apparent relevance to equal educational opportunity questions. If inequities concerning voting rights and criminal procedure offend the equal protection clause because of the constitutional importance of those rights, should not inequities with respect to education, which could plausibly be described as the developmental prerequisite to the informed exercise of other constitutional rights, similarly offend? If classifications which disadvantage "discrete and insular minorities"—those premised on race, and, perhaps, poverty—are suspect, wasn't the plight of children residing in property-poor school districts constitutionally salient? And didn't classifications grounded on linguistic ability, mental or emotional handicap, and sex, present analytically similar problems?

The equal educational opportunity issues themselves—fairness with respect to the distribution of resources within states and school districts, and with respect to the treatment of "minority" groups—were hardly new. Scholars had devoted considerable (if varying) attention to them; nor had they gone undiscussed in school boards and legislatures, the forums which customarily make allocative determinations. Yet neither scholarly analyses nor political debates had much altered the *status quo*. The lawyers' contribution to the policy debate was the articulation of ancient grievances before a newly receptive forum, the courts;

in that forum, principled elaboration of argument in constitutionally-recognizable terms counted for more than the familiar determinants of political success. More bluntly, recourse to the courts marked an end-run around institutions politically unresponsive to equity-based grievances.

In each instance, some judicial success was marked:[12] the *Serrano* interdistrict and the *Hobson* intradistrict resource allocation cases; decisions in Pennsylvania and Washington, D.C., giving constitutional credence to the right of handicapped children to an "appropriate" education; *Lau*, which found in the 1964 Civil Rights Act statutory warrant for requiring that attention be paid to the educational needs of the non-English speaking; and lower court opinions which assured women access to particular school offerings—notably competitive athletic programs and vocational classes—and protected them against exclusion on the ground of pregnancy.

These were famous victories which, taken together, produced a heady sense of the possibilities inherent in judicially-ordered reform. To be sure, the Supreme Court's holding in *Rodriguez*, that the equal protection clause is not offended by state school finance systems which preserve wealth-based inequities among school districts, did represent a significant setback to this movement. Not only did it weaken the drive for restructuring state finance laws, but it also dashed the notion that education was of sufficient constitutional importance to warrant preferred treatment. The lawyers and their allies, however, saw the setback as only temporary, the larger thrust toward judicial redefinition of equal educational opportunity essentially unimpaired.

Whether the relationship between court-created change and educational policy-making is viewed favorably—the courts perceived as "heroes," as one educator put it—or thought to embody the unprincipled usurpation of political and professional authority without constitutional license,[13] the influence of courts on the policy process is now conceded. It has become the new conventional wisdom. Yet in areas of inquiry as dynamic as education, new conventional wisdoms become dated even as they supplant the old; that may be the case with regard to prevailing assessments of the judicial role. The post-*Rodriguez* world has witnessed no notable judicial boldnesses. The Supreme Court has not addressed constitutionally-framed questions of equal opportunity[14] and the pace of judicially-mandated reform in the lower federal and state courts has noticeably slackened. This little-noticed reversion to something more like the older judicial restraint may be attributable to the immediate after-effects of *Rodriguez* itself; it is, after all, only four years since that decision was handed down. But, as subsequent sections of the essay suggest in greater detail, deeper explanations concerning both the judiciary's competence to resolve these equal educational opportunity questions (beyond setting constitutional minima) and the relationshp between courts and the avowedly political branches of government, hold greater intellectual interest and more evident plausibility.

Many of the equal educational opportunity questions now being presented for judicial resolution strain the capacity of the courts. In some instances, what the litigants are seeking has no operant meaning: the efforts to define a "thorough and efficient system" that have plagued New Jersey's school finance litigation

serves as an apt illustration.[15] In other cases, as with suits questioning the adequacy of particular educational programs provided for handicapped youngsters, the empirical data needed to inform the adjudicatory process do not exist; consequently, the mixed fact-law questions which lie at the heart of those disputes cannot confidently be resolved.[16] In yet other situations, the demand is that courts order what is presently impossible of attainment, as with the litigation seeking to hold school districts accountable in damages for the achievement failures of students.[17] In each of these circumstances, equity-based arguments have appropriately limited appeal for the courts, particularly in the context of cautiousness established by *Rodriguez*.

Matters of legal doctrine and reconsiderations of judicial role are not the only reasons for the recent demise of judicial activism. With respect to each of the educational equity issues other than school finance, the aggrieved groups have obtained through legislation much of (in some instances, more than) what they sought through litigation. The resource demands of women, the non-English speaking, and the handicapped have been embodied in new state and federal laws; and, with respect to these last two groups, moneys which no court could order expended have been made available to meet their particular needs. Even in the school finance domain, where comparable federal legislation has not been forthcoming, the 1970s witnessed the passage of statutes in some eighteen states which reduce resource disparities among school districts; while the federal government has not underwritten these equalization efforts, it has made available money to plan for them. Intradistrict resource inequalities have lessened considerably as a consequence of the federal statutory requirement that non-federal expenditures in schools receiving compensatory education funds be "comparable" to expenditures in other schools in a given district.

By itself, judicial activism did not "cause" this new legislative responsiveness. Other factors—among them, the availability of state budgetary surpluses in the early 1970s (or, as with the federal sex discrimination legislation, the feasibility of low cost legislative action), the reformist inclination of the legislature, the role of pressure group suasion and liberal political leadership—have all had significant influence.[18] Yet the relationship between judicial and legislative reform efforts is not coincidental. The primary effect of judicial involvement in the equal educational opportunity realm may well be found not in court-defined resolution of these questions, but more nebulously in the judicial impetus for an essentially political solution, with courts affording new legitimacy to particular equity-based concerns.[19] This dynamic interaction does not cease with legislative assumption of responsibility. Statutes are often sufficiently general in their terms to require judicial interpretation; in that sense, court assessment of equity claims is not stayed, but rather placed on a new and more limited footing. More speculatively, if the promise of reform inherent in the current legislative initiatives goes unrealized, aggrieved parties may be encouraged yet again to render these equity questions into constitutional form.

It is these two related hypotheses that I wish to explore: First, *constitutionally-based educational equity claims which demand more than a minimal entitlement are typically non-justiciable, and for that reason have not been honored by*

the courts. Second, *judicial minima-setting may best be appreciated not as defining policy but rather as contributing to a political resolution of the underlying policy dilemma that equal educational opportunity claims invariably present; that contribution represents one stage in a continuing interchange between legislatures and courts.* The essay focuses less on doctrinal development than on institutional dynamics within the court and between the political and judicial branches. Because the school finance issue offers the richest, most textured history, it is treated separately in Part II. Part III asserts a pattern to the judicial-political interplay with respect to educational equity questions generally, and considers areas of legal and policy inquiry other than school finance in developing the argument. The conclusion sets this assessment in a somewhat broader context, sketching a revisionist understanding of the courts' role with respect to problems of distributive justice.

School Finance: The Constitutional Near-Revolution

The California Supreme Court's 1971 decision in *Serrano v. Priest*,[20] striking down that state's system of financing public education on the grounds that it discriminated against those living in property-poor districts and hence violated the equal protection clauses of both the United States and California constitutions, represented the high-water mark for efforts to secure through the judiciary equal educational opportunity with respect to financing education. In the wake of that decision, similar suits were brought in thirty-one states; the federal and state courts that confronted the question immediately thereafter liberally borrowed both *Serrano*'s legal analysis and constitutional conclusions.[21]

In 1977, such excitement as can still be attributed to this question has been generated by a second *Serrano* decision, this one decided exclusively on state constitutional grounds. Elsewhere, the issue of court-generated finance reform is essentially moot. The broader hopes for a judicially-worked school finance revolution of national scope were demolished by the Supreme Court's *Rodriquez*[22] decision, and while suits premised on equal protection and specific education provisions in state constitutions have subsequently been filed,[23] the judicial response has been at best mixed. But even as the courts' involvement in this issue has diminished, legislatures in eighteen states, most of them states where no authoritative judicial decision was rendered, have revised their financing system in ways which reduce the impact of local wealth disparities. The anticipated revolution has been overtaken by the politically commonplace task of securing incremental change. The tale of this transformation bears telling.

The aspirations of those who initially litigated the school finance issue were avowedly egalitarian: equal protection demanded equal treatment of students. That objective could be achieved either by distributing educational resources according to need or on a head count (one scholar, one dollar) basis.[24] This challenge to the near-universal practice of relying primarily on property taxes raised by school districts of widely differing wealth to support public schools was

rebuffed by the courts in the late 1960s. Judges lack the capacity to make "needs" determinations, the court in *McInnis v. Shapiro*[25] concluded: "[T]here are no 'discoverable and manageable standards' by which a court can determine when the Constitution is satisfied and when it is violated." The proposed alternative—distributing resources on the basis of absolute dollar equality—was thought by the judges to insure only a deadening sameness. *McInnis* viewed these particular shortcomings of plaintiffs' argument as indicative of a broader truth: school finance presents a political, not a constitutional question. "[T]he allocation of public revenues is a basic policy decision more appropriately handled by a legislature than a court." The state's reliance on locally-raised property taxes as the chief source of school financing was characterized as a means of allowing "local choice and experimentation." That decision was affirmed without opinion by the United States Supreme Court.

The genius of the *Serrano* approach,[26] as contrasted with that taken in *McInnis*, was that it conceded the *McInnis* court's conclusion that school finance questions are generally political, while carving out a limited and comprehensible role for the courts. The doctrinal roots of the two approaches were almost identical: both argued for the constitutionally-preferred status of education and both noted the adverse effects on those whom state finance schemes rendered poor. The crucial difference lay in the remedy proposed. *Serrano* did not speak to the particulars of resource distribution as *McInnis* had, but only declared impermissible one among many modes of financing schooling. The fiscal neutrality standard successfully urged in that case—the constitutionally-compelled severance of the nexus between local wealth and educational expenditure—left states free to select from a myriad of revenue-raising and distributing alternatives. The political system, not the courts, would weigh the competing claims of uniformity of distribution against the particular needs of urban districts, racial minorities, low achieving students and the like. Legislatures could also opt to preserve "local choice" with respect to levels of financing education, a value alluded to in *McInnis*; as long as school districts had equal or constructively equal tax bases, expenditure variation did not offend the constitutional standard.

Because fiscal neutrality reaches only one form of inequity, that attributable to taxable wealth, and because that particular form bears a most uncertain relationship to the desserts of children, Supreme Court adoption of this standard would not have resolved questions concerning children's resource equity claims.[27] At best, judicial action would have forcibly focused legislative energies on equity as one part of the reform task. Although the legal argument failed in *Rodriguez*, this *political* aspiration has to some extent been realized.

Rodriguez did not deny that inequity inhered in prevailing school finance systems. "Chaotic and unjust," Justice Potter Stewart said of the Texas statute—even in concurring with the majority that this unfairness was not of constitutional dimension. The Court rejected the constitutional predicates offered to justify adoption of the fiscal neutrality principle. No identifiable class of poor *persons* was found to suffer discrimination (*Serrano* had relied upon the discrimination explicitly worked by statute against property-poor districts); and education was held not to be a constitutionally fundamental interest. While *Rodriquez* did hint

that a state finance system which afforded students less than "an adequate minimum educational offering" would violate equal protection guarantees, the possibility for successfully fashioning such an argument was remote; the Texas statute upheld in *Rodriguez* preserved widely disparate funding levels among school districts. In deeming that fact constitutionally irrelevant and, differently, in reiterating the *McInnis* decision's concern that judicially-mandated financing reform would disturb the allocation of authority between local school districts and the state, *Rodriguez* intimates that school finance questions are essentially legislative in nature.

While *Rodriguez* left little room for further school finance litigation premised on the federal equal protection clause, there remained the possibility of renewing the challenge on state constitutional grounds.

It could be, and has been, asserted that finance systems which tied resource availability to local wealth violated both state equal protection clauses and education articles mandating, for instance, "a uniform system of free public schools" or "common schools, where all the children of the state may be educated."[28] The state supreme courts of Idaho, Washington, Montana and Oregon dismissed such arguments;[29] elsewhere, as in Connecticut, the issue remains pending.

In *Robinson v. Cahill*,[30] the New Jersey Supreme Court upheld a challenge to that state's finance system, holding that the existing mode of raising and distributing resources did not satisfy the state constitutional requirement of a "thorough and efficient system" of public education. But "victory" in that case should give law reformer advocates even more cause for concern than the *Rodriguez* defeat. Through seven separate decisions and innumerable concurrences and dissents, the task of specifying the meaning of the constitutional requirement has gone unachieved. "Thorough and efficient" remains as much a mystery as the *McInnis* "needs" standard—just what must the legislature do to assure "that educational opportunity which is needed in the contemporary setting in order to equip a child for his role as a citizen, and as a competitor in the labor markets"? Political and analytic efforts to solve this mystery provoked only conflicting policy stabs into the judically-created darkness and legislative deference to the judiciary's vagaries. In July 1976, the court took the extraordinary step of ordering the closing of New Jersey's schools until the lawmakers funded an "equalization act" which they had passed the previous session. That constitutional confrontation did produce legislative action acceptable to the judges; whether anyone else will be satisfied is less clear. Ironically enough, in light of the sweep of the original *Robinson* decision, the New Jersey equalization statute has only modest effects on existing wealth-based disparities; and the urban plaintiffs who brought the suit are worse off after the reform than before. If *Robinson* stands for anything, it is that broad questions of distributional equity cannot be resolved by judicial exegeses of such elusive phrases as "thorough and efficient."

Constitutional theory continues to be reworked, in hopes that a new approach, to a presumably more responsive judiciary, will unseat *Rodriguez*. The alleged irrationality of finance formulas that do not take into account the greater municipal services burdens and higher education costs of large cities is presently the subject of litigation in New York.[31] An equal protection-based argument for a

"negative inputs" measure of equal educational opportunity has also been advanced. "The concern is only with expenditure or input inequalities that lack rational justification, such as but not limited to wealth-created disparities." That approach, it is said, "preserves the dispute-settling function of the judiciary while at the same time being mindful of its limitations."[32] Although the present Supreme Court has shown no enthusiasm for a constitutional standard broader than that rejected in *Rodriguez*, perhaps the issue will again be ripe for presentation a decade from now. Perhaps "negative inputs" will carry the field where "fiscal neutrality" failed, or perhaps a constitutional argument as yet undreamt will ultimately receive judicial blessing. It is far too soon to know.

Attention presently focuses primarily on state legislatures, newly energetic with respect to educational policy generally, and, secondarily, on the federal government. Particularly during the early 1970s, school finance reform was an important state issue. In each of the fifty states, appointed study committees evaluated the existing system, and new legislation was introduced. Although analyses and politicking often yield no change, eighteen states did pass legislation producing incremental shifts away from existing wealth-based disparities and toward a more rational allocation of dollar resources.[33] Among the steps taken were increases in the level of state aid, property tax relief, and distributional formulas which took into account special needs of particular type of children and cost of living differentials.

The courts' involvement in these efforts has varied substantially from state to state. In some instances, the interplay between judicial press and political response has been overt. Judicial decisions in Minnesota[34] and Kansas[35] prior to *Rodriguez* adopted the *Serrano* approach and hence technically forced legislative reform. But in both states those decisions merely reinforced a preexisting political willingness to take the judicially-required action; if the suits were not technically collusive, they had considerable support from their nominal political adversaries.[36] Michigan's governor, a most unusual plaintiff, sought and obtained a state supreme court determination that that state's finance laws violated the equal protection clauses of the federal and state constitutions; a year later, after the Michigan legislature enacted a financing formula providing for a somewhat more equitable distribution than had previously existed, the court vacated its previous opinion and dismissed the case.[37] By doing so, the court made clear that its real purpose was to cajole some reform, not to insist upon a particular reform as constitutionally compelled.

Other states, among them New Mexico and Maine, required no judicial prod before acting. By 1970, school finance reform had come to seem politically plausible, not preposterous. Taxpayers in school districts forced by their low tax bases to set ever-higher tax rates were demanding relief, enabling politicians to convert school finance into a politically saleable issue; then-existing budgetary surpluses permitted states to increase their relative contribution to education without having to increase state taxes;[38] and reform seemed the right thing to do.

In addressing this issue, the state legislatures proceeded very differently from the judiciary: their action was differently grounded, and treated different factors as relevant to the decision. Where courts require principled, constitutionally-premised reasons to ground their decisions, legislators operate under no such con-

straints; legislative motivations are consequently more varied and pragmatic. The judiciary assessed the claim that financing inequities embodied a denial of equal educational opportunity as a "case," and hence did not make trade-offs among competing social policy goods; to have done otherwise would have been improper. By contrast, legislative support for finance reform was often secured through classic political bargaining. For example, urban and "rich" communities were each able to protect the interest of their own constituencies as their price for support: the "urban factor" in California and Florida, and the universal tendency in reform states to increase expenditures in poor districts rather than undoing existing advantages enjoyed by wealthy districts, illustrate the process. Endorsement of school financing change was also premised on action in other, unrelated policy areas: the governor of Kansas, for instance, insisted upon passage of a corporate income tax provision before signing the school finance law. The judiciary searched for narrow, coherent standards against which to define a constitutional right concerning resource equity; *Robinson v. Cahill* is the conspicuous exception. The legislatures took a quite different tack, perceiving school finance as one element of broader reform concerns, among them the identification and provision of support for children with special needs, the imposition of cost controls on spendthrift districts, and the introduction of accountability requirements.

Even where courts had no formal part in these efforts, the constitutional question has not been irrelevant. *Serrano* offered one persuasive definition of fair treatment; that definition embodied an aspiration to which appeal could be made, even if it did not rise to the level of formal obligation. In Oregon, for example, the leading state senate proponent of finance reform relied on *Serrano* as demonstrating the need for "every child to obtain a decent education without regard to the vagaries of government boundaries."[39] Federal legislation which reimburses states for the cost of developing or financing plans whose primary purpose is achievement "of equality of educational opportunity for all children" hints at the hard-to-measure impact on political thinking generated by *Serrano*,[40] at the least, that decision cemented the bond between finance and equity. Some day, the federal government may provide general aid to the states, with the effect of further reducing expenditure disparities and inequities in tax burdens. This idea too has its modern political genesis in *Serrano*, even if it is light-years away from anything that the most aggressive fiscal neutrality advocate could have imagined a court ordering.

These efforts need to be appraised with a certain skepticism. For one thing, some of the state reforms have had more to do with property tax relief than with educational equity, however that latter concept is defined; at least in the short run, the expenditure gap between the richest and poorest school districts actually increased in some states after "reform."[41] For another, reform efforts substantially diminished with the disappearance both of state budgetary surpluses and judicial pressure. Among eighteen states identified as having revised their school finance systems between 1971 and 1975, only five had acted since 1973, the year *Rodriguez* was decided.[42] Even in the states where some reform has already occurred, greater equalization is in certain instances dependent on further legislative action, and one cannot but wonder whether the willingness to appropriate

new money will survive the recent economic recession and the more skeptical stance that legislators increasingly adopt toward education. The anticipated 1977 increase in state tax revenues should permit an empirical test of the durability of the political commitment to finance reform and resource equity.

If the political reform movement dissipates, leaving substantial inequities in place, yet another effort to involve the judiciary in the resolution of the school finance question can be anticipated. The end of this interplay between courts and legislatures has yet to be written.

Beyond School Finance: The Pattern of Judicial-Legislative Response to Equal Educational Opportunity Issues

In no other realm has the array of judicial responses to equal opportunity questions been so broad, the debate over the proper constitutional function of the courts so pointed, as in the school finance domain. The central themes of this history do, however, have their analogues in efforts to give political and constitutional definition to equality of opportunity in other contexts. Whether equality is defined in terms of within-district resource distribution, or treatment of the handicapped, the non-English speaking, or women, a common pattern emerges. The equality claim is cast in constitutional terms after political reform efforts fail (or, in some instances, are not attempted); a minimalist version of the claim achieves judicial recognition; [illegible] as the chilling effects of *Rodriguez*, and more generally a judicial reluctance to move beyond minimalism in fact and value indeterminancy, constrain constitutional decisions; federal and/or state legislative action effects an essentially political resolution of the distributive justice issue, in which the courts' new task is interstitially to interpret broad equal educational opportunity statutes. No iron law with respect to the evolution of equal opportunity is proposed here. With respect to specific claims, certain stages of the process have been irrelevant; with respect to others, further constitutional attention may be anticipated. But because the pattern does not appear dominant, it is the organizing framework for this part of the essay.

The Constitutionalization of Equal Educational Opportunity Issues

Equality has no single meaning. At one moment, it may refer to identity of treatment among groups. At another, it may be held to require equal attention to some other value, such as choice (as with fiscal neutrality) or needs as variously defined; utilizing such an approach will produce outcomes which deviate from identity of treatment. The constitutional language, "equal protection of the laws," has sufficient flexibility to encompass these divergent understandings of equality.

Those claiming that extant resource distributions denied equality of opportunity on the basis of school assignment within a given district and on the basis of sex equated equality with identity of treatment. In both instances, recourse to the courts was undertaken without serious prior efforts to achieve the desired result through the political system.

Hobson v. Hansen[43] was the first case to test the constitutionality of intra-district differentiation. *Hobson* held that gross disparities between the resources provided to predominantly white and wealthy schools and those available in preponderantly poor and black schools within Washington, D.C., constituted a denial of equal protection. In several respects, this constitutional argument was more easily fashioned than the interdistrict disparity claim. Those hurt by the differentiation were not poor districts but poor and black schoolchildren; the system discriminated against a definable class of individuals. And the district, unlike the state in *Rodriguez*, could not argue that concern for programmatic diversity and local autonomy justified the differentiation. The tax burden, defined in terms of the property tax rates, was identical throughout the city; the actions of a single school board, not separate districts, produced the disparity. The court's standard —equalization of resources, apart from *de minimus* variations and differentiation caused by a focus on definable student needs—was both manageable and proper.

In most respects, schools do not distinguish on the basis of sex in allocating resources. Such distinctions do arise, however, with respect to particular areas of the curriculum (notably vocational programs), competitive athletics (from which girls have traditionally been barred), admission to academically selective high schools (with higher standards set for women than men), and the exclusion from school of pregnant women (that exclusion having the effect of denying to one class of women all access to schooling). The demonstrable inequality worked by exclusion has posed no acute constitutional dilemmas, but the courts have experienced considerable difficulty in defining equality in the other sex-specific contexts. With respect to athletics, for instance, while a complete lack of opportunity has uniformly been deemed constitutionally wrongful, the constitutionality of sex-separate but otherwise equal programs remains uncertain.[44] Similarly, while the imposition of a higher admissions standard for entrance to academically selective high schools on women than men has twice been overturned,[45] the permissibility of sex-separate secondary schools remains unsettled;[46] the defense of such schools, phrased in terms of choice, introduces an added element of unpredictable consequence into the constitutional calculus.

The handicapped and the non-English speaking, by contrast, sought to define equality with respect to the particular needs of those groups. In both cases, it was argued, identity of treatment only results in inequality in fact; the particular "needs" for which constitutional recognition was sought varied both between these groups and among sub-classes of the handicapped population.

The most obvious denial of equal educational opportunity is occasioned by the exclusion of an entire class of children from access to education. That those children are handicapped, and hence belong to a group that can make a legitimate claim for judicial recognition as a constitutionally suspect classification, renders the exclusion legally even more vulnerable. Certainly, the handicapped fit the *Rodriguez* definition of suspectness: "[a] class . . . saddled with such disabilities, or subjected to such a history of purposeful unequal treatment, or relegated to such a position of political powerlessness as to command extraordinary protection from the majoritarian political process." The consistent legislative rebuffs

encountered by those representing the interests of handicapped youngsters, and the shabby treatment they consequently received, gave further credence to the argument that they deserved the particular attention of the courts.

In *Pennsylvania Association for Retarded Children (PARC) v. Commonwealth of Pennsylvania*,[47] suit was brought challenging a Pennsylvania law which treated severely retarded students as "incapable of benefiting" from publicly-subsidized instruction. The consent decree in that case concluded that providing free education to "normal" children while depriving youngsters with mental handicaps of an equivalent right "established a colorable constitutional claim." All children, the court stated, are capable of benefiting from instruction—if only in the sense that they can be rendered relatively less dependent on others. Shortly thereafter, in *Mills v. Board of Education*,[48] a federal district court extended this "right to an education" to all handicapped children, not just the retarded.

PARC and *Mills* did for the seriously handicapped what *Serrano* achieved for those concerned with the interdistrict finance question. The judicial successes spawned substantial popular and scholarly attention, and similar lawsuits in more than thirty states.[49] But even as the pace of legal activity increased, the dilemmas of defining equality with respect to the handicapped began to emerge. It was easy enough to say that the seriously handicapped had some constitutionally cognizable entitlement. But was that entitlement merely to the education that public schools generally made available, or did it also encompass a substantive right to a minimally adequate education which was suited to the child's particular needs? A remedy cast in mere access terms would have no practical benefit. But a more tailored remedy posed the old *McInnis* issue: how might "needs" be rendered in judicially manageable form? The possibility that courts would have somehow to decide whether a particular service constituted education—as distinguished from mere care—and, differently, assess the financial implications of a needs-specific remedy, only confused the issue further. The summary dismissal of this latter question in *Mills*—"The [district's] interest in educating the excluded child clearly must outweigh its interest in preserving its financial resources"—was hardly an adequate response. Could the autistic child demand that $10,000, ten times the average per pupil cost, be expended annually on his education? *PARC* and *Mills* defined the right broadly to education in broad "needs" terms, identifying an entitlement to an "appropriate" or "suitable" education, but left the task of elaboration to court-appointed masters (in Pennsylvania) and to resolution in due process hearings to which both decrees entitled those dissatisfied by the fit between apparent need and the school offering.[50] "Needs" questions would be treated in the particular, not by general rule.

"Needs" definition with respect to the seriously handicapped was not the only equity puzzle raised by suits challenging treatment of the handicapped as inequitable. The seriously handicapped wanted something special; by contrast, some of those identified as mildly retarded saw differentiation as itself stigmatizing, and hence constitutionally proscribed, particularly where the designation was disproportionately applied to non-white and non-English speaking students. "Special" treatment, these groups asserted, was merely a euphemism for inferior education; what they wanted was indistinguishability from the "normal" school

population, insofar as this was feasible.[51] By contrast, those with other mild handicaps, notably the learnings disabled, objected to the schools' refusal to pay special attention to their needs. They saw regular class placement as inefficacious, and for that reason constitutionally offensive; borrowing from the *PARC* and *Mills* approaches, such assignment was described as effective or functional exclusion.[52] These disparate claims presented the real possibility that either the placement or the failure to place a particular type of student in a special education program might result in a constitutional grievance. The fact that the etiology of mild handicapping conditions is not well understood, and the absence of generally effective educational cures for these hard-to-identify ills, rendered the judicial task even harder.

Those who do not speak English offered much the same sort of constitutionally-premised equal education opportunity argument as the handicapped children who claim an entitlement to "appropriate" education: the non-English speaking child is effectively excluded from school when offered only an English language education. The measure of "equality" urged was need-specific; the failure to distinguish between the "linguistically handicapped" and the normal population was perceived as depriving the child of even that minimum level of education which *Rodriguez* dictum treated as a constitutional right. The non-English speaking, like the handicapped, also argued that their status as a linguistic minority victimized by the majoritarian political process provided another reason for the close scrutiny of allegedly disadvantegeous treatment that is the constitutional due of "suspect" classification.

One federal district court found these arguments persuasive.[53] "[I]t would be a deprivation of equal protection for a school district to effectuate a curriculum which is not tailored to the educational needs [here, for bilingual-bicultural education] of minority students." Making programmatic sense of that court-ordered requirement presented the same array of questions as did the needs-based claims of the handicapped. Clarity concerning what special treatment was constitutionally required, while obviously desirable, could not readily be achieved in the face of indeterminancy, both with respect to desired ends and plausible means. Was the constitutional aspiration to prepare students as quickly as possible to function in an English-language (and dominant culture) school world? Or did the non-English speaking have a constitutional right to instruction both in their native language and in English? Was minority culture, as well as language, something that the schools were constitutionally obliged to incorporate into the curriculum? The conflict among objectives was matched by the lack of the knowledge required to implement any one of them. Perhaps for these reasons, one circuit court held that linguistic-minority students are not "entitled under the Fourteenth Amendment to an educational experience tailored to their unique cultural and developmental needs."[54]

The Impact of Rodriguez and the "Needs" Conundrum

Rodriguez has influenced judicial assessment of equal opportunity claims beyond those explicitly resolved in the decision. Courts read *Rodriguez* as re-

quiring that they apply a less exacting standard of review with respect to educational opportunity questions. More broadly, the cautiousness that pervades *Rodriguez* has evoked a judicial skepticism with respect to equal educational opportunity arguments, particularly those in which the constitutional demand was for more than the judicial declaration of a minimal entitlement. Many of the post-*Rodriguez* decisions reveal a renewed respect for the shadowy, but no less real demarcation between constitutional problem solving and problem solving generally.

In *Brown v. Board of Education*,[55] for example, a suit patterned after *Hobson*, the district court declined, on the authority of *Rodriguez*, strictly to scrutinize resource disparities among Chicago's schools. Although a demonstration of racial discrimination *was* sufficient to evoke the more exacting judicial standard, the minimal—less than one percent—allocation differences which existed at the time the suit was brought were held to be non-discriminatory. A *PARC*-type suit, challenging on its face an Ohio statute which excluded from publicly-subsidized schooling those "incapable of profiting substantially by further instruction," was dismissed, the court offering a rather delphic reference to *Rodriguez* as justification for this decision;[56] were proof of actual harm introduced, the judge intimated that the outcome might well have been different. In New York, a federal district court also drew on *Rodriguez* for support in dismissing a suit challenging the adequacy of the educational program offered to institutionalized retardates.[57] "[I]f there is no constitutional infirmity in a system in which the state permits children of normal mental ability to receive a varying quality of education, a state is not constitutionally required to provide the mentally retarded with a certain level of special education." One court even relied on *Rodriguez* in upholding the exclusion from regular school of a pregnant student; the fact that the woman could finish her education in night school was viewed as distinguishing her case from one of *total* exclusion.[58] Following *Rodriguez*, these courts accepted minimal state provision, and did not insist on equality of treatment.

Rodriguez could be—and was—distinguished in several equal opportunity cases which upheld the rights of multiply-handicapped children and learning disabled children to particular educational programs. As one of these opinions noted, *Rodriguez* "left open the possibility that a denial of a minimally adequate educational opportunity may trench upon a fundamental interest . . ."[59] Several of the other post-*Rodriguez* decisions—the pregnancy exclusion case, most clearly—take a too-sweeping view of that holding. Yet even where *Rodriguez* has not been treated as a formal bar to judicially-ordered relief, some hesitancy to extend constitutional equal opportunity doctrine can be detected. This judicial modesty is perhaps most evident in *Lau v. Nichols*,[60] the one equal educational opportunity case (excluding desegregation cases) which the Supreme Court has decided since *Rodriguez*.

Lau was regarded by both parties as a constitutional test of the right of non-English speaking students to bilingual instruction; the district and circuit courts had so treated it. The Supreme Court, however, entirely avoided the constitutional question, resting its decision instead on the legally narrower ground of the 1964 Civil Rights Act. The Act states in pertinent part: "no person . . .

shall, on the ground of . . . national origin . . . be denied the benefit of . . . any program or activity receiving federal assistance."[61] This language is quite as vague as the equal protection clause; but by premissing its decision on the statute rather than the constitution, the Court essentially left the issue to political resolution. One concurring opinion treated the statutory claim as dependent on the size of the non-English speaking student population in a given district, an approach directly at odds with that concern for personal rights with underlies the Fourteenth Amendments.[62]

Lau's reliance on federal statute, not the equal protection clause, may have presaged a new mode of interplay between courts and legislatures. The courts have established constitutional minima concerning most of the equal opportunity claims. It is the province of the political system to give more detailed meaning to these minima and to secure the resources needed for their implementation.

From Constitutional Requirement to Political Mandate: The Evolution of Equal Education Opportunity

Even as the judiciary has edged away from detailed equal educational opportunity prescriptions, federal and state legislatures have within the past half-decade rendered into statutory law many of the equity-based claims pressed before the courts; in several cases, those laws go well beyond extant judicial mandates. In the near term, courts are likely to be most active, in reviewing allegations of statutory violation, not developing new constitutional doctrine.

Intradistrict resource disparities, for example, are now largely prohibited by Title I, Elementary and Secondary Education Act, which provides substantial federal funding for the education of the "educationally disadvantaged." As a condition of eligibility for funds under that Act, Congress required that school districts render state and local expenditures in Title I (predominantly poor) and non-Title I schools "comparable."[63] This requirement was imposed not to satisfy *Hobson*-type concerns for fairness, but rather to assure that the federal moneys were in fact additive, not merely equalizing. And comparability calculation under Title I differs from the *Hobson* nondiscrimination requirement: the constitutional but not the statutory standard treats teacher-seniority-based salary differentials as a resource to be included in the equalization calculation. It is the comparability requirement that provided statutory support for, and assured the widespread impact of the concern for within-district equity which underlies *Hobson*. The statute, not *Hobson*, has given practical meaning to this definition of equal educational opportunity.

The special education litigation witnessed an interaction between the judiciary and the courts similar to that of the post-*Serrano* finance cases. Colorado and California *PARC*-type suits and challenges to state failures to address the needs of autistic children, were each dismissed when the defendant state voluntarily undertook to do *something* about the problem. Even *PARC* itself was welcomed by state officials. As one scholarly observer noted: "[T]he federal court did not resolve a dispute between contesting parties, but instead ratified

an agreement between advocates for children's services and professional service agencies to raid state treasuries for greater funds on behalf of their shared clientele."[64]

In retrospect, the generally successful litigation on behalf of the handicapped seems less impressive for what it directly achieved than for the state and federal legislation which it provoked. By the simplest measure of effect, money, the impact has been remarkable: state expenditures for handicapped children have more than doubled in just three years, climbing from $900 million in 1972 to an estimated $2.03 billion in 1974;[65] and the new Education of All Handicapped Children Act[66] authorized the federal government to spend $2.243 billion by 1979, as much as is presently expended on any federal education program.[67]

Not only dollars, but also the values identified by the courts as constitutionally paramount, are reflected in this new legislation. The Education for All Handicapped Children Act notes that federal assistance is needed "in order to insure equal protection of the law." Its provisions mirror the court decisions in assuring due process protection against arbitrary placement of handicapped students; attending to the varied needs of handicapped youngsters while insuring that, to the greater extent possible, handicapped children are not isolated from their normal peers; and scrutinizing the tests and other screening devices used in identifying handicaps to guard against a racial or cultural bias. In its particulars, the bill goes beyond anything that a court might have ordered, including mandated thrice-yearly individual reviews and educational plans, required in-service training, and a ten percent ceiling on the numbers of students labelled as handicapped (designed to keep money-conscious districts from excessively labelling youngsters as in need of special help). The aspirations of the litigants have more than been realized by this law.

Not that the legislation "solves" the educational dilemmas, among them dissensus concerning both the proportion of the population appropriately identifiable as having specific handicaps[68] and the educational regime best suited to respond to such handicaps. The new federal resources, coupled with an elaborate federal enforcement machinery to prevent at least gross abuses of the act's purposes, and a judiciary empowered to review alleged statutory violations, together promise more salutory effects than any court could achieve by constitutional decree.

The preemption of constitutional questions by legislative action has been even more fully accomplished with respect to the non-English speaking men and women. In the 1974 Equal Educational Opportunity Act, Congress specifically approved the *Lau* reading of nondiscrimination.[69] Failure by an educational agency to take "appropriate action to overcome language barriers that impede equal participation by its students in its instructional programs" is now an explicitly forbidden denial of equal educational opportunity.[70]

Similarly, Title IX of the 1972 Education Amendments and HEW regulations adopted pursuant to that statute now resolve almost all of the sex discrimination questions earlier treated in constitutional terms.[71] The general language of Title IX parallels Congressional treatment of racial discrimination: "No person . . . shall, on the basis of sex, be excluded from participation in, be denied the bene-

fits of, or be subjected to discrimination under any educational program or activity receiving federal assistance." In its specifics, Title IX does not demand absolute sameness of treatment, and it leaves for constitutional resolution the permissibility of sex-separate academic high schools. Concerning issues likely to arise more frequently—such as access to vocational programs, the right to "equal athletic opportunity": (neither a guarantee of equal resources for men's and women's sports nor a bar to sex-separate athletics but an attempt to secure the widest possible athletic participation by both sexes), and exclusion on grounds of pregnancy—Title IX, rather than the constitution, should be the authoritative legal standard.

The courts' impact in these areas has been mixed. While *Lau* doubtless spurred passage of the 1974 legislation, the efforts of the women's movement, not court decision, explains the adoption of Title IX. In both instances, a political response was made easier because Congress merely required others to do something, rather than funding new programs itself. The effect of these actions is to force lower levels of government, particularly school districts, to reshape their budgets in ways that accommodate the new understandings of distributive justice with respect to sex and the non-English speaking. The courts may influence this process, but they will do so by interpreting the legislative mandate, not by making new constitutional law.

Some Concluding Observations

During the 1960s, the equal protection clause was widely seen as a primary instrument for attaining distributive justice, and the courts viewed as able through reliance on that constitutional provision to bring about a fairer social order. No longer is that expectation tenable. Beyond the declaration of minimum entitlements, at least, courts have been inclined to stay their hand, relying on the political process for a resolution of distributive justice question with respect to particular public goods, such as education. As the Supreme Court observed, in a case involving welfare benefits:[72] "In the area of economics and social welfare, a State does not violate the Equal Protection Clause merely because the classifications made by its laws are imperfect. The problems of government are practical ones and may justify, if they do not require, rough accommodations—illogical, it may be, and unscientific." Even concerning disadvantage worked against "suspect" groups (racial minorities, women, and the like), the traditional source of deep equal protection concern, the judiciary has grown more reluctant to intervene as the element of invidious discrimination historically evident in those cases has become ever-harder to identify; the apparent implication is that relief from these asserted wrongs would have to come from legislative action, not constitutional determination.

These decisions have evoked considerable unhappiness for doctrinal reasons. And rightly so: it has become increasingly difficult to detect the thread of principle running through equal protection law and hence harder to justify *any* court decision, whether rights-expanding or rights-contracting in its effect. Those

used to special judicial protection see these decisions as marking a retreat or, worse, a betrayal; they do upset familiar expectations. Yet as a pragmatic matter, this new judicial modesty has something to commend it. Gross discrimination, whether it involves access to constitutionally-protected rights or disadvantaging of "minorities," raises intuitively constitutional questions. But with few exceptions, gross discrimination no longer survives in statutory law. Nuanced questions of distributive justice do persist, but these cannot be satisfactorily decided solely by reference to the constitution. Typically, they involve trade-offs among goods, choices concerning allocations among nominally equally deserving claimants, and for that reason may be better fit for political than judicial resolution. Under our constitutional system, they are probably best fit for resolution jointly and continuously undertaken, the courts initially defining minimal constitutional guarantees,[73] the lawmakers giving substance to these declarations, the judiciary subsequently clarifying statutory ambiguities in the light of constitutional principles. The evolution of the concept of equal educational opportunity may some day be appreciated in just these terms.

Notes for Further Research

Hypotheses, not data, are what this essay treats. The extant literature pays little attention to the interrelationships between the courts and the judiciary in the development of equal opportunity policy. In most of the specific domains, such institutional analyses are almost non-existent; and even with respect to school finance, where there is some discussion of the policy-making process, the most common treatment of the courts' role consists of merely noting their existence. More is called for, not only in making sense of the past but also in anticipating the future. Put differently, demonstration or refutation of arguments advanced here is the most substantial item on the research agenda.

Several other discrete matters also deserve more scrutiny than they have received. If, as I suggest, the coming interplay will involve judicial interpretation of legislative mandate, it would be useful to mine other areas where a similar role has been carved out (for instance, with respect to the 1964 Civil Rights Act) in order to anticipate, among other things, judicial reactions to legislative vaguenesses and judicial acceptance of interpretative regulations. A more narrow doctrinal point—the authority of the legislature, under the Fourteenth Amendment, to define rights and remedies in terms narrower than have been ordered by courts under the equal protection clause—also deserves examination. This issue most commonly arises with Congress's periodic attempts to limit the scope of busing remedies, but the differences between the *Hobson* and Title I standards for within-district comparability, and possible distinctions between a constitutional and statutorily-based right to bilingual education, suggest its broader relevance.

At a different level, the implementation of legislation either imposing new requirements (with respect to sex discrimination, bilingual education, and within-district resource comparability) or tying new money to particular new

requirements (federal aid to the handicapped and certain states' school finance reforms) should be closely followed. The new requirements work substantial changes on extant institutional cultures; even where, as with federal aid to the handicapped, they are accompanied by additional funds, Washington's expectations may go unfulfilled. How, for instance, can school districts with a finite number of programs for the handicapped (and even less sense of how appropriately to educate many of them) produce meaningful individualized contracts, as mandated by federal statute? And where, as with Title IX, districts are obliged to reallocate their own resources, will prevailing patterns be disloged by federal insistence? And what forms with that insistence take; will HEW actually cut off funds to districts, or will some less drastic, more effective sanctioning system be devised?

The equal educational opportunity claims derive from myriad perceived (and actual) disadvantages. In political terms, these claims compete with one another; there is just not enough money or institutional energy to attend to them all fully and simultaneously. The decline in state school finance reform efforts and the considerable difference between funds authorized and funds expended in federal educational programs illustrate the money problem; widespread reports of "discrimination," in one form or another, suggest the institutional dilemmas. One suspects that the resolution of these competitions will depend as much upon the effectiveness of pressure on decision-makers as on the intuitive merits of the claim; empirical assessment would further sharpen that hunch.

Most of these research suggestions do not address doctrinal concerns, the usual province of lawyers. That omission is deliberate: as the essay argues, bold new constitutional law initiatives are not anticipated. Some such questions do, however, deserve attention, among them the constitutional permissibility of ignoring "municipal overburden" in calculating state school aid (the *Levittown* case question); the constitutional plausibility of a claim for more state aid brought on behalf of children forced, by community disapproval of tax levies, to attend school for foreshortened years; and the reconciliation of *McInnis*-type "needs" concerns with remedy-making in constitutional cases involving the handicapped and non-English speaking.

Of greater moment, there also remain vital questions concerning the courts' ability to cope with the problems concerning judicial role which equal educational opportunity cases raise. How, for instance, can the trial judge be aided in interpreting complicated data, presented typically in adversarial fashion (a question specifically addressed by *Hobson*)? Of what benefit is the appointment of a master, both to aid in the fact-finding process and to supervise a remedial regime? How have other court devices—the appointment of a "review panel," the imposition of due process hearing requirements, and the like—helped in achieving the judicial objective? Concerning school finance specifically, a comparison of the courts' role in *Serrano* and *Robinson* would be most instructive.

With respect to at least some of these questions, decent theory has been generated. What is needed now, most of all, is careful case studies, chosen with an eye to their generalizability; one chief focus of those case studies should be

the local school district, the setting in which equal educational opportunity is likeliest to be given decisive, particularistic meaning.

References

1. Dowle, "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," 6 *J. of Pub. Law* 279, 294 (1958).

2. Kurland, "Equal Educational Opportunity or the Limits of Constitutional Jurisprudence Undefined," *The Quality of Inequality* 47, 65 (C. Daly, ed., 1968).

3. Brown v. Board of Education, 347 U.S. 483 (1954).

4. Concerning students *see, e.g.,* Tinker v. Des Moines Indep. Community School Dist., 393 U.S. 503 (1969); Goss v. Lopez, 419 U.S. 567 (1975); Wisconsin v. Yoder, 406 U.S. 205 (1972). Concerning teachers, *see, e.g.,* Pickering v. Board of Education, 391 U.S. 563 (1968); Board of Regents v. Roth, 408 U.S. 564 (1972).

5. *See, e.g.,* Serrano v. Priest, 487 P.2d 1241 (1971); *but see* San Antonio Independent School District v. Rodriguez, 411 U.S. 1 (1973).

6. *See, e.g.,* Hobson v. Hansen, 327 F. Supp. 824 (D.D.C. 1971).

7. *See, e.g.,* Pennsylvania Association for Retarded Children (PARC) v. Commonwealth of Pennsylvania, 343 F. Supp. 279 (E.D.Pa. 1972) (consent decree); Mills v. Board of Educ., 348 F. Supp. 866 (D.C. 1972).

8. *See, e.g.,* Lau v. Nichols, 414 U.S. 563 (1974).

9. *See, e.g.,* Brenden v. Indep. School Dist., 742, 477 F. 2d 1292 (8th Cir. 1972); Berkelman v. San Francisco Unified School Dist., 501 F.2d 1264 (9th Cir. 1974); *but see* Vorcheimer v. School Dist. of Philadelphia, 532 F. 2d 880 (3d Cir. 1976).

10. See Chayes, "The Role of the Judge in Public Law Litigation," 89 *Harv. L. Rev.* 1281 (1976).

11. For a fuller treatment of this development, see Kirp, "Judicial Policy-Making: Inequitable Public School Financing and the *Serrano* Case (1971)," *Policy and Politics in America* 82 (A. Sindler ed., 1973).

12. See notes 5-9, *supra.* These cases are discussed in greater detail in sections II and III, *infra.*

13. Compare Blatt, "The Legal Rights of the Mentally Retarded," 23 *Syr. L. Rev.* 991, 992 (1972) with Glazer, "Towards an Imperial Judiciary?," *Int.* 104 (1976).

14. See text at note 62, *infra.*

15. Robinson v. Cahill, 303 A.2d 272 (1973), 306 A.2d 65 (1973), 335 A.2d 6 (1975), 351 A.2d 713 (1975), 355 A.2d 129 (1976), 358 A.2d 457 (1976), 360 A.2d 400 (1976).

16. *See, e.g.,* Fialkowski v. Shapp, 405 F. Supp. 946 (E.D.Pa. 1975); New York State Association for Retarded Children v. Rockefeller, 357 F. Supp. 752 (E.D.N.Y. 1973).

17. Peter Doe v. San Francisco Unified School Dist., Civ. No. 36851 (Cal. Ct. App., Aug. 6, 1976).

18. See Berke, Shalala, & Williams, "Two Roads to School Finance Reform," *Society* (Jul/Aug 1975).

19. More pragmatically, legislatures may have acted in order to stave off judicial intervention. Legislative initiative permits the introduction of legitimate political concerns (trade-offs and coordination among items on the agenda, for instance) that have no relevance in judicially-resolved disputes.

20. 487 P.2d 1241 (Calif. Sup. Ct. 1971).

21. See generally School Finance Project, Lawyers' Committee for Civil Rights Under Law, *A Summary of State-Wide School Finance Cases* (1974).

22. 411 U.S. 1 (1973).

23. See generally School Finance Project, Lawyers' Committee for Civil Rights Under Law, *Update on State-Wide School Finance Cases* (1976).

24. See A. Wise, *Rich Schools, Poor Schools* (1968).

25. 293 F. Supp. 327 (N.D.Ill. 1968), *aff'd sub nom*, McInnis v. Ogilvie, 394 U.S. 322 (1969).

26. A "genius" almost wholly attributable to J. Coons, W. Clune, and S. Sugarman, *Private Wealth and Public Education* (1970).

27. See Kirp & Yudof, "Review," 6 *Harv. Civ. Rights-Civ. Lib. L. Rev.* 219 (1971).

28. For a compendium of these provisions, see E. Bolmeier, *The School in the Legal Structure* 89-97 (1973 ed.).

29. State ex re. Woodahl v. Straub, 520 P.2d 776 (Montana Sup. Ct. 1974); Northshore School Dist. v. Kinnear, 530 P.2d 178 (Washington Sup. Ct. 1974); Thomas v. Engelking, 537 P.2d 635 (Idaho Sup. Ct. 1975); Olsen v. Oregon, P.2d (Oregon Sup. Ct. 1976).

30. 303 A.2d 273 (1973).

31. Board of Educ. of Levittown v. Nyquist, No. 8208/74 (Sup. Ct., Nassau Cty, N.Y.).

32. McDermott, "The Cost-Quality Debate in School Finance Litigation," *Indeterminancy in Educaion* 169, 181-84 (J. McDermott ed., 1976); *compare* Kirp Yudof, supra Note 26, at 625: "Does there exist any rational reason to distribute educational resources on the basis of criteria unrelated to the child or his family?"

33. National Conference of State Legislators, *School Finance Reform: A Legislators' Handbook* 8 (J. Callahan and W. Wilkins eds., 1976). As the text subsequently notes, the term "reform" is generously employed by the handbook.

34. Van Duzartz v. Hatfield, 334 F. Supp. 870 (D.Minn. 1971).

35. Caldwell v. Kansas, Civ. No. 50616 (Dist. Ct., Aug. 30, 1972).

36. See J. Berke, M. Kirst, and M. Usdan, *The New Era of State Education Politics* (forthcoming, 1977); Morley, "Minnesota," *A Legislator's Guide to School Finance* 33 (Lawyer's Committee for Civil Rights Under Law, ed., 1972).

37. Milliken v. Green, 203 N.W.2d 457 (Sup. Ct. Mich. 1972), *vacated* 212 N.W.2d 711 (Sup. Ct. Mich. 1973).

38. *See* Berke, Shalala and Williams, *supra* Note 18.

39. J. Berke, M. Kirst, and M. Usdan, *supra* Note 37.

40. Education Amendments of 1974, 20 U.S.C.A. § 246 (Supp. 1975).

41. National Conference of State Legislatures, *supra* note 34, at 5,6. In New Mexico, for example, the disparity between minimum and maximum tax rates increased from $2.78/$1000 assessed valuation to $10.04/$1000 assessed valuation; the tax burden on all districts at or above the median wealth was, however, equalized. In Illinois, the range of expenditures per pupil jumped from $1325 ($721-$2046) to $1681 ($762-$2443).

42. *Id.* at 8.

43. 327 F. Supp. 821 (D.D.C. 1971).

44. *Compare* Brenden v. Independent School Dist. 742, 477 F.2d 1292 (8th Cir. 1973) *with* Bucha v. Illinois High School Ass'n., 341 F. Supp. 258 (N.D.Ill. 1972).

45. Berkelman v. San Francisco Unified School Dist., 501 F.2d 1264 (9th Cir. 1974); Bray v. Lee, 337 F. Supp. 934 (E.D.Mass. 1972).

46. Vorcheimer v. School Dist. of Philadelphia, 532 F.2d 880 (3d Cir. 1976). Compare Williams v. McNair, 316 F. Supp. 134 (D.S.C. 1970), *aff'd mem.*, 401 U.S. 951 (1971).

47. 343 F. Supp. 279 (E.D.Pa. 1972) (consent decree).

48. 348 F. Supp. 866 (D.D.C. 1972).

49. For a discussion of the impact of *PARC* and *Mills*, *see* Kirp, Buss & Kuriloff, "Legal Reform of Special Education: Empirical Studies and Procedural Proposals," 62 *Calif. L. Rev.* 40 (1974).

50. In Lebanks v. Spears, 60 F.R.D. 135 (E.D.La. 1973) (consent decree), the court approved an agreement calling for individualized "educational plans" for each handicapped student, to be developed by the school district and approved by the student's parents; the dissatisfied parent could request review in a due process hearing.

51. *See, e.g.*, Larry P. v. Riles, 343 F. Supp. 1306 (N.D.Cal. 1972); Diana v. State Board of Educ., Civil No. C-70-37-RFR (N.D.Cal. June 18, 1973) (stipulated settlement).

52. *See, e.g.*, Frederick L. v. Thomas, 408 F. Supp. 832 (E.D.Pa. 1976).

53. Serna v. Portales Municipal Schools, 351 F. Supp. 1279 (D.N.M. 1972) *aff'd on other grounds*, 499 F.2d 1147 (10th Cir. 1974).

54. Keyes v. School Dist. No. 1, 521 F.2d 465, 482 (10th Cir. 1975).

55. 386 F. Supp. 110 (N.D.Ill. 1974).

56. Cuyahoga County Association for Retarded Children & Adults v. Essex, 411 F. Supp. 46 (N.D.Ohio 1976).

57. New York State Association for Retarded Children, Inc. v. Rockefeller, 357 F. Supp. 752 (E.D.N.Y. 1973).

58. Houston v. Prosser, 361 F. Supp. 295 (N.D.Ga. 1973).

59. Frederick L. v. Thomas, 408 F. Supp. 832, 835 (E.D.Pa. 1976). *See also* Fialkowski v. Shapp, 405 F. Supp. 946 (E.D.Pa. 1975).

60. 411 U.S. 563 (1974).

61. 42 U.S.C. § 2000d (1970).

62. "For me, numbers [of non-English speaking students] are at the heart of this case and my concurrence is to be understood accordingly," 414 U.S. at 572 (Blackmun, J. concurring).

63. The HEW regulations, 45 C.F.R. § 116.26 (1975), give specificity to the comparability requirement.

64. Quoted in Kirp, Buss & Kuriloff, *supra* note 50 at 61.

65. W. Wilken & D. Porter, *State Aid for Special Education: Who Benefits?* 1 (1976).

66. 20 U.S.C.A. § 1405 et seq. (Supp. 1976), amending 20 U.S.C.A. § 1401 et seq. (1974).

67. 2 *Congressional and Administration News* 1425, 1462 (94th Cong. 1st Session 1975).

68. Prevalence estimates with respect to particular handicapping conditions vary enormously: the proportion of learning disabled, for example, varies between 1.12 percent and twenty-six percent. Even with respect to apparently more detectable handicaps, such as hearing impairments, the range is wide: from .060 percent to 2.1 percent, depending upon the study. *See* Wilken & Porter, *supra* note 66, Table I-5.

69. 20 U.S.C. § 1703 (f) (Supp. 4 1974).

70. The judiciary will continue to hear cases which test the statutory meaning of "appropriate action to overcome language barriers. . . . " *See, e.g.*, Morales v. Shannon, 516 F.2d 411 (5th Cir. 1975).

71. 20 U.S.C. § 1681 et seq. (Supp. 1974). See 45 C.F.R. § 86, 1 et seq. (1975). See also Comment, 124 *U. Pa. L. Rev.* 806 (1976).

72. Dandridge v. Williams, 397 U.S. 471 (1970).

73. See generally Michelman, "The Supreme Court, 1968 Term—Foreword: On Protecting the Poor through the Fourteenth Amendment," 83 *Harv. L. Rev.* 7 (1969). Professor Michelman's discussion of constitutional minima remains the most intellectually arresting theory of equal protection. Michelman stresses doctrinal and normative justifications for such an approach, rather than arguments premised on an allocation of decisional authority premised on institutional legitimacy and competence.

Chapter 3

Toward Understanding the Equity Consequences of School Finance Reform

Lee S. Friedman and Michael Wiseman

Introduction

Over the past decade, there have been major changes across the nation in school financing methods. Many of these reforms were stimulated, if not mandated, by court challenges based on equal protection arguments. Of course, there have been other reasons for undertaking reforms, like general property tax relief. But no matter how complex the tangle of reasons for enacting reforms, there can be little doubt that they affect the level and distribution of educational opportunities. We believe it is important to learn just how school finance reforms affect the equality of educational opportunity available to children.

It is, of course, much easier to ask the questions than to provide the answers. Putting aside empirical difficulties for the moment, there are numerous diverse concepts of equality to consider. For example, one might ask about the equality of (a) the educational inputs to children; (b) the educational effects of those inputs; or (c) the post-graduation life success of those educated. Each of these represents a distinct and important concept of equality worthy of exploration, and the list could be extended.[1]

In this paper, we will focus on only a very limited number of these diverse concepts. Furthermore, we will select some quite precise empirical measures of the concepts we discuss. We do so with some trepidation, for it is not our intention to suggest that other concepts and measures are less interesting. Rather, we believe that there are generally useful theoretical and empirical questions which can and should be applied to different reforms and bodies of data, and we hope that our illustrative efforts will be of some use to those pursuing this task.

There are three general themes of our work. First, more conceptual work would be useful to clarify diverse concepts of equal opportunity and to recognize relationships (particularly inconsistencies) among them. We try to illustrate this

with a discussion of "fiscal" or "wealth" neutrality, a concept of equality of current policy concern. In this discussion, we point out that two plausible definitions of it are inconsistent with each other, though both seem to be required by the *Serrano* court. The thrust of this discussion is to suggest that the construction of a wealth-neutral financing system may be harder than originally thought.

Our second general theme is that very little is known about how equality of educational opportunity, under any definition, has changed in response to school finance reforms. Using Illinois as a case study, we choose a few specific equality concepts and construct some empirical measures of the changes in equality. One unexpected lesson from conducting this exercise is that there are practical reasons, likely to arise elsewhere, why the desired tests cannot be made precisely as designed. Thus while the measures were designed to be strictly comparable with studies that could be conducted elsewhere, practically this may be difficult.

Our third general theme is that there should be more predictive theoretical modeling and empirical testing of hypotheses about the behavioral responses of firms and families to school finance reforms. These responses are important for two distinct reasons: (1) they may represent significant unintended consequences of the reforms; and (2) the effectiveness of finance reforms depends on the behavioral responses. In the first category, we are concerned about changes in the proportion of children enrolled in private schools, and changes in property values, as examples. To the extent that reforms alter the cost to districts of purchasing education, and to the extent that they alter the voter composition and tax bases of districts (through the changes mentioned above), they will further alter the level and distribution of public school expenditures, as well as the use of outside educational resources (e.g., parental time, use of libraries) as supplements to public school instruction. Thus, these responses are important determinants of the effectiveness of any reform. We provide two examples of these effects. One is an empirical example, suggesting that private school enrollments may decline where public school expenditures rise rapidly, but may rise where public school expenditure increases are limited. The other is a very rudimentary theoretical model which, when developed, may suggest that in a shift from purely local financing to district power equalizing, the long-run tendency for families of similar wealth to live in the same districts may be increased; furthermore that the equality in school spending may decrease.

The paper is organized as follows. First, we attempt to select several quite specific concepts of equal educational opportunity for theoretical and empirical examination. The concepts selected are those that seem to us to be of most current policy interest, judging from some recent court decisions as well as recent legislative enactments. On the theoretical level, we try to show that these concepts are not all consistent with one another, and eventually trade-offs among these equality goals must be made. Second, having chosen some specific equity concepts, we present a case study of school finance reform in Illinois to see empirically how much progress toward these goals has been made. Third, we try to illustrate that the consequences of school finance reform policies cannot be understood without accounting for a variety of behavioral responses. In this section we review expectations about changes in residential patterns as well as in the mix of public and

private education. Finally, a concluding section will present a summary and our overall assessment of research needs in this area.

Measurable Concepts of Equal Educational Opportunity

In the United States, vast resources are devoted directly and indirectly to the provision of education through a complex set of social institutions. While many of these resources are allocated to schools,[2] colleges, and universities, other educational resources are utilized through tutoring or private individual instruction, through parental, peer, and neighbor time, through museums, libraries, and community centers, through training institutes and through institutions designed primarily for other purposes (e.g., the military, private employers). While we generally associate education with youth, opportunities to be educated have increased both for adults (e.g., through night schools, continuing education programs, and changes in college and university admission standards) and for "pre-school" children.

Because of the importance of the role of education in our society, both to individuals and to the collective social interest, there is substantial concern about the equity of its distribution. Of course, what is equitable and what is not is fundamentally a value judgment, not a matter of science. However, science provides the basis for informed value judgments with both logical and empirical analysis of equity issues. Logical analysis is concerned with understanding the implications of imposing various equity rules or requirements on an educational system which can be described by the formal and behavioral rules which govern it. For example, it may be logically impossible to satisfy two particular notions of equity simultaneously, though independently each may seem desirable.[3] Empirical analysis attempts to establish the degree of correspondence (or lack thereof) between possible equity standards and the reality of educational opportunities.

We would like to illustrate, in the balance of this paper, the types of logical and empirical analysis which we believe important for understanding the equity consequences of school finance reform. To do so, however, means making some hard choices from among all the equity concepts, educational institutions, and population groups that could be used for our illustrative purposes. We would like to stress that the general techniques of analysis are entended to be applicable to the much broader set of educational activities mentioned above. Our examples will focus primarily on children attending public elementary and secondary schools, and the equity questions on matters of expenditures per child. This choice is convenient in terms of data availability as well as current policy interest, though it has the disadvantage of ignoring other important issues such as correcting for resource cost differences across geographic areas.[4]

We begin by suggesting several general concepts of equity which we find particularly useful in trying to understand the issues raised by the *Serrano* case as well as other types of school finance reforms.[5] We then try to specify the *Serrano* court requirements, and show that there seem to be two quite different requirements which are logically inconsistent in a school financing system allowing local

choice.[6] The section concludes with a brief review of other recent developments concerning school finance and equal opportunity.

Specific Illustrative Concepts of Equal Educational Opportunity

In trying to specify concepts of equal opportunity useful for understanding school finance reforms, it is important to distinguish two different types of questions: (a) whether the *division* of "shares" (e.g., expenditures per child) is equitable,[7] and (b) whether the *distribution process* is equitable.

Concepts of equity of division are defined independently of who receives the shares, and thus are easier to investigate. One equal opportunity concept of this type that we will use is:

(A1) All shares should be equal.

Another example of this type, which is of potential interest in school finance litigation,[8] is:

(A2) All shares should be at least a certain minimum size.

For both of these examples, it is possible to construct empirical measures of the degree of equal opportunity; that is, how close or far reality is from the ideal.[9]

Equity concepts of the distribution process are more subtle, and have been the most common basis for legal complaint alleging harm to specific groups. They apply whenever it is possible for students to receive different shares. The question is whether the differences arise from an equitable process. One specific concept of this type which we call *distributional equality* is:

(B1) Any potential recipient of a share should have the same chance for each share as any other potential recipient.

We will come back to the definition of "potential recipient" shortly, but for the moment assume it is well-defined. Note that if A1 is true, B1 is true; however, the reverse does not hold. It is quite possible to have inequality in division but perfectly equal opportunity in distribution. For example, suppose the schools in a district are identical with the exception that one has an elaborate scientific laboratory. Suppose there is open enrollment in the district, and all students wish to attend the well-equipped school (which would exceed its capacity). If the selection from among them is made by a fair lottery, then there is perfectly equal opportunity in the distribution process. Some will gain entrance and some will not, but all students had equal opportunity in terms of this concept.

In practice, one can establish the presence or absence of distributional equality only in a limited sense. To illustrate the problem, suppose in the above example that the lottery was rigged. Unknown to everyone except the lottery operator, certain children were favored and others were discriminated against. Could this be discovered by looking at the results? Unfortunately, the answer is only perhaps. If it is noticed that one identifiable *group* of children is not getting its fair share of admittances (e.g., children of one race, or one sex, or who are currently attending one particular school in the district), then suspicions may be aroused. Otherwise, however, the rigging may go unnoticed.[10]

From the standpoint of a statistician, the procedures used to define groups for testing distributional equality need only be unbiased with respect to the hypothesis. Distributional equality could be tested, for example, by comparing school expenditures per child for children with two vowels in their last names to expenditures per child for children with only one. Indeed, an infinite number of potential classifications exist. As a result, for a statistician actually to examine distributional equality, some judgment must be imposed concerning the grouping or groupings upon which the test is to be based.

Those classifications which the law, public sentiment, or researcher inclination cause to be selected define what we shall call "suspect" groups or classes.[11] In actual practice the selection of groupings to be termed "suspect" for this purpose reflects both past experience that such groups have been rigged against and social sentiment that such rigging is particularly invidious. In fact, a requirement for *neutrality* with respect to these groups can be a substitute for the stronger requirement of distributional equality. One such concept is *simple neutrality:*

(B2) Potential recipients in a suspect group should have the same chance for each share as do all other potential recipients.

Simple neutrality is evaluated by comparing the actual shares within the suspect group or groups to the actual shares for all other recipients. In practice such comparison has concentrated on mean (average) share size. But in theory the concept of simple neutrality involves equality of all moments of the distribution.

There are some important relationships to recognize between this concept and those mentioned previously. Note that whenever B1 is true, B2 will be true; however, the reverse does not hold. This property is important in terms of policy considerations: one can require simple neutrality with respect to a suspect group without requiring distributional equality. Since A1 implies B1, A1 implies B2; but not the reverse. That is, equal division ensures simple neutrality, but simple neutrality does not require equal division.

One drawback of a simple neutrality requirement is that it does not allow for differences in average shares (more formally, expected shares or other moments) for which legitimate reasons exist. It is hard to characterize generally the nature of these allowable exceptions. One type of legitimate reason that might arise is recognition that different people, faced with the same opportunities, will respond differently to them and should be allowed to do so. For example, in the open enrollment illustration, perhaps only *some* students would seek entrance to the well-equipped school (rather than the *all* we assumed earlier). If a different proportion of the suspect group compared to all others chooses to apply, then simple neutrality would not be expected to hold. Nonetheless, one might consider this situation one of equal opportunity. Another type of legitimate reason might be that educational characteristics are not necessarily the same across different groups, but it is thought that educational policy should consider them. Suppose in the open enrollment illustration discussed earlier the rule employed by the school district to ration admissions to the school with a laboratory favored the admission of the "best qualified" students. If the proportion of these students in a suspect group differs from the more general student population, then simple neutrality would not be expected to hold. This exception might be judged legiti-

mate. Let us refer to these characteristics of groups which might be allowed to cause deviations from simple neutrality as the "exceptional characteristics." In order to allow for equal opportunity which does not meet the simple neutrality requirement due to the exceptional characteristics, we define one final concept, *conditional neutrality:*

(B3) If the potential recipients in a suspect group had exceptional characteristics identical to those of other potential recipients, then members of both groups should have the same chance for each share.

This concept is related to the others in several ways. As with simple neutrality, A1 implies B3 but not the reverse, and similarly for B1 and B3. This simply means that equal division as well as distributional equality imply that exceptional characteristics do not cause differences (either there are no exceptions allowed, or all potential recipients have identical exceptional characteristics). The more interesting relationship occurs when exceptional characteristics do cause differences and they are not identical across groups. Then B2 and B3 cannot both hold at the same time; that is, in general, simple and conditional neutrality cannot both be true.

Directions of the Past Decade

At this point, we would like to begin the transition from formulation to use of our equal opportunity concepts. To do so, we must have a more precise specification of shares, potential recipients, suspect groups, and exceptional characteristics. In order to provide some guidance for the specification, we turn to a brief review of some recent court decisions and legislative enactments in this area.

We begin with a discussion of the *Serrano* decision, which also serves an additional purpose. We argue that the *Serrano* court requirements involve two different equal opportunity principles which, generally, cannot both be simultaneously satisfied. Furthermore, each principle has quite different policy implications. While this is important on its own merits, it also shows the usefulness of trying to formalize equal opportunity concepts and analyze their logical implications. After the *Serrano* discussion, we summarily review some other decisions solely for the specification purpose.

The Serrano Decision. There are actually three distinct rulings in the *Serrano* case: the 1971 *Serrano I* decision by the California Supreme Court,[12] the 1974 trial court decisions,[13] and the recent 1976 *Serrano II* ruling by the California Supreme Court.[14] Each ruling stated, in some fashion, that the state's system of financing public education must be wealth neutral. In the 1971 decision, considered a major victory for reformers, the court held that the injured interests were those of children living in property poor districts. The injury demonstrated was that there were large disparities in school expenditures per child, where property poor districts had substantially lower expenditures per child than property rich districts. The state caused this injury by allowing the local property tax to be used by districts of varying wealth as a main source of raising school revenues. This

made "the quality of a child's education a function of the wealth of his pa[redacted] and neighbors."[15] Similar suits were filed in states across the nation,[16] though considerable wind was taken out of the movement by the 1973 *Rodriguez* decision of the U.S. Supreme Court. In that decision, the justices rejected a *Serrano*-like argument based on federal constitutional grounds.[17] In the 1974 *Serrano* trial court decision, the court found the California financing system in violation of the state constitution. The 1976 *Serrano II* decision upheld the trial court's ruling.

In terms of equal opportunity concepts, it seems like the violation is one of simple neutrality. The shares, in this case, are district expenditures per child. There is no requirement of equal division; the court is concerned with distribution. The suspect group is identified by its wealth: children in property poor districts (i.e., those with low assessed valuation per child). Potential recipients are children attending the public schools in the state. The average share in the suspect group is lower than the average share of other children, and there are no exceptional characteristics. Simple neutrality is violated because expenditure disparities are wealth-related; the system must be made wealth neutral.

In the court discussion of remedies, it is consistently made clear that they are not requiring a uniform expenditure level, or equal division. While the legislature may choose equal division through full state assumption, they may also redraw district boundaries so that all districts are of equal wealth (analogous to distributional equality), or they may retain local choice in the current districts by using a district power-equalizing system. This system, first discussed by Coons, Clune, and Sugarman,[18] allows local districts to choose their expenditure level, and has the property that districts choosing the same tax rate will receive the same expenditure per child. It is this type of system which has received the most attention from state legislatures, and which is widely thought of as "the" *Serrano* solution. We believe that the power-equalization principle "equal tax efforts should result in equal expenditures" is *inconsistent* with simple wealth neutrality, that the court fails to recognize this inconsistency, and that the priority requirement of the court is simple wealth neutrality.

Note that district power equalizing *imposes a new condition* not mentioned before: that equal tax rates be associated with equal expenditures. If this were to be required by the courts, then they would be specifying an exceptional characteristic. The new requirement would no longer be simple neutrality but conditional neutrality: if tax rates in property poor districts equal those in other districts, the average expenditures per child in each must be the same. We have already shown that when exceptional characteristics are allowed to cause differences, it is generally impossible to satisfy both simple and conditional neutrality simultaneously. Only when the exceptional characteristics—in this case, the district tax rate choices—are *ex post* the same in the suspect group as among other children, can both be satisfied. Many economists have pointed out, however, that the tax rate choices under district power equalizing will still be influenced by wealth and are not expected to be similar among districts of varying wealth. If they should end up similar, it would be a fortuitous result having nothing to do with matters of equity.[19]

If district power-equalizing were only a policy proposal, there would be no serious problem. Legislatures could adjust the design (by violating the equal tax rate, equal expenditure principle) to achieve simple neutrality. This seems to be what Professor Coons suggests:

> Economists seem to assume that even well-intentioned legislatures will adopt formulas that are inappropriate to create fiscal neutrality among their districts. There is nothing in *Serrano* . . . which impedes the adoption of whatever formula will achieve neutrality in fact. The Maine and Wisconsin systems seem to be living examples of neutrality. Yet otherwise useful and able critics like Professor Feldstein [ref. note 19] confound the issues. Under his or other assumptions, a power-equalizing formula could favor poorer districts as he predicts. Anything can be badly designed. But why should this be assumed?[20]

The problem is, there is something in *Serrano* which impedes simple neutrality. The trial court made the following two conclusions of law (among many others):

> The equal protection provisions of the California Constitution require that school districts receive the same revenues for the same tax rate.[21]

And,

> The wide disparities in expenditure levels between low-wealth school districts and high-wealth school districts . . . are unconstitutional because they have significant adverse effects on the quality of the educational programs and opportunities afforded the children in the low-wealth school districts as compared with the quality of educational programs and opportunities afforded the children in the high-wealth school district.[22]

That is, *the trial court seems to require both simple and conditional neutrality*. The only way to do this is by equal division (full state assumption) or distributional equality (redistricting); otherwise the court must settle for only one of the concepts. If they choose to settle, which concept should it be? Conditional neutrality has, in this case, the attractive feature that it can be verified *ex ante*, i.e., without ever looking at what is happening to children. All one needs to do is look at the formula used to finance educational revenues and ask if any given tax rate would produce identical school expenditures in each district. This is a curious principle of taxpayer equity, and it is not clear whether taxpayers would favor it.[23] To meet objections that this is too rigid a definition of taxpayer equity, the court might only require that equal tax "effort" lead to equal expenditures, and leave the legislature to decide what efforts are equal.

But the *ex ante* advantage is also its disadvantage: *ex post* anything could be happening to children. That is the usefulness to the law, indeed the importance, of what the economists have demonstrated: a legal requirement based on some notion of tax equity practically guarantees (except for a fortuitous result) violating the simple neutrality principle regarding the influence of wealth on educational spending. A legislature can adjust the formula, as Professor Coons suggests, only if it is legal to do so. If the child is to be protected from the influ-

ence of wealth, then the court must either abandon its tax equity requirements or recognize that they are requiring full state assumption or redistricting.

The disadvantage of the simple neutrality requirement is that the legislature might have to work harder in order to find an acceptable school finance system which preserves the existing decision-making units.[24] The court could express this intention quite clearly, for example, by stating that wealth neutrality means that the average expenditures per child in districts of one wealth class should equal the average expenditures per child in districts of any other wealth class. Furthermore, it is hard to imagine the court abandoning a principle which protects children from the influence of wealth in favor of one that offers them no protection. In the *Serrano II* decision, the California Supreme Court seemed to make it clearer that it is the violation of simple neutrality which most offends them. They did not strike down, however, any of the trial court conclusions nor disavow the fallacious notion that the condition of taxpayer "equity" will result in the removal of wealth-related expenditure disparities. Thus while the ambiguity remains to be settled, the courts seemed to give priority to the simple neutrality principle in the following passage:

> In view of all the foregoing, it is clear that substantial disparities in expenditures per pupil resulting from differences in local taxable wealth will continue to exist under S.B. 90 and A.B. 1267. The reason for this is that essentially local wealth is the principal determinant of revenue, that high wealth districts do not need to make the same tax effort as low wealth districts in order to reach, let alone exceed, the level of the foundation program.[25]

This priority seems to be affirmed even more strongly in a footnote:

> [T]he fact that disparities in district wealth result in disparities in tax effort required to reach foundation levels is not by itself determinative of the issue before us. It is only insofar as such disparities have the effect of producing disparities in educational opportunity that they here concern us.[26]

To clarify that in the above passage "educational opportunity" refers to school offerings, and that the court is fundamentally concerned with expenditure disparities, we cite one final passage:

> Substantial disparities in expenditures per pupil among school districts cause and perpetuate disparities in the quality and extent of availability of educational opportunities. For this reason, the school financing system before the court fails to provide equality of treatment to all the pupils in the state.[27]

These passages seem to indicate that the Court's use of the conditional test is intended as a rough and imperfect guide to predicting whether the current system will continue to generate actual educational expenditure disparities that are related to wealth differences.

In sum, the *Serrano* trial court requirements cannot all be met in school financing systems which allow local choice from among districts of varying wealth. If it is the court's intention to allow this type of local choice, then they must choose between simple and conditional neutrality. Conditional neutrality

is a principle which requires that districts making the same tax efforts receive the same expenditures: it is associated with policies of power-equalization. As a principle of law, it offers no protection to children from the influence of wealth. As a policy, it may result in substantial correlation between expenditures per child and district wealth. Simple neutrality is a principle that requires average expenditures per child to be the same among property poor districts as among other districts. This guarantees protection to children from the influence of wealth. It is more difficult, but not impossible, for legislatures to implement this principle with existing districts. The California Supreme Court in *Serrano II* has indicated that the simple neutrality principle is of greater priority than the conditional neutrality principle.

The *Serrano* challenges, and the legislative responses to them, suggest three empirical questions about equal opportunity which should be investigated: to what extent do disparities still exist in district expenditures per child, to what extent are they wealth-related, and to what extent would the same tax rate choices in districts lead to equal expenditures per child. We will illustrate approaches to answering each of these questions (and others) in our case study of Illinois.

Other Recent Equity Issues. In this section, we provide a summary review of other recent policy issues as a means of identifying other specifications of shares, potential receivers, suspect groups, and exceptional characteristics.[28]

In another court case, *Hobson v. Hansen*,[29] it was found that *intradistrict* disparities in teacher expenditures per child violated the equal protection clause of the U.S. Constitution when the disparities arise "along racial and socioeconomic lines."[30] Like the *Serrano* case, the court was not requiring equal division; the question was one of distribution. In this case the applicable concept is simple neutrality, the shares are school expenditures on teachers per pupil, and potential receivers are all children attending public schools within the school district. The suspect group is all children attending schools whose population is primarily white and there are no exceptional characteristics.

This case raises the general empirical question about the extent of expenditure disparities within school districts, as well as how expenditures per child vary by race or socioeconomic status currently. While we do not have data on intradistrict expenditures for Illinois, we do know the number of students in each district eligible for federal Title I compensatory funds. By using this as a proxy variable to define a suspect group for students with lower socioeconomic status, we will investigate if equal opportunity has increased for this group in Illinois.

An interesting variation of the school finance reform cases is one in New York State, the *Board of Education of Levittown v. Nyquist.* In this case, Levittown, joined by the school boards of the state's four largest cities (New York, Buffalo, Rochester, and Syracuse), claimed that the state general aid formula does not adequately offset the differences in wealth among school districts because it does not take account of municipal overburden or the higher costs of providing education in some districts compared to others. This challenge is probably best interpreted as a *Serrano* case where the nominal dollar measures of wealth, expenditures, and perhaps tax effort are argued to be inadequate and

inequitable to those districts with higher costs or overburden. While the issues involved in adjusting the nominal measures for these differences go beyond the scope of this paper, we mention this case because it seems to us appropriate that equal educational opportunity should be evaluated in terms of real rather than nominal resources. While some states have made attempts to construct cost indices, their efforts have not been reported as highly successful.[31]

There are several cases which challenge the definition of potential receivers: *Mills v. Board of Education*[32] and *Wolman v. Walter.*[33] The *Mills* case affirmed the rights of handicapped children to publicly funded education, and *Wolman* affirmed and expanded the rights of children attending parochial schools to receive public funds for some educational purposes. The latter case potentially involves a very large number of children, since approximately ten percent of all children in the country attend private rather than public schools. It is interesting that the opportunity offered students in private schools has generally been excluded from considerations that begin with the child's right to equal educational opportunity. We discuss this further in our section on behavioral responses and private education.

One final issue to mention here is the one raised in *Lau v. Nichols.*[34] In this case, the right of non-English speaking Chinese children to bilingual, compensatory education was affirmed. This raises the whole issue, in our minds, of whether the "shares" are to be thought of as resource inputs, or as something that a child is supposed to receive as a consequence of the inputs. That is, it is clear in the *Lau* case that the court rejected the idea that equal input to students represented equal opportunity; some students needed more input to have equal opportunity. But a standard requiring equal opportunity on other than an input basis may be inconsistent with an input standard.

For example, the *Serrano* case held that wealth cannot influence the quality of a child's education. The neutrality standard, however (either simple or conditional) was based on equal opportunity for expenditures per child. The *Lau* standard might suggest another condition on *Serrano*: the expenditures must be adjusted for a pupil "needs" factor *before* one can test for wealth neutrality. Indeed, the New York court in the Levittown case dealt with this issue, as well as the cost issues discussed earlier.

Several states have adopted pupil weighting systems in their general aid formulas, including Illinois.[35] In our case study, we will be able to investigate whether equal opportunity has increased under this definition as well.

Having raised a great deal of empirical questions, we now turn to the task of attempting to answer some of them in our case study.

Measuring Inequality

Empirical work on equity in school finance involves three distinct tasks aside from the choice of which equal opportunity concept to measure. The first is identification of the pre-reform degree of inequality. The second is identification and measurement of the effects of policy changes in educational finance

on this observed inequality. The third task is prediction of the effects of policy changes before they occur. The measurement questions are taken up in this section, and prediction is considered in the next. Our objective is to suggest a common framework for analysis which, when replicated, could be expected to produce comparable results for evaluating the impact of school finance reforms in a wide range of state systems.

As we have seen, there are a variety of interesting concepts concerning equal opportunity and school finance. Since the distribution of public school inputs has been a central policy concern of the equity controversy to date, this review will concentrate on these inputs as the shares children are to receive. To illustrate, we use data from Illinois schools before and following the introduction of a major reform, the 1973 "Hoffman-Fawell Act." We begin with a summary of the system's operation prior to this change, and present empirical measures of divisional and distributional equal opportunity. We then describe the reform and examine its impacts on these measures.

Pre-Reform School Finance in Illinois

Prior to 1973, state aid for education in Illinois was provided under both "foundation" and categorical grant programs.[36] Total local district school expenditures came from three sources: (a) revenue raised locally through the property tax, (b) state categorical and general assistance, and (c) federal categorical programs. State categorical assistance was provided for pupil transportation, certain types of vocational training and special educational programs, and meals for needy students. State general-purpose aid, while a function of a school district's property tax base and its student population, was not influenced by "effort" as identified by the local tax rate. A minimum of $48 per "weighted average daily attendance" or "WADA" was guaranteed every district. Weights in this calculation were determined by grade composition of the average daily attendance level.[37]

For convenience, computations in this paper will utilize only data from "unit" districts--that is, districts combining kindergarten, elementary, and high school. As of 1972-73, there were 436 of them, including Chicago. Since available data simply do not permit proper adjustment of input costs between Chicago and the rest of the state, Chicago has been excluded from the sample. This exclusion sidesteps the important cost-index problem that is the focus of the *Levittown* case. The remaining sample covers approximately 22 percent of the Illinois public school population.

Equality of Division. Table 1 reports the distribution of expenditures for ADA[38] from general purpose funds in Illinois Unit Schools for 1972-73. The data are reported by deciles; the lowest decile represents the bottom tenth of the distribution of expenditures per student.[39] Clearly, substantial inequality in expenditures per ADA existed in Illinois schools five years ago. Individual district expenditures per ADA ranged from $637 to $1,789.

Several judgments are involved in this presentation of the data, and they should be made explicit. First, these distributions are calculated on a per-student

TABLE 1

DISTRIBUTION OF EXPENDITURES/ADA
ILLINOIS UNIT SCHOOL DISTRICTS
1972-1973

Decile	Mean Expenditures/ADA for Decile
1	$ 702
2	750
3	775
4	798
5	824
6	852
7	892
8	916
9	948
10	1,038
Overall mean	849
Standard deviation / mean	.120

Source: Calculations by authors. See note 39.

(actually per ADA) rather than a per-school-district basis. There may be other considerations which suggest data break-out along school district lines; we would caution that the *magnitude* of most equity problems depends on the number of students harmed, not the number of districts. However, any measures derived from district level data, and intended to examine equality among students, do require the assumption that within district distribution of general purpose expenditures is equal. This misspecification continues to be an important matter for research.[40]

The second judgment is that such data are best presented using general purpose revenues and grants. As already mentioned, Illinois schools receive categorical assistance from both federal and state sources to cover certain exceptional expenses and compensatory education (roughly 15 percent of total spending). It is assumed here that such funds, when distributed to cover exceptional district costs such as those related to transportation, appropriately adjust for differences in costs of providing education across districts and thus rectify inequities created by cost differences beyond the control of the district.

Whether or not such grants do rectify cost differences is a matter for separate investigation. We treat categorical grants for special student needs as if such payouts are not relevant to achieving equal opportunity for inputs (that is, they are intended to supplement the fair input share in order to achieve an outcome effect). To the extent that categorical grants for such needs are spent as part of the general fund, however, these monies belong in the data used here. Here, again, the degree of misspecification produced by these assumptions is an important object for research.[41]

Third, aside from the mean and coefficient of variation of the raw data, no summary measures of inequality have been presented.[42] This may appear somewhat paradoxical, given the amount of research devoted to the problem of and procedures for measurement of inequality.[43] The motivation of this research is that such an index, if available, would permit summary assessment of the underlying distribution for the data reported in table 1 and would permit unambiguous statements about the equity effects of possible school finance changes on this distribution. However, although many measures have been proposed no one has been generally accepted.

The reason for the absence of consensus is that distributions cannot, in general, be uniquely described by one or two numbers. At minimum, preferences in this area seem to involve at least three characteristics of distributions—mean, spread, and kurtosis. No single index can cope with this problem. Under this circumstance, listing by deciles—which gives a readily grasped picture of the whole distribution—seems to present more useful information than any additional summary measures (such as the variance of logarithms, the gini coefficient, etc.). It should be recognized that for some purposes, a comparison of various summary measures may be useful.[44]

Simple Neutrality for Wealth and Poverty Groupings. As discussed in the preceding section, public, legislative, and judicial attention in matters of school equity has involved not only equality of division, as illustrated in table 1, but also neutrality of the distribution process for various groups of students. For illustration, this section will consider the *Serrano* relation between school district expenditures per ADA and district wealth per student. For an alternative grouping, we also consider the relation between expenditures and family income of students, where the latter classification is made in terms of poverty status.

There is an important empiricial distinction between *discrete* (and usually dichotomous) classifications, like race, and *continuous* classifications, like wealth. If one asks if students of a certain race receive an equitable share, it is reasonably clear how to divide all students into those in the suspect class and those not in it (though, to be sure, there will be a few students hard to classify). However, if one asks if children in property poor districts receive an equitable share, then one has to empirically decide where the cut-off point is for "property poor"—e.g., should it be all districts with less than average wealth, or the poorest 5, 10, 20, or 40 percent of all districts? In thinking about resolving the *Serrano* issue, analysts assume that the neutrality requirement would apply to *any* grouping of districts by property wealth, *including* rich districts. That is, a school financing system which so restricted the ability of rich districts to generate educational

expenditures for their children, compared to other districts, would be just as undesirable and perhaps unconstitutional as the reverse.[45] Empirically, this roughly translates into a requirement that average expenditures per child be the same in every wealth class no matter how narrowly defined.[46]

One easily understandable way of testing this requirement is arbitrarily to divide all students into some finite number of wealth groupings (like deciles) and calculate the average expenditure per child for each group. A more precise (but less transparent) method is to compute a regression based on a sufficiently general functional form to capture any peculiarities of the wealth-expenditure relation that may exist.[47] With this method, simple neutrality is statistically satisfied when the coefficient of each term in the regression (except the constant) is not significantly different from zero. In application, small coefficients, even if statistically distinct from zero, may be judged to have little policy relevance (e.g., there may be a statistically significant increase in expenditures of $10 per child from the poorest to the richest districts, but it is unlikely such a small difference would cause alarm).

As table 2 indicates, in 1972-73 there existed a clear, positive relationship between expenditures in a student's school district and the ratio of property

TABLE 2

AVERAGE EXPENDITURE/ADA CALCULATED BY DECILE OF THE DISTRIBUTION OF PROPERTY TAX BASE/ADA

1972-1973

Decile[a] of Assessed Value/ADA	Mean Assessed Value/ADA in Wealth Decile	Mean Expenditure/ ADA for ADA in Wealth Decile
1	$ 9,466	$742
2	13,364	779
3	15,515	843
4	17,391	815
5	18,589	862
6	20,092	857
7	21,848	886
8	23,100	893
9	25,674	892
10	37,539	920

Overall mean assessed value/ADA = $20,231

Overall mean expenditure/ADA = $849

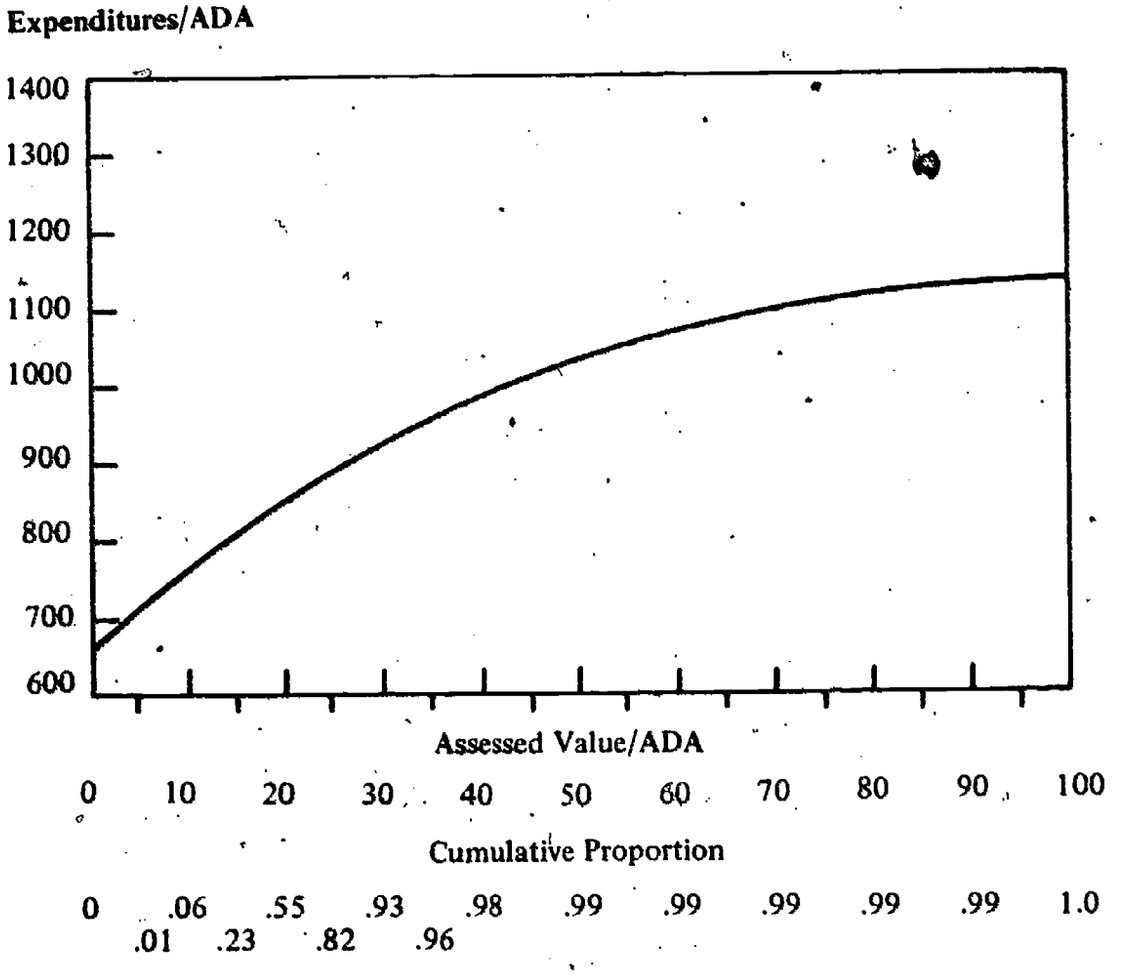
$$\frac{\text{Standard deviation}}{\text{mean}} = .1195$$

[a]Approximate; see note 39.

tax base to school size. In equation (1) we have summarized this relationship with a regression. The range of wealth from the student in the poorest district to the one in the richest was, per ADA, from $2,921 to $99,962. The regression indicates that this was associated with a predicted difference of $439 per student in expenditures. Although the relation in not linear over the range of wealth exhibited by Illinois unit school districts, it is monotonically increasing and statistically significant. The R^2 figure indicates that about 31 percent of variation in school district expenditures for students in this sample was associated with variation in the property base in the student's school district.[48] The relation between expenditures and property tax base implied by equation (1) is plotted in figure 1. The figure includes the cumulative proportion of unit district ADA on the horizontal axis. Note that districts with assessed valuation per ADA between $10,000 and $30,000 accounted for slightly over 86 percent of the attendance in our sample.

FIGURE 1

ESTIMATED EXPENDITURES PER ADA BY TAX BASE, ILLINOIS UNIT SCHOOL DISTRICTS 1972-1973



$$\frac{\text{Expenditures}}{\text{Average daily attendance}} = 658.49 + .01128(w) - .8558 \times 10^{-7}\,(w)^2 + .1989 \times 10^{-12}\,(w)^3 \quad (1)$$

w = assessed valuations/ADA

$R^2 = .31$

In the *Serrano* trial court decision concerning California, Judge Jefferson ruled that wealth-related disparities exceeding $100 per child were unconstitutional. If this standard had been applied to Illinois, and the simple neutrality interpretation of *Serrano* used, then our tests indicate that the pre-reform system in Illinois would have been held unconstitutional. (Note, however, a court may require a finer breakdown than deciles.)

For an additional example of simple neutrality evaluation, we compare expenditures/ADA for children from poor households to the same figure for the rest of the children in unit schools. For purposes of illustration, we treat as "poor" the estimated number of children in each district certified eligible for categorical assistance under Title 1 of the Elementary and Secondary Education Act of 1965.[49] A high proportion of ESEA eligibles[50] is, in general, likely to mean that a high proportion of school enrollment is accounted for by children from minority and/or poor families. Other tests could be conducted on the basis of family income or minority status alone.[51]

The simple neutrality test for this case is a comparison of mean expenditure/student for Title 1 eligibles to expenditures for all others.[52] Because the classification is discrete and dichotomous, this is all that is required. The comparison appears in table 3. These results indicate that Title 1 eligible students experienced,

TABLE 3

EXPENDITURES PER STUDENT IN OWN SCHOOL DISTRICT BY ESEA TITLE I ELIGIBILITY 1972-1973

	Title I Eligibles	All Others
Expenditures/ADA	$840	$850

on average, $10 fewer resource "inputs" than did other students when the federal assistance that comes with this designation is not considered. Therefore, ESEA money, in part, served to compensate for the modest disadvantage imposed on students in this group by the school finance system. Our data indicate that there were about 100,000 Title 1 eligibles in unit districts in 1972-73. Thus, it would have taken $1 million in ESEA money to compensate for this disadvantage alone.[53]

Conditional Wealth Neutrality. Finally, consider a conditional wealth neutrality test. Suppose all districts choose the same tax rate. What would be the resulting inequality of expenditures? In table 4, the distribution of expenditures from general purpose grants and revenues is reported for unit districts on the assumption that all adopt, as an operating tax rate, a levy of $2.47 per $100 of assessed valuation.[54] As is indicated by table 4, the dispersion of expenditures clearly does not pass a conditional wealth neutrality test. It is interesting to note that the inequality in division under the conditional assumptions exceeds the inequality of the actual division (the coefficients of variation are .1425 and .1195, respectively). Prior to 1973, school districts partially compensated for variation in the tax base by establishing an inverse relation between tax rates and tax base.

TABLE 4

CONDITIONAL NEUTRALITY TEST
AVERAGE EXPENDITURE/ADA WITH UNIFORM TAX RATE[a],
BY DECILE OF ASSESSED VALUATION/ADA

1972-1973

Decile[b] of Assessed Value/ADA	Mean Assessed Value/ ADA in Wealth Decile	Mean Expenditure/ADA for ADA in Wealth Decile
1	$ 9,466	$ 733
2	13,364	761
3	15,515	801
4	17,391	812
5	18,589	830
6	20,092	835
7	21,847	862
8	23,100	901
9	25,674	898
10	37,539	1,057

Overall mean assessed value/ADA = 20,231

Overall mean expenditure/ADA = 348.50

$$\frac{\text{Standard deviation of expenditure/ADA}}{\text{mean expenditure/ADA}} = .1425$$

[a]2.47 percent.

[b]Approximate; see note 39.

Note that the conditional test is meaningful only as an illustration of the existing properties of the tax and grant procedures Illinois uses for financing its schools; it is not a prediction. If such an "experiment" were ever actually run, the change would affect property values through capitalization.[55] Such effects are not included in conditional neutrality computations of the type illustrated in table 4.

Summary. These data reveal considerable inequality in the division of expenditures among pupils in Illinois unit school districts during the 1972-73 school year. The system also fails both simple and conditional wealth neutrality tests. The simple neutrality test based on student poverty status, as identified by ESEA Title 1 eligibility, indicates only a small deviation from neutrality. The conditional neutrality test indicates that if each district had chosen the same tax rate, the inequality among students would have been greater than in the actual division.

ave made the following methodological recommendations:

- Analysis of equity and distribution should be carried out primarily on a per-student basis.
- Simple tables are a very useful way to present data on the equality of opportunity for educational outlays.
- Equity of input tests are probably best carried out using only expenditures from general purpose funds, though this requires making several assumptions about the use of categorical funds.
- Equal opportunity tests can be usefully carried out on the basis of poverty estimates available on a district basis. Improvements in their quality should be considered an important objective for facilitation of equity evaluations as well as distribution of grants-in-aid.

The Effect of Reform

The effects on equality of inputs of a change in school finance procedures is usefully divided into three phases:

(1) The *impact* effect: The change in expenditures per student brought about by alteration in state aid formulas, all behavior held constant.

(2) The *intermediate* effect: The change in expenditures per student after voter response to the new procedures has been realized and local tax rates have achieved new "equilibrium" levels.

(3) The *long-run* effect: The change in expenditures per student brought about by shifts in the "equilibrium" tax rates due to altered locational patterns of firms and households, and household compensatory changes in private purchases of education.

Both district and family adjustments in the face of alterations in finance procedures take time, and such adjustments are further delayed when policy alterations are phased in gradually. Given these lags in adjustment, analysis of equalizing effects of reform must focus not only on the impact effects of such

changes on data, but also on the intermediate effects and on the nature of the incentives the system creates for further change.

Most reforms can be described in terms of three effects on school district and family behavior. The first is the *income effect*, the alteration in the amount of resources a district has to meet the various demands it faces (for schooling, police, fire protection, etc.). This is calculated as the change in school aid assuming no change in the tax rates. The second is the *price effect*, the alteration the reform imposes on the terms of trade between other expenditures and school expenditures (the change in local revenue required for each school expenditure level). The third is the *rules effect*, brought about by constraints imposed by legislation of the tax-expenditure choices made by districts. A system of block grants for education (like basic aid, or other state aid that has no matching requirements) has only an income effect; regardless of the size of the grant, the increment in expenditures per ADA that can be accomplished for a given change in the local property tax rate remains constant. On the other hand, a system of matching grants (where the state guarantees some increment in state aid for each dollar of local tax revenues raised) alters the price to voters of additional outlays on education.[56] A statutory maximum on permitted local property tax rates for education is an example of a "rules" reform.

Given these common properties, equity evaluation of procedures for school finance change is conveniently based on description of the income, price, and rules effects of such changes on districts and the distribution of such changes by district characteristics. If it is determined that an unacceptable relation exists between district wealth and expenditures—the essence of the *Serrano* decision—analysis of a reform must concentrate on the degree to which proposed reforms act to counter the unacceptable relationships. To see how this might be done, we return to the Illinois case.

The 1973 Illinois School Finance Reform. The core of the 1973 Illinois school finance reform was a "resource-equalizer" formula, a funding mechanism similar to district power equalizing. The new program guaranteed an expenditure level for each possible tax rate choice, by providing general state aid to make up the difference between what a district receives from its own sources and the guarantee. If its assessed valuation per weighted average daily attendance were less than $42,000, a district would receive in own taxes and "equalizing" state aid revenues equal to what it would have enjoyed had assessed valuation been $42,000.[57]

The plan was not "pure" district power equalizing, however. The modifications are numerous and illustrative of the substantial complications the researcher encounters in work of this type. The principal difference between strict DPE and the Illinois system is that no "recapture" of revenues existed for districts with assessed valuations in excess of the equalizer standard. Furthermore, districts were given the option of receiving the grant implied by the resource-equalizer formula, the grant that they would have received under the foundation plan in operation in 1972-73, or, as a third alternative, a foundation-type ("Strayer-Haig") grant calculated on the basis of a slightly modified pupil-weighting scheme. For both the Strayer-Haig alternative and the resource-equalizer formula, the state chose to concentrate aid on districts with above-average num-

bers of Title 1 eligibles by adjusting district attendance figures upward according to the numbers of eligibles present. Although this adjustment was complicated,[58] the essential impact under both was to create an income effect, where the size of the grant to each district is made on the basis of the estimated number of children from poor families. For districts which chose the equalizer formula for state aid computation and had large numbers of Title 1 eligibles, the weighting scheme also lowered the tax price of education.

The exceptional weight given Title 1 eligibles by the Illinois reforms posed a problem of interpretation. Basically, there are three different options for thinking about the nature of these funds: (1) the money can be treated as categorical assistance designed to improve educational attainment for poor students; (2) the money can be treated as compensation for exceptional costs associated with teaching lower-class children; or (3) the money can be treated as additional general-purpose transfers. Since we are focusing on wealth neutrality tests for inputs, let us consider whether and how one would include them under each alternative interpretation. Under options (1) and (2), the funds can be excluded if the usual student weighting (WADA) is used in the calculation, or included if the special pupil weighting (TWADA) is used. Both of these follow from thinking of the reasons for the funds as "exceptional characteristics" which the calculation must take into account. To control for them when they are included, one must weight the total by the amount of special "needs" or "high cost children"—the TWADA measure. When they are excluded, the exclusion itself serves as the control (thus WADA is appropriate). Under either of these options, one must be careful in evaluating *changes* in equality over time. No matter which measure is used for the reformed system, the pre-categorical system should be TWADA weighted; otherwise the new system is effectively adjusted for these factors but the measure for the pre-categorical system is not (as if there were no "needs" or "high cost children" before reform).

In this instance, however, we favor option (3). The reason for favoring it here is primarily because Illinois places no restrictions on the use of funds given to districts on the basis of Title 1 eligibles. Indeed, the state does not even report the portion of local aid attributable to the poverty count. Therefore, they seem more like general purpose grants. We recognize that a strong case can be made for concentration of aid upon *districts* which are high cost; however there is no evidence that the number of Title 1 eligibles represents a reasonable cost index. Without this evidence, and with no attempt to ensure the appropriate children receive the funds, we will treat these as equivalent to general purpose aid. Under this interpretation, the funds are included in the equal opportunity tests with the usual student weighting (WADA).

In addition to the resource-equalization and student-weighting properties, the law included numerous other provisions. The most significant were a partial phase-in procedure that precluded substantial year-to-year changes in district state aid and a ceiling on allowed local tax rates. The first change constrained each district to an increase in state aid of no more than its aid entitlement under the old system, plus one-fourth of the difference between the old and new entitlements. The second restriction was apparently inserted to assure that part of

the new state aid made its way into tax relief. However, numerous possibilities for override of the restriction are included in the law. If district taxpayers want a tax rate in excess of the maximum, they can vote it.

Identifying the Effect of the Reform. Since the long-run effects of the Hoffman-Fawell Act on school finance in Illinois are yet to be observed, we begin with an analysis of the *ex ante* properties of the system (those that can be examined without observing the system in operation, based on the new formula). Above, we demonstrated that school expenditures per ADA in Illinois showed considerable variation across students in 1972-73, and that substantial variation was associated with school district wealth. Thus, a reform aimed at equalization of school inputs should have both "income" and "price" effects which favor low-expenditure and low-wealth districts.

Under a shift from a foundation-grant plan to "pure" district power equalization, income and price effects are readily inferred from the aid formula. However, in a system as complicated as Illinois', the distribution and consequences of such changes are more obscure. One procedure for summarizing the incentive effects of reform is to calculate income and price effects explicitly and then regress the changes on initial district expenditures or assessed valuation per student. We have done this for the Illinois reform; the estimated income effects appear in equations (2) and (3), below. The left-hand variable for both regressions is estimated *change* in local school district expenditures, given the change from the foundation to the resource-equalization system, no change in district tax rates, and 1972 assessed valuation. This represents the "income effect." For this purpose, "ultimate entitlements" are calculated; we have ignored the fact that such changes were originally planned to be phased in over four years. The calculations retain the "safeguard" provisions of the Illinois reform, which precluded loss in revenue or aid by any district.[59]

$$\frac{\text{Change in aid}}{\text{ADA}} = \underset{(7.6)}{\$341} - \underset{(5.1)}{.2714}\ (\text{1972-73 Expenditures/ADA}) \qquad (2)$$

$$R^2 = .05$$

$$\frac{\text{Change in aid}}{\text{ADA}} = \underset{(23.8)}{\$281} - \underset{(15.7)}{.007122}\ (\text{Assessed value/ADA}) \qquad (3)$$

$$R^2 = .36$$

Equation (2) indicates that the new system was likely to concentrate aid to a modest extent on low-expenditure districts, although the relationship is relatively weak. Equation (3) indicates that aid was clearly concentrated on districts with a small per-pupil tax base.

The price effect of the reform can be illustrated by the relationship between the marginal tax rate increase required to increment expenditures per ADA by $50 in these districts before and after the reform (call this the "tax-price"). We have calculated this change again on the basis of 1972-73 assessed valuation and all properties of the new law except the graduated phase-in restrictions. Equations (4) and (5) show the estimated change in price regressed on district expenditures and wealth.

$$\text{Change in marginal tax increase required} = -.006 + .629 \times 10^{-5} \quad \text{(1972-73 Expenditures/ADA)} \qquad (4)$$
$$(14.2) \quad (11.4)$$
$$R^2 = .23$$

$$= -.004 + .479 \times 10^{-8} \quad \text{(Assessed value/ADA)} \qquad (5)$$
$$(28.1) \quad (19.0)$$
$$R^2 = .45$$

These regressions indicate that the price change increased with the initial district expenditure level and with district wealth. Both effects create incentives for increased equalization: the incremental tax effort necessary to raise an additional $50 rises with both expenditures and wealth. We have also calculated the increase in grant and tax-price change as defined above for Title 1 eligible students and students who are not. These results appear in table 5. The effect of the new system with regard to students in this classification is quite pronounced. On average, school districts which included Title 1 eligibles would receive, by the new-law formula, almost twice as much additional aid per student under the new system as was true for districts of noneligibles. This difference greatly exceeds the initial difference between the classes of students as measured in table 3. The price effect of the formula change is also significantly greater for eligibles than for noneligibles. Clearly, the revision in finance procedures operates to expand expenditures for Title 1 eligibles over noneligibles.

TABLE 5

INCOME AND PRICE EFFECTS OF ILLINOIS SCHOOL FINANCE REFORM

	Mean Change In State Aid Per ADA	Mean Change In "Tax Price"[a]
Title 1 eligibles	$315	-.0022
Non-Title 1 eligibles	$153	-.00153

[a] See text.

As a final illustration of the properties of the new system, consider again the *conditional neutrality* test. Suppose all districts were to choose the same tax rate. What would be the effect on the dispersion of expenditures? For convenience, we have calculated this test on the basis of 1974-75 attendance, expenditures, and tax base figures. The results appear in table 6. Clearly, in terms of conditional neutrality the reformed Illinois system is substantially superior to the old foundation grant program. However, it still fails a strict interpretation

of the "Jefferson" test—wealth-related disparities in expenditures exceed $100 between schools in the various property value deciles. Assuming all districts apply a tax rate of 2.38 percent, no pattern of uniform positive association between expenditures and wealth emerges. Indeed, if anything here the relationship is U-shaped, with highest expenditures in the lowest (because of a high poverty count)[60] and highest (because of the option of collecting aid calculated on a foundation grant basis) deciles. Choice of a common tax rate produces a coefficient of variation of expenditures of .123. Recall that the corresponding ratio for expenditures under the "conditional neutrality" test for the system in operation during the 1972-73 school year was .1421.

To summarize, while the price and income effects of the reform appear to be favorable for increased equalization of expenditures, anomalies clearly exist. Most of these appear to be associated with the exceptional weight placed on Title 1 eligible students in distribution of state aid under the revised formula. When this aid is treated as general purpose aid, the Hoffman-Fawell reform fails the conditional neutrality test.

TABLE 6

CONDITIONAL NEUTRALITY TEST AVERAGE EXPENDITURE/ADA WITH UNIFORM TAX RATE[a], BY DECILE OF ASSESSED VALUATION/ADA
1974-1975

Decile[b] of Assessed Value/ADA	Mean Assessed Value/ ADA in Wealth Decile	Mean Expenditure/ADA for ADA in Wealth Decile
1	$ 9,488	$1,191
2	13,999	1,025
3	15,950	1,020
4	17,746	1,018
5	19,336	1,018
6	20,715	1,021
7	22,697	1,029
8	24,076	1,006
9	26,524	1,036
10	38,393	1,125

Overall mean assessed value/ADA = $20,787

Overall mean expenditures/ADA = $1,050

$$\frac{\text{Standard deviation of expenditures/ADA}}{\text{Mean expenditures/ADA}} = .123$$

[a] 2.38 percent.

[b] Approximate; see note 39.

Measured Effect of Reform on Equality. The ambiguity encountered in evaluation of properties of the reform package reappears when actual post reform expenditure data are examined. In table 7 the distribution of expenditures by decile is presented again for 1972-73 (see table 1) and for 1974-75. These data indicate that dispersion in the division of expenditures actually increased somewhat between the two years. While the dollar amount of expenditures in these school districts clearly increased, the real value of educational outlays probably

TABLE 7

DISTRIBUTION OF EXPENDITURES/ADA ILLINOIS UNIT SCHOOL DISTRICTS 1972-1973 AND 1974-1975

Decile	Mean Expenditures/ADA for Decile	
	1972-1973	1974-1975
1	$ 702	$ 791
2	750	850
3	775	880
4	798	912
5	824	952
6	852	982
7	892	1,016
8	916	1,061
9	948	1,120
10	1,038	1,201
Mean	849	973
Standard deviation / mean	.120	.125

declined. Between 1972-73 and 1974-75, the price deflator for state and local government purchases of goods and services in the U.S. National Income and Products Accounts rose by about 19 percent. If this is an acceptable proxy for a price index for education in Illinois, the mean real value of educational services within these districts fell by about 4 percent.

It should be noted that the equalizing effect of the Illinois reforms was somewhat ameliorated in 1974-75 by the disruptive effect of the recession on state finances. State aid was actually funded at a level about 11 percent less than the entitlements determined by the official formulas. Since, under Illinois state aid procedures, state aid in general constitutes a larger proportion of expenditures for low-wealth districts than for high ones, the impact of this adjustment was to reduce equality of expenditures.

Simple Neutrality for Wealth and Poverty Groupings. Table 8 reports mean expenditures per ADA for each wealth decile for 1974-75. Table 8 should be compared to table 2, in which the corresponding distribution was reported for 1972-73. Although the comparison of table 1 and table 7 revealed the two-year impact of the Hoffman-Fawell reform on *equality of division* to be negligible, the initial impact of the changes on *simple wealth neutrality* is more striking. Wealth-related disparity in expenditures among students in Illinois unit school districts is far less significant in 1974-75 than was true in 1972-73. In fact, at least in the short run, the Hoffman-Fawell reform satisfies the simple neutrality version of the "Jefferson" test.

Essentially the same conclusion can be derived by use of a regression. Equation (6), below, is the product of regression of expenditures per ADA on school-district wealth under the new system. Two important inferences can be drawn from these results. First, by 1974-75; a far smaller portion of interdistrict variation in expenditures per ADA could be "explained" by interdistrict variation in tax base. The R^2 in the expenditure equation has fallen from .31 to .05. Second, the "slope" of the relationship has become essentially zero over the range of school district tax base typical of the school districts attended by the great majority (over 86%) of pupils in unit schools in this sample. From the regression, the range of predicted expenditures between the school districts with the lowest and

TABLE 8

AVERAGE EXPENDITURE/ADA CALCULATED BY DECILE OF THE DISTRIBUTION OF PROPERTY TAX BASE/ADA

1974-1975

Decile of Assessed Value/ADA	Mean Assessed Value/ ADA in Wealth Decile	Mean Expenditure/ADA for ADA in Wealth Decile
1	$ 9,488	$ 958
2	13,999	966
3	15,950	944
4	17,746	962
5	19,336	1,000
6	20,715	978
7	22,697	988
8	24,076	944
9	26,524	968
10	38,393	1,006

Mean assessed value/ADA = $20,787

Mean expenditures/ADA = $973

$$\frac{\text{Standard deviation expenditures/ADA}}{\text{Mean expenditures/ADA}} = .125$$

the highest assessed valuation/ADA is still $310, from $892 to $1292. However, over the tax base interval $10,000-$30,000, predicted expenditures rise from $964 to $989, an increase of only $25. This is, of course, consistent with the information in table 8.

$$\frac{\text{Expenditures}}{\text{ADA(1974-75)}} = 993.46 - .0046W + .1790 \times 10^{-6} W^2 - .1031 \times 10^{-11} W^3 \quad (6)$$

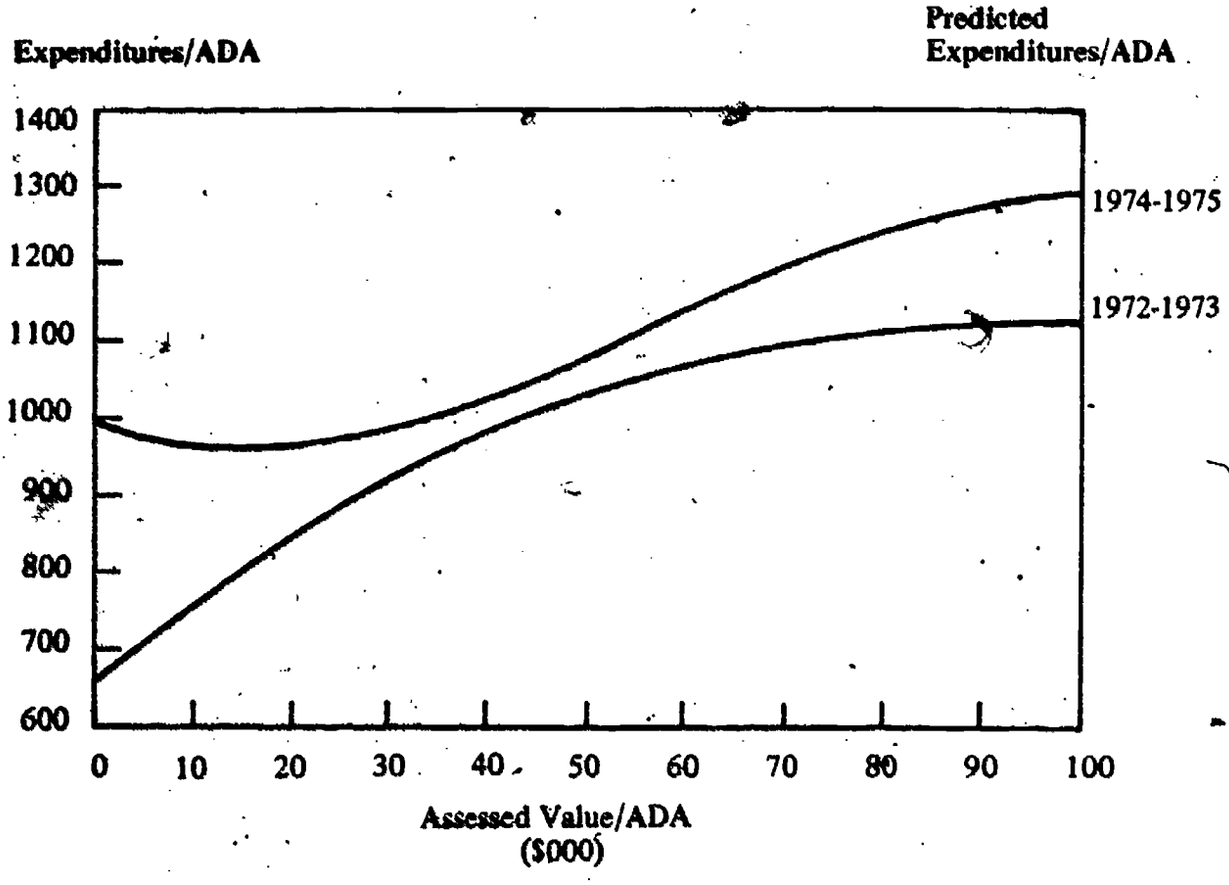
$$R^2 = .05$$

W = Assessed valuation/ADA

In figure 2, expression (6) is plotted on the same scale as was used for plotting the previous expenditures-wealth relation. In both 1974-75 and 1972-73, approximately 86 percent of all unit district students attended schools in districts for which the property tax base per ADA fell in the $10,000-$30,000 range. As is apparent from the graph, the new finance procedure made mean expenditures virtually independent of wealth within this group. Indeed, it is only for the 7.8 percent of total ADA located in districts with wealth per student greater than $30,000 that the old relation seems to have endured. This is approximately the group for which aid continues to be allocated on the basis of the adjusted

FIGURE 2

ESTIMATED EXPENDITURES PER ADA BY TAX BASE
ILLINOIS UNIT SCHOOL DISTRICTS
1972-1973 and 1974-1975



foundation formula. The *increment* in aid within this group between 1972-73 and 1974-75 is largely attributable to the Title 1 student-weighting formula.

Finally, actual expenditures for Title 1 eligibles calculated on the basis of reported data increased substantially and, for 1974-75, exceeds expenditures for non-Title 1 eligibles by $34. This suggests that the virtually indistinguishable effect of the reform on divisional inequality of school expenditures in Illinois unit school districts is due to the offsetting nature of two parts of the formula. On the one hand, the resource-equalizer component has, for the great majority of students, broken the relation between district property tax base and expenditures. On the other hand, exceptional aid given on the basis of Title 1 count, plus interdistrict differences in choice of tax rate, increases the extent of inequality of division. We conclude that an important empirical issue facing those concerned with equality in this system is the extent to which the weighting scheme implicit in the formula is justifiable on objective grounds.

As is apparent from figure 2, the Hoffman-Fawell Act substantially increased aid received by all Illinois school districts. This "new money" is probably the dominant influence behind the short-run equity consequences of the reform that we have measured in this section. The long-run effects on distribution depend upon district tax and expenditures adjustments to the new system. We now turn to procedures for predicting these responses.

Behavioral Responses to Financial Reform

The long-run effect of the Hoffman-Fawell Act on equality of education in Illinois will depend on the response of voters in school districts to the price, income, and rule effects the change introduced. In addition, these changes may affect the use of private schooling, and may have substantial relocation effects on households and firms. We believe consideration of these factors to be a critical part of equity analysis in education. In this section we discuss illustrative examples of research on school district and household response to finance reform and the implication of such work for predicting the consequences for equality of educational opportunity. The first section discusses school district response. The second section discusses the use of private education, and the final section considers household locational choice.

School District Response

In most of the early literature on school finance reform, the impact of changes in the finance system was evaluated on the basis of plausible but arbitrary assumptions about district choice of expenditure levels or tax rates under the new arrangements.[61] The shortcomings of this research were widely recognized, and subsequently researchers began to analyze explicitly the response of

school districts to the "income," "price," and "rule" effects of reform. The object of this research was estimation of a district "demand" function which isolated and quantified the effect on district expenditure levels of voter incomes, the size and composition of the local property tax base, the amount and character of education grants received, and the "price" of education (as determined by the elasticity of externally-provided grants with respect to locally-raised revenues). Such demand functions are expected to provide better projections of long-run district response to reform plans like district power equalizing.

An example of research on such effects and incorporation of them in evaluating equity effects of school finance reform is provided in two papers on local school finance recently published by Helen F. Ladd.[62] The first paper reports the results of estimation of a demand for education function for a sample of Boston-Area Massachusetts school districts. In this study education is measured by inputs: expenditures per pupil from all funds in 1970 for schools in the sample.

The independent variables in Ladd's model include measures of income, price, and taste effects. The *income* of voters (or, more precisely, the income of the decisive "median" voter) is represented by median family income within the district, the ratio of market value of residential property within the district to the total number of students present, and the amount of externally provided block grants. The *price* of education is assumed to be a function of available general purpose matching (not block) grants and the composition of the tax base.[63] *Taste* effects are considered somewhat crudely by including as independent variables the number of pupils attending public schools, and the share of the population employed in the "professional, technical, and kindred worker" positions. Taste effects are assumed not to influence the price and income elasticities of education demand.

Results of estimation of the equation are best interpreted by recalling the questions raised in the preceding section about the long-term consequences of the Illinois reforms. Ladd's calculations suggest: (a) that the effects on district expenditures of the sizable increase in state aid will diminish with time as the funds are absorbed as tax relief (i.e., substitute for, rather than supplement, local revenues); (b) that the "price" effect of the reform will have a substantial effect on district expenditures; and (c) that effects of the reform will differ according to the composition of the tax base in each district.

Concerning the "income" effect, Ladd estimates that $1 in block (i.e., general purpose, non-matching) grants will result, in the long run, in only about $.50 additional district expenditures. Recall that the Illinois data we presented demonstrated a substantial improvement in simple wealth neutrality of the school finance system in that state. This improvement was largely the consequence of the massive increase in state aid, concentrated on low-wealth districts, which occurred with implementation of the reform program. If Ladd's results are applicable to Illinois districts, this favorable "income" effect should diminish over time.

The diminution of the wealth neutralizing effect of the Illinois reform that occurs as grant monies are absorbed into tax relief will be offset to some extent by the "price" effects of the reform. Ladd's results indicate that for the Massa-

chusetts school districts in her sample expenditures are quite price elastic. Thus the higher the matching rate, the greater will be district expenditures. As we showed earlier, in general the matching rate for state grants in Illinois is, under the new finance procedures, inversely related to school district wealth. Thus the net long-run effect of the reform on simple wealth neutrality depends upon the relative strength of the "grant absorption" and "expenditure stimulation" effects. Ladd's estimates are consistent with Feldstein's warning that, in the long run, district power equalization type transfer programs may produce an *inverse* relation between wealth and expenditures.[64] Under the influence of extremely favorable state grant matching rates, expenditures in low wealth districts will grow faster and ultimately surpass those in high wealth districts.

Prediction of the long-run consequences of the Illinois reform is further complicated by lack of data on the composition of the tax base in Illinois school districts. Ladd's results indicate that the higher the proportion of the tax base attributable to non-residential wealth, the greater will be the willingness of the school district to spend. The effect is further complicated by differential influences of commercial and industrial property. Since even information on the residential share of the tax base for Illinois districts is not publicly available, the long-run effect of the Hoffman-Fawell Act (assuming no further legislative changes) cannot be predicted. This is unfortunate for many contexts of educational policy making, particularly those attempting legislative guidance.

The Ladd results confirm the importance of the suspicions raised earlier: prediction of the long-run effects of reforms like those undertaken in Illinois is very difficult and depends substantially on information that can only be obtained through econometric analyses, and, in some cases, better data collection.

In her second paper, Ladd utilized the demand function estimated earlier to predict by simulation the impact of reforms aimed at neutralizing the effect of commercial and industrial property on educational expenditures. These simulations are of considerable policy interest given frequent references to the distribution of non-residential property as a source of inequities in school expenditures. The reform she considered was uniform taxation of all commercial and industrial property regardless of location, combined with return of the tax collected in grants to local school districts. The effect of this procedure was to eliminate commercial and industrial property effects from the "price" term in the demand equation and to increase the "block grants" received by each district. Two grant schemes were utilized: In one, the money collected from non-residential taxes was returned to the district on a flat grant per pupil basis. In the other, the grant was made proportional to the district's pupil/residential wealth ratio. The "simulation" consisted of making these changes, calculating the grants and revised "price" term for each district in the sample, and then calculating district expenditures under both new finance schemes. The results could then be compared to values that the demand equation predicted for the districts in the absence of the hypothetical reforms.

We have recalculated some of Ladd's results on a per student basis (as well as a per district basis); the results are reported in table 9.[65] These calculations have several features that should be of interest to those contemplating either

plan as a policy for equalizing inputs in Massachusetts. First, both reforms increase school expenditures, but the consequences for inequality of division as measured by the coefficient of variation are ambiguous using unweighted calculation procedures and clearly detrimental on a per student basis. For both calculations the "redistributive" form of the grant is superior to the flat-grant alternative with regard to the estimated coefficient of variation of post-reform expenditures. Second, weighting the calculations by district size *reverses* Ladd's conclusions concerning the effect of these reforms on the distribution of expenditures by poverty status of school district families. The third set of columns show the simple correlation of expenditures per student and proportions of families in poverty under the original system and for each of the hypothetical reforms. Ladd's calculations reveal virtually no effect of reform on this correlation; our recalculations, weighted by school district attendance, lead to the conclusion that either of these changes would probably have a relatively favorable effect on districts with sizable numbers of children from poor families. Finally, unlike the case for the Illinois figures, for the Ladd data we have the value of residential wealth per student within the school district. This can be used as a rough measure of average family income in the district. The data in the last columns of table 9 indicate that both reforms can be expected to reduce modestly the correlation between expenditures per pupil and local residential wealth per pupil. The "redistributive" formula is slightly more successful in this respect than is the flat-grant formula.

TABLE 9

SIMULATION RESULTS, ALTERNATIVE MASSACHUSETTS SCHOOL FINANCE PROCEDURES

Method	Mean[a] Expenditure		Coefficient of Variation Expenditure/Pupil		Zero-order Correlations of per Pupil Expenditures with: Proportion of Families in Poverty		Residential Wealth per Pupil	
	Weighted[b]	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted
Existing system, calendar year 1970	938	952	.124	.134	.14	.30	.61	.67
Uniform taxation of commercial and industrial property; flat grant redistribution of proceeds	958	976	.136	.137	.14	.36	.49	.59
Uniform taxation of commercial and industrial property: proceeds distributed in relation to district pupil/wealth ratios	956	974	.133	.132	.16	.35	.43	.54

Source: Calculation by authors from data supplied by H. Ladd. For description see Ladd (1975).

[a]Antilog of regression-predicted logarithm of expenditures; adjusted for residual variance of regression.

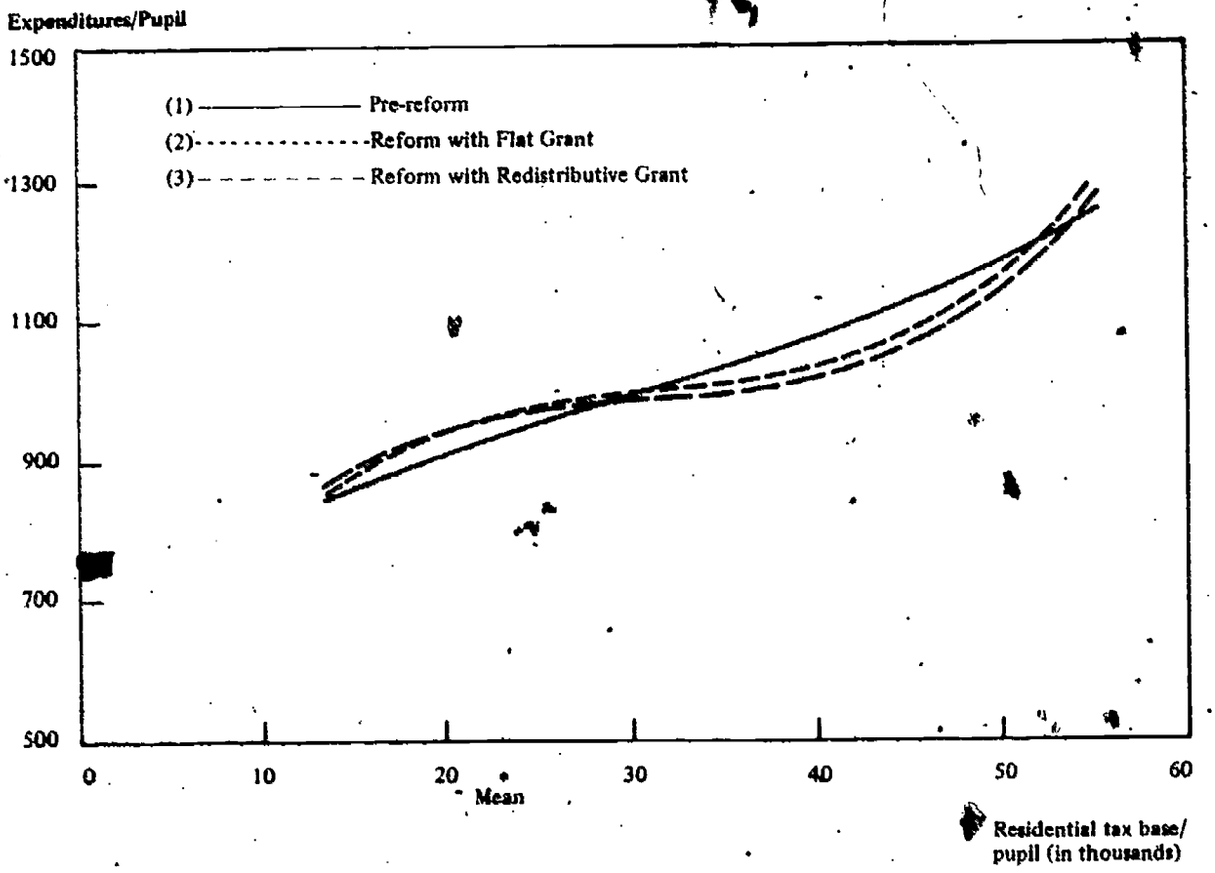
[b]"Unweighted" results use school districts as the unit of observation. "Weighted" results use students as the unit of observation and are comparable to our earlier results for Illinois.

However, the important test of simple neutrality (in this case of residential wealth neutrality) involves not the correlation of expenditures and wealth but the *slope* of the regression of expenditures on wealth. In figure 3 we have plotted predicted values of expenditures per ADA on district wealth per ADA for the Ladd sample (a) without her hypothetical reforms (line 1), (b) with her reform and "flat grant" redistribution (line 2), and (c) with her reform using concentrated redistribution of proceeds on low-wealth districts (line 3).[66] As can be seen from the figure, the redistributive formula shows a smaller slope in the region of mean residential tax base per pupil ($23,260). Relative to the situation prior to reform (line 1), the redistributive function (line 3) modestly increases expenditures for most students in the lower part of the wealth distribution and modestly reduces outlays for high wealth schools. While these results indicate that state assumption of the commercial and industrial tax base would not result in satisfying a simple neutrality test based on family wealth, it would improve the degree of simple neutrality.

Ladd's work (and the related research by Feldstein, Stern, and others) suggests that estimated demand functions and simulations based upon them can be a valuable tool in predicting the long-run consequences of school finance revisions for the distribution of school expenditures. We believe such work can be usefully extended. It should be recognized, however, that this type of research ignores

FIGURE 3

PREDICTED EXPENDITURES PER PUPIL GIVEN RESIDENTIAL TAX BASE IN SCHOOL DISTRICT, LADD SAMPLE



several important assumptions that are themselves appropriate objects of research. Among these are the assumptions that (1) improvements in the equality of distribution of inputs of the public schools are not offset by compensating purchases of privately provided education by families with children, (2) the changes do not significantly influence district population or choice of residence by families with children, and (3) the property tax base within school districts is not itself affected by reform. These effects are difficult to study, but some possibilities do deserve consideration. We turn now to a brief discussion of them.

The Substitution of Public and Non-Public Education

Generally, investigations of equality of opportunity in education have been restricted to comparisons of *public* school offerings. It is not readily apparent, apart from the availability of data, that this should be so. There are at least two distinct reasons why one might include non-public school offerings in examing changes in equality of opportunity: (1) a value judgment that equality of opportunity should refer to all children, not just those in public schools; and (2) the range, extent, and cost of non-public school offerings affects the degree of equity in public schools.

We will not dwell at length on the first reason. At this time, there has been no policy commitment concerning *resource* requirements or regulations for private schools. However, it is conceivable that this may change. For example, suppose one argued it is necessary that a certain child attend a private school in order to uphold religious principles. Does the exercise of religious freedom make it any less necessary or less important that the state ensures the child receive adequate educational resources? Whether public funds may or should be used for these purposes is a separate question. Both of these policy issues raise constitutional questions involving the separation of church and state.

The recent *Wolman v. Walter* decision of the U.S. Supreme Court is indicative of the awkwardness the separation issue causes: public funds may be used to directly aid these students but not for supporting the schools they attend. While this decision extends the eligibility of parochial school students for public funds, it does not require that public funds be provided. In the non-judicial branches of government, there does not seem to be any consensus about what should be provided. For example, the President's Commission on School Finance urged in 1972 that "local, State and Federal funds be used to provide, where constitutionally permissable, public benefits for nonpublic school children . . ."[67] However, the highly respected Fleischmann Commission report of 1972 concluded: "The principle of separation of church and state should not be abrogated: public funds or tax revenues ought not to be used in support of the attendance of students at sectarian schools."[68] In short, there does not appear to be any consensus value judgment about the appropriateness of ensuring equal opportunity for those children attending private schools.

The second reason is more subtle but has substantial implications for the financing reforms of public schools: offerings in nonpublic schools affect, and

are affected by, offerings in public schools. Let us consider first an extreme case to make the point, and then turn to more realistic examples. Suppose a state imposed a uniform expenditure level of $1,500 per child, to be financed locally with state subsidies to poorer districts. Suppose further that there is one wealthy district composed almost entirely of affluent families (with children) living in single family residences. The district has been spending $3,000 per child in their public schools. To avoid the state rule, the citizens of the wealthy district withdraw their children from the public schools and form private schools with tuition of $3,000 per child. The public schools in this district close (or operate on a token level), so local taxes are reduced. The children continue getting a $3,000 education, the wealthy parents pay the same amount for education as previously, and the state has achieved "equal opportunity" in its public schools.[69] However, there may be no real change in equal opportunity among children.

Of course, this represents an extreme case. But it should be clear that the effectiveness of equalization policies depends on the degree to which families will substitute private for public education (or supplement public education with extracurricular educational activities obtained from other public or private sources) in response to those policies.[70] The last example raised the possibility of the flight of the wealthy from public schools as policy restricted their spending. An opposite effect might occur in the less wealthy districts, where spending on public schools is substantially increased. That is, some families in the less wealthy districts who have been sending their children to private schools, now might feel that reform has so increased the quality of their public schools that their children can no longer forego this opportunity.

Some data on enrollments in California private schools from 1973-75 are suggestive of this latter point. In California during this period, expenditures per child in the lower wealth districts were growing more rapidly than expenditures in the higher wealth districts, due to increased subsidies to the former and revenue limits on the latter (which can be overridden by local vote, but still increase the difficulty of spending more). Total private school enrollments increased from 386,974 to 404,079 (an increase of 4.4%), but enrollments in church-affiliated schools decreased from 324,143 to 319,880 (a decrease of 1.3%).[71] The church-affiliated schools, which in 1973 had 80% of all the California children attending private school, generally struggle to provide even minimum educational resources to their pupils (that is, they are not wealthy schools nor are the children in them primarily from wealthy families). As expenditures in the public schools near them rise, it should not be surprising if many parents are induced to switch from the private school to the public school. The opposite is true at the other end of the spectrum. Enrollments in the non-church affiliated private schools during the same period *increased* from 62,831 to 84,199, or an increase of 34 percent. As a group, these schools represent the more expensive private schools, and they become more attractive as expenditures in relatively high spending public schools are constrained (for example, by failure to override revenue limits).

We have indicated that the above data are only suggestive. There are, of course, many other possible reasons to explain why enrollment in the church-

affiliated schools has declined and in other private schools has risen: perhaps the former is entirely due to a decline in religious preferences, and the latter due to attempts to avoid desegregation.[72] More careful research on this matter is necessary to sort out the independent impact of public school expenditure changes on private school enrollment.

There has been some empirical research which looks at the effect of private school enrollments on public school spending. However, none of these studies distinguish enrollments in minimum expenditure schools attended primarily for religious reasons, and enrollments in the more expensive private schools. In these studies, cause and effect is thought to run from use of private schools to public expenditures, rather than vice versa. Weisbrod, using the 48 adjacent states as the units of observation in a study based on 1960 data, found a positive association between the percent of pupils enrolled in public schools and public school expenditures.[73] He attributed this result to the reluctance of non-public school children to support public education. However, the association could also reflect a greater propensity to use public rather than private schools because public school expenditures are higher. In a recent re-examination of Weisbrod's model, Greene found a negative but not significant association between these two variables. Greene's units of observation were 53 school district-*cities* in 1970 in New York State.[74] He suggested that this result might be expected because the smaller the percentage of families using non-public schools, the greater the burden of public school finance on the families of children attending public schools (making them less likely to spend more money than if others had to pick up a bigger part of the tab). However, this might be better explained by variation in religious composition of the districts, reflecting different propensities to use public schools. Finally, Feldstein included a variable measuring the number of private school students per capita in his cross-section study of Massachusetts school districts, but did not find any stable association between public expenditures and this variable (using several different estimation procedures, the sign varied). The relationship was significant (>2x standard error) and negative in three equations using 1970 data, but significant and *positive* when the same sample was restricted to only those districts receiving matching state grants (i.e., eliminating the highest wealth districts). The change in sign as the high wealth districts are added may reflect the quite different (non-parochial) purposes which induce families in high wealth districts to utilize private schools (implying, other things equal, a lower propensity to use private schools).

While the findings of these studies could be quite consistent with our hypothesis, none of them were designed to specifically test the effect on demand for private education caused by expenditure changes in public education. We attempted to construct a rough but more direct test by combining our Illinois public school data with private school enrollment data from other sources[75] for towns within unit districts. The idea was to explain the change in private school enrollment per district as a simple function of general change in district enrollments and change in public school expenditures per ADA. However, after eliminating all Cook County schools because of possible confoundment with school deseg-

regation efforts, and a few districts due to incomplete observations on private schools, we were left with only eight usable observations characterized by little variation in expenditure change. This strongly suggests that reasonable tests of this hypothesis should be made from a more detailed[76] sample of *families* exposed to different public-private trade-offs for the education of their children.

We also note that the shift from public to private school is a costly and discontinuous adaptation of families to perceived inadequacies in the public schools. It is possibly more common for families to adjust to such developments by purchasing or in other ways providing education from non-public school sources as a supplement to what is received in the public schools. The extent of such activities, their association with variations in public school expenditures, and changes in these factors over time are difficult to analyze because data are hard to come by. But any information on this front would be valuable in assessing the implication of change in school finance for the total supply and distribution of education received by children.

In sum, the opportunities available to children through private schools may bear significantly on public school enrollment, as well as on the quality of educational opportunity generally. Because school finance reforms change the trade-offs families face in the public-private enrollment decisions and incentives for supplementation of public school offerings, they will induce a behavioral response which could advance or hinder equal opportunity goals. More empirical research on this issue is necessary to establish the magnitude of these effects.

Locational Responses to School Finance Reforms

In most of the literature on school finance reform, little attention is paid to the possible changes in residential as well as business locational patterns which might be induced by the reforms. Changes in these patterns, while important by themselves, would cause changes in property values, in the wealth of districts, in the tax rate choices of districts, and, therefore, in the equality of educational opportunity available to children. However, the *Serrano* court refers, for example, to the variations in property wealth across districts as "fortuitous." The economic models developed to predict tax rate changes as a consequence of reform (cited earlier) also assume relative property wealth is constant—an assumption appropriate for near-term responses. But in the long run, the location of wealth is neither fortuitous nor fixed. Furthermore, there is a class of economic models—the Tiebout models—which can be used to generate empirically testable propositions concerning the consequences of school finance reform.[77] Since the consequences of these long-run effects can be both profound and counter-intuitive, perhaps overshadowing the near-term effects, we think it quite important that more effort be devoted to understanding them. In this section, we present a rudimentary bare-bones model of residential location to illustrate these effects.

Tiebout models revolve around the notion that households vote with their feet. In any region, there are numerous communities (or school districts) in which households can locate. Each community has its own distinctive features which make it more or less attractive as a residence: e.g., distance from employment centers, air quality, the quality of its public schools and other local services, and its tax rate. Households compete for locations, causing land prices to be high in the more popular areas and lower elsewhere. Local governments, in determining local services, are assumed to be responsive to the preferences of their residents. Tiebout argued that, under certain other assumptions including perfect household mobility, an optimal provision of local services would result from this locational process. However, here it is only the determinants of household location and the resulting groupings which interest us.

School finance reforms like district power equalizing change the local tax rate necessary to raise a given level of educational expenditures; in some communities it is raised and in others it is lowered. This will change the relative attractiveness of communities to households, who increase their bids to live in those areas favored and reduce them elsewhere. A regrouping of households results; the regrouped communities may prefer different levels of local services than those offered previously.

While there is controversy in the economics literature about how "optimal" the Tiebout process is in the actual world of limited mobility, few doubt that locational choices are sensitive to the quality and cost of local government services, particularly education.[78] Newacheck estimated, for example, that in California in 1972 a $1.00 increase in annual educational expenditures per child would result in a $15.50 increase in home value, while a $1.00 increase in the property tax rate would lead to a $1700 decline in home value.[79] Using this to simulate the capitalization effect on a $30,000 home of switching to a district power equalizing system (with state aid constant), he estimated gains or losses of up to $5000 in extreme districts, with the majority of districts experiencing gains or losses in the $1000-$3000 range. If the magnitude of these estimates is correct,[80] then school finance reform may generate substantial regrouping of households over time.

To illustrate the possible nature of the regrouping that might arise in switching from a local finance system to one of district power equalization, we develop a very simple model.[81] Assume we are in a region divided into K districts where each family has one child attending public school. Any family can be characterized by its wealth w and its preference for education, a. The family preference function is assumed to be Cobb-Douglas, where utility comes from the educational expenditures on its child and its after-tax wealth. We assume our population consists of many different families with varying tastes for education and varying wealth.

It is useful to begin with the simplest problems. If each family was a complete school district, then they would choose an education tax rate t to:

$$\underset{t}{\text{Maximize}}\ u[tw,(1-t)w] = (tw)^a[(1-t)w]^{1-a} \qquad (1)$$

The solution to this problem is to choose $t = a$. To see this, first note that we can simplify the above expression:

$$\underset{t}{\text{Maximize}}\ w[t^a(1-t)^{1-a}] \tag{1a}$$

Then

$$\partial u/\partial t = w[at^{a-1}(1-t)^{1-a} - (1-a)t^a(1-t)^{-a}] = 0$$

and since the w drops out,

$$at^{a-1}(1-t)^{1-a} = (1-a)t^a(1-t)^{-a}$$

$$\text{or } a/(1-a) = t/(1-t)$$

$$\therefore \text{ or } t = a$$

Now let us complicate the problem slightly by assuming there are N families living in one district, and they must collectively decide on the district tax rate and expenditures per child. We assume this decision is made by the median voter rule.[82] Since average wealth in the district is

$$\bar{w} = \sum_{i=1}^{N} w_i/N,$$

then each family i will seek to:

$$\underset{t}{\text{Maximize}}\ (t\bar{w})^a[(1-t)w_i]^{1-a} \tag{2}$$

Note we can rewrite this slightly as

$$\underset{t}{\text{Maximize}}\ (\bar{w}^a w_i^{1-a})t^a(1-t)^{1-a} \tag{2a}$$

Since the first term in parentheses does not depend on t, the solution only depends on the last two terms. But these are identical to those in (1a), so the solution is identical: $t = a$ is the most desired tax rate for a family in the district. The choice does not depend on the wealth of the district nor the wealth of the family. Let $\bar{a}$ be the median preference for education among the families in the district; then the district will choose $t = \bar{a}$ by the median voter rule. Note the education expenditures a child received, $a\bar{w}$, clearly depend on district wealth; but the choice of that expenditure level depends only on family preferences.

Now let us relax the assumption that there is only one district, and assume there are K ($K<N$). Families are free to locate in any of the K districts. Each district will choose a tax rate $t = \bar{a}_k$, from the result in (2) above. In equilibrium, when families have no incentive to relocate, it must be that for any family of preference a in district j:[83]

$$U_a(\bar{a}_j\bar{w}_j, (1-\bar{a}_j)w) \geq U_a(\bar{a}_k\bar{w}_k, (1-\bar{a}_k)w) \tag{3}$$

$$\text{for all } k = 1,2,\ldots,K$$

Each family will settle in a district only when its utility there is at least equal to the utility it could get in any other district.

Rewriting expression (3) under our assumptions about preferences, the condition is:

$$(\bar{a}_j\bar{w}_j)^a[(1-\bar{a}_j)w]^{1-a} \geq (\bar{a}_k\bar{w}_k)^a[(1-\bar{a}_k)w]^{1-a} \tag{3a}$$

Notice in the above equation that individual wealth, w, drops out. This implies that families with the same preferences, independent of their wealth, will be satisfied in the same districts. If any one family of preference type a strictly prefers a district j, then all families of preference type a must be in it. The only way families of similar preference types can choose different districts is when they are indifferent among them. If there is only *one* type of family indifferent between two districts, we conjecture this implies the rest sort by preference *intensity*: each district will be composed of families whose preferences (a's) are similar, but not necessarily identical. However, suppose we consider the possibility that families of different preference types can be indifferent between the same districts: in particular, that there are *two* family types, a and a', each indifferent between district j and district k. Then

$$(\tilde{a}_j \bar{w}_j)^a (1-\tilde{a}_j)^{1-a} = (\tilde{a}_k \bar{w}_k)^a (1-\tilde{a}_k)^{1-a} \tag{4a}$$

and

$$(\tilde{a}_j \bar{w}_j)^{a'} (1-\tilde{a}_j)^{1-a'} = (\tilde{a}_k \bar{w}_k)^{a'} (1-\tilde{a}_k)^{1-a'} \tag{4b}$$

By dividing these two equations:

$$[\tilde{a}_j \bar{w}_j/(1-\tilde{a}_j)]^{a-a'} = [\tilde{a}_k \bar{w}_k/(1-\tilde{a}_k)]^{a-a'} \tag{5}$$

or $$[\tilde{a}_j/(1-\tilde{a}_j)]\bar{w}_j = [\tilde{a}_k/(1-\tilde{a}_k)]\bar{w}_k \tag{5a}$$

This implies (for $0<\tilde{a}_j, \tilde{a}_k<1$):

$$\text{if} \quad \tilde{a}_j > \tilde{a}_k \quad \text{then} \quad \bar{w}_j < \bar{w}_k \tag{6}$$

$$\text{if} \quad \tilde{a}_j < \tilde{a}_k \quad \text{then} \quad \bar{w}_j > \bar{w}_k$$

In words, this "preference scattering" can only occur if the district with the higher tax rate has lower wealth. Even if there is a correlation in the general population between preferences and wealth (either positive or negative), the possibility of the "scattered" equilibrium suggests some families of unequal wealth may group together (reducing wealth and expenditure inequality across districts).

However, under district power equalizing this possibility disappears. Under this policy, educational expenditures depend on the tax rate choice and the state standard $\bar{w}$. The equilibrium condition is

$$(\tilde{a}_j \bar{w})^a (1-\tilde{a}_j)^{1-a} \geq (\tilde{a}_k \bar{w})^a (1-\tilde{a}_k)^{1-a} \tag{7}$$

$$\text{for all } k = 1,2,\ldots,K$$

Here the $\bar{w}$ cancels out. As before, families sort out by preferences. If we check for the possibility of preference scattering, we find:

$$[\tilde{a}_j/(1-\tilde{a}_j)]^{a-a'} = [\tilde{a}_k/(1-\tilde{a}_k)]^{a-a'} \tag{7a}$$

or $$\tilde{a}_j/(1-\tilde{a}_j) = \tilde{a}_k/(1-\tilde{a}_k)$$

or $$\therefore \tilde{a}_j = \tilde{a}_k$$

That is, preference scattering can only occur in districts choosing the same tax rate, but in equilibrium these districts must have the same wealth as well. Therefore, they are not really different. Under district power-equalizing, there is no incentive to deviate from unscattered preference grouping. This may result in greater district inequalities in wealth and expenditures.

Of course, this model is not fully developed, and its policy implications (that district power equalization could increase inequalities) may disappear in a more realistic version. However, we believe the development of these types of models, and then the empirical testing of them, should be encouraged.

Summary and Assessment of Research Directions

In this chapter, we have attempted to illustrate a variety of procedures for understanding the equity consequences of school finance reform, and to point out methodological difficulties in applying the procedures. In order to keep our task manageable, we focused on a very limited number of concepts of equal opportunity. However the techniques of analysis we illustrated are applicable to a broad range of concepts.

We first suggested that there has not been sufficient logical analysis of the interrelationships between different concepts of equal opportunity and the school and finance systems on which they are imposed. We drew a distinction between concepts of *division*, which do not depend on *who* receives the shares of educational opportunity being discussed, and concepts of the *distribution process*, which focus on whether differences in shares arise from a fair process. As a broad generalization, we find that research on educational equity has focused primarily on investigations of division, while policy has focused on the distribution process. That is, while researchers have pointed out differences in shares among various [illegible], there has not been enough thought given to the construction of a conceptual framework for analyzing the conditions under which these differences might be thought fair. We tried to illustrate this by developing (among others) the concepts of *simple* and *conditional neutrality* and applying them to the *Serrano* case. It is clear that the *Serrano* court requires both, which is logically impossible in school finance systems allowing local choice from among districts of varying wealth. The policy implications of each concept are quite different, and the recent *Serrano II* decision seems to give priority to the simple neutrality requirement. This implies that policies like district power equalization may not satisfy the courts if they do not work as their designers intended (a likely possibility based on all existing empirical economic research).

We then turned our attention to empirical questions concerning the effect of school finance reforms on the equality of educational opportunity. Using Illinois as a case study, we constructed empirical tests of simple and conditional neutrality (as well as of the equality of division) and applied them both before and after the 1973 Hoffman-Fawell reform. Our results indicate (subject to a number of qualifications in the text) that the Illinois pre-reform system failed

both tests, that the reform improves the neutrality of the system by both tests but not the division of expenditures, and that the post-reform system fails the conditional neutrality test but *satisfies* the simple neutrality requirement. We believe that this type of investigation is important, and should be replicated for other reforms and bodies of data.

While the above empirical work is important both for assessing the degree of existing equal opportunity and the near-term impact of reforms, it does not attempt to address the longer-run consequences of reform. These longer-run consequences, which may be substantial and quite different from the effects observed in the near-term, depend on a variety of behavioral responses of households and firms. Reasonable prediction of these responses (and their consequences) depends on careful econometric investigation. We reviewed the work of Professor Helen Ladd as an illustration of one type of research we would like to encourage. Ladd attempted to estimate the determinants of voter behavior in Massachusetts in regard to local tax rate choice; and then to simulate the effect of school finance reforms involving the removal of commercial and industrial property from the local tax base. When we applied a simple neutrality test to this simulation, we found that these reforms would improve the degree of simple neutrality with respect to residential wealth.

We note a reform such as that investigated by Ladd can have substantial implications on the locational patterns of firms; all reforms that substantially alter educational expenditures and tax-prices can have substantial implications on household residential patterns as well as the use of private schools. To illustrate the potential for research in these areas, we began to develop a model of household locational choice to examine the implications of various financing reforms on locational patterns. This model is too rudimentary to allow any conclusions to be drawn from it; however, we would encourage further development of such models. We also suggest that some empirical research be undertaken to learn more about the educational opportunities available in private schools and the relationship between the use of private schools and school finance reform.

We hope that this review, and our illustrative efforts to learn about the equity consequences of school finance reforms, will induce others to pursue what we believe to be a tremendously important and fertile area for further research.

References

1. For some additional concepts of equal opportunity in education, see Kirp and Yudof [1974], Chapter 6; Jencks *et al.* [1972]; and Levin [1973].

2. In fiscal year 1976, we estimate that $60 billion was spent just on public elementary and secondary education.

3. For example, Arrow has shown that a policy of equal inputs to individuals of differing abilities is necessarily output-regressive. See Arrow [1971].

4. For a review of the issues involved in the construction of education cost indices, see W. Norton Grubb, "Cost of Education Indices: Issues and Methods," in Callahan and Wilken [1976], pp. 87-93.

5. For a fairly comprehensive review of recent reforms, see Callahan and Wilken [1976].

6. This argument is discussed in greater detail in Friedman [1977].

7. By "shares," we mean any interesting measure of what is being distributed. We will usually use "expenditures per child" as a convenient example (since that is what the *Serrano* court used), though for other purposes one might want to use "skills," or "future income," or "teacher attention," etc. By definition, the quality of shares is homogeneous. If one is interested only in good teachers, then other teachers are not part of the shares to be distributed.

8. In the 1973 *Rodriguez* decision of the U.S. Supreme Court, the justices rejected a *Serrano*-like argument. However, in that ruling, they did leave open the possibility that a claim for a certain *minimum* education may be a fundamental right protected by the federal constitution.

9. Empirical examples from our case study of Illinois are presented in the [illegible] section. Berne [1977] presents a good synthesis of general empirical and conceptual problems related to measuring the concept of divisional equity in education. He mentions, but does not focus on, the distributional problems we emphasize.

10. The reason for use of groups in this test is interesting in and of itself. In most educational situations, we simply cannot test for equality of distribution on an individual basis. To do so would require multiple "drawings," and, after sufficient lotteries, a comparison of the distribution of draws for each child. Things do not work this way in practice; each individual child gets only one chance. As a result, we are forced to fall back on tests which treat draws by a number of children as a substitute for multiple tests on one.

11. While we intend a connection between the "suspect" groups that we will use and those classifications termed "suspect" as a matter of law, we presume that our rather arbitrary criteria for awarding a group that label are much less stringent than those a court would require. For example, in the law, suspect classifications involve state actions.

12. *Serrano v. Priest*, 5 Cal. 3d 584 (1971).

13. See "Memorandum Opinion Re Intended Decision," *Serrano v. Priest*, Superior Court of California, County of Los Angeles, No. 938, 254, April 10, 1974; and "Findings of Fact and Conclusions of Law," August 30, 1974.

14. *Serrano v. Priest*, 18 Cal. 3d 728 (1976).

15. *Serrano I*.

16. See Browning and Long [1974].

17. See note 8.

18. Coons, Clune, and Sugarman [1970].

19. See for example, Reischauer and Hartman, 1973; Feldstein, 1975; Grubb and Michelson, 1974; Stern, 1972; Friedman, 1977.

20. Coons [1977], p. 33.

21. Conclusion of Law #88, p. 59.

22. Conclusion of Law #115, pp. 64-65.

23. In the provision of local services generally, taxpayers may wish to ensure that all localities can provide minimum essential services. However, it is usually more efficient and more equitable to provide these funds from taxes on general wealth rather than from taxes on their own local services (for wealthy districts, district power equalizing is a state tax on their local spending). Also, since the incidence of the property tax is still an unsettled economic question, we neither know who is bearing the burden of the status quo nor who would benefit or lose from the change.

24. This can be done, however, through a matching grant system. See Feldstein [1975]; and Friedman [1976]. See also our empirical results in the next section for the Illinois school finance reform.

25. *Serrano II*, pp. 23-24.

26. *Serrano II*, p. 44, no. 35.

27. *Serrano II*, pp. 25-26. They continue in this passage to reaffirm that equal expenditures per pupil are not required. For other examples that the court's concern with tax rates is subsidiary to its concern for expenditure disparities, see pp. 20, 25-26, and pp. 46-48, no. 38, in the decision.

28. For a more thorough review of the court cases mentioned in this section, see Kirp [1977].

29. 327 F.Supp. 844 (D.C. 1971).

30. See Kirp and Yudof [1974].

31. Callahan and Wilken [1976].

32. 348 F.Supp. 866 (D.D.C. 1972).

33. Our information on the *Wolman* case is based on an article in the *Los Angeles Times* the morning after the U.S. Supreme Court decision on June 24, 1977.

34. 414 U.S. 564 (1974).

35. See Callahan and Wilken [1976].

36. For details, see Hickrod *et al.* [1975] and Yan [1975].

37. WADA = .5 x average daily attendance of children + 1.0 x average daily attendance of students in grades 1 to 8 + 1.25 x average daily attendance in high schools.

38. Henceforth, the familiar "ADA" is used in place of "WADA." All figures are composition-adjusted, using Illinois weights. These numbers are calculated from estimated revenues and aid entitlements and may deviate from actual district expenditures by small amounts. Dr. Thomas W. C. Yang of the Department of Educational Administration of Illinois State University provided these and all other Illinois data used here. We gratefully acknowledge help provided by Dr. Yang and Dr. G. Alan Hickrod. Their assistance vastly exceeded the necessities of scholarly courtesy. Errors in interpretation of these data are our own.

39. Decile divisions for this and subsequent tables are only approximate. In all cases, decile divisions evaluated on the bases of weighted average daily attendance (see note 38, *supra*) fell within school districts. Rather than subdivide students within schools, "decile" divisions were made only in moving from one district to the next. Thus, in table 1, the first decile actually covers the lower 10.1% of total WADA, the fifth decile includes only 9.8% of all WADA, and the last decile actually includes 9.4% of all WADA. This computational simplification is unlikely to influence the results significantly.

40. For recent evidence, see Horvath [1976] and Levy, Meltsner, and Wildavsky [1974].

41. The matter of cost compensation can be treated as a problem of creation of a price index for education services; for discussion see Grubb, *op. cit.* in Callahan and Wilken [1976]. In general, available research indicates that categorical grants are largely translated into expenditures; see Ladd [1975].

42. The coefficient of variation of expenditures per ADA is the ratio of the standard deviation of that variable to its mean. Its value is zero when there is perfect divisional equality among students.

43. See Berne [1977] and the Hickrod and Yang papers already cited.

44. See Berne [1977].

45. This interpretation is the conventional wisdom. However, it is possible that courts would not insist on restricting the educational expenditures of the property rich district. A guaranteed tax base for property poor districts may be thought sufficient.

46. More precisely, expected expenditures must be the same. This is not a requirement, in the limit, for equal division; the requirement is for statistically random distribution around the average expenditure level.

47. The relation between expenditures per student and tax base per student has typically been described in terms of a simple correlation or the elasticity of expenditures per student with respect to assessed valuation in the student's school district. Neither measure seems sufficiently descriptive. High correlation can be achieved when, in fact, the number of dollars difference in expenditure per student between low-wealth and high-wealth districts is very small. The assumption of constant elasticity (implied when a functional relationship between expenditures and wealth that is linear in logarithms is estimated) has no apparent rationale other than the fact that elasticities are conveniently unit-independent and are readily compared across states, even in instances when the absolute dollar levels of expenditure are quite different. Since such considerations do not appear persuasive, the relationship between expenditures and wealth is probably better demonstrated with a table or with a linear regression based on a more general functional form.

48. In calculating the coefficients for this relation, it is assumed that each year's data can be treated as an "experiment," in which a school finance system like Illinois' is imposed upon a set

of 435 school districts and the resulting relation between district wealth and expenditure is observed. The residual in this regression is the product of random influences on expenditures other than wealth and its correlates. Under these assumptions, it is meaningful to perform an "F" test of the significance of the calculated relationship, since presumably, even with no persistent relation between school district wealth and expenditures, annual "experiments" might produce data that revealed one. For these data, the odds that the relation detected is spurious are negligible. Largely as a result of our assumption of intradistrict homogeneity of expenditure, the "t-statistics" for all the coefficients in equation (1) exceed 20. However, the true precision of estimation is lower because this assumption must lead to understatement of total variation in the inputs students receive.

49. While the ESEA measure used above involves a type of income classification, for some purposes it would be useful to evaluate the relation between educational expenditures and the entire range of the income distribution. Conceptually, such a test would be carried out with procedures like those adopted for analysis of the relation between expenditures and school district wealth. The significant barrier to such evaluation concerns the quality of income data available on a school district basis. The only source of such numbers is the decennial census. The census data tend to be unreliable because of undercounting of income, the long interval between censuses, and distortions generated by the difficulty of controlling for family composition in assessing district-to-district income variations. ESEA eligibles estimates are based in part on census income data and are subject to these same flaws.

The alternative to use of census data is to search for proxies for average income in a community or the number of families in poverty that are regularly evaluated. Since residential property value tends to be correlated with family income, residential assessed valuation per student provides one proxy measure of community income status. This measure is subject to the same demographic distortions as is true for census income figures but possesses the exceptional virtue of being readily measurable in most districts.

50. The deficiencies of the Title 1 eligibles estimation procedures are well known and are the object of other research. See U.S. Department of HEW [1976], pp. 46-50, for a discussion of these techniques and the congressional mandate for improvement.

51. For a discussion of the relation between various family income measures and other school district characteristics in Illinois, see Illinois State Board of Education [1977].

52. These data must be treated with caution. We do not possess a breakdown of ESEA eligibles by grade level. Hence, these comparisons are calculated on the basis of ADA. The outcome of correctly weighted calculations is unknown.

53. Assuming no spillovers to non-ESEA students.

54. This is the rate which, when applied to aggregate assessed property value in unit districts, would produce the same total local revenue as was collected in 1972-73.

55. See Newacheck [1976] for a discussion of capitalization effects.

56. Note that matching grants, when initiated, involve both price and income effects. Without compensating changes in other transfers, not only will such grants lower the price of education as perceived by voters, but they will also increase income in the sense that the district can buy all the services it did before the reform at a lower total cost. This releases resources for expenditures on other public or private goods.

57. See Illinois Office of the Superintendent of Public Instruction [1973] for a complete description.

58. "Title 1 weighted average daily attendance (TWADA)" under the reform plan is defined by the following formula:

$$\text{TWADA} = \text{WADA} + .375\,(\text{number of district Title 1 eligibles}) \times \min\left\{2.0, \left[\frac{\text{No. of Title 1 eligibles}}{\text{WADA}}\right] \Big/ \left[\begin{array}{c}\text{state average}\\ \text{ratio of Title 1}\\ \text{eligibles to WADA}\end{array}\right]\right\},$$

where WADA = district average daily attendance adjusted for grade composition and min (X,Y) = X if $X \leq Y$ or Y if $Y < X$. See Illinois Office of the Superintendent of Public Instruction [1973], p. 52.

59. The numbers in parentheses are t-statistics.

60. Should the Title 1 weighting scheme used in Illinois be judged to appropriately account for "exceptional characteristics," a "conditional neutrality" test would call for calculation of dispersion of expenditures assuming equal tax rates *and* equal proportions of Title 1 eligibles.

61. See, for example, Benson, *et al.* [1972].

62. See Ladd [1975] and [1976]. Both studies are derived from Professor Ladd's dissertation; see Ladd [1974].

63. The grant effect on prices is measured by the quantity one minus the matching rate for state-provided aid. If the state grants the district $.50 for every dollar raised locally for school expenditures, Ladd assumes the price of a dollar's more education as perceived by voters is $.67. The composition effect on prices is measured by the quantity one minus the share of the local tax base attributable to non-residential property. If 25% of the local tax base is non-residential in character, and in the extreme case in which all non-residential taxes are shifted either to persons outside the district or to small groups which do not include the median voter, an additional $1 in education costs the median voter-taxpayer only $(1 - .25)x\ \$1 = \$.75$ more in taxes. Ladd's actual price effect allows for taxpayer behavior corresponding to an assumption of less than complete shifting and separate incidence effects for commercial and industrial property.

65. We are grateful to Professor Ladd for providing these data. Her simulation results were not reported in a form that permits easy comparison with measures of equality such as those employed for analysis of the Illinois data. Her measures of equality included only the coefficient of variation of expenditures per student and for evaluation of neutrality she employed simple correlation of expenditures with district residential wealth, median family income, and the proportion of families in poverty. In the original article, all of these calculations were done on a per district, rather than a per student basis. While the unweighted measures in table 9 are identical in concept to those reported by Ladd [1976], some of our results differ slightly from those she originally published due to corrections (by Ladd) in the data.

66. The regressions used for this purpose are of the same form as those estimated using the Illinois data. The exact functions and related statistical data are available on request from the authors.

67. The President's Commission on School Finance [1972], p. 56.

68. New York State Commission on the Quality, Cost and Financing of Elementary and Secondary Education [1972], Vol. 1, p. 53.

69. A similar example is provided in Reischauer and Hartman [1973] p. 86.

70. For one theoretical treatment of this issue, see Stiglitz [1974].

71. California State Department of Education [1975], Table 1.

72. For a more extended discussion of private school economics, see Reischauer and Hartman [1973], Chapter 5.

73. Weisbrod [1965].

74. Greene [1977].

75. *The Handbook of Private Schools* [various years]; *Patterson's American Education* [various years]; and *Private Independent Schools* [various years].

76. The details should include demographic and socioeconomic characteristics of the families, and an indicator of the family preference for a sectarian education, as well as school and district characteristics.

77. Tiebout [1956].

78. See Oates [1969]; Buchanan and Goetz [1972]; Edel and Sclar [1974].

79. Newacheck [1976].

80. Calculations of this type are hazardous because they are based on "all other things equal" assumptions. The Newacheck analysis assumes that tax rates and expenditures in all competing locations remain unchanged. Should district power equalization or any other major reform be introduced, many districts would be changing tax rates and expenditures. Under these circumstances the net consequences for homes in particular construction-cost classes become somewhat more ambiguous than the single-equation technique suggests.

81. We are indebted to Bart McGuire for his assistance in the formalization of this model.

One interesting model of this type exists in the literature; however, that model does not focus on the role of preferences. See Aronson and Schwartz [1973].

82. Imagine a series of votes on tax rates of increasing size, where each family votes in favor of the increase as long as it is less than or equal to its optimal tax rate. The process ends where a majority will not vote to continue, which is at the point where the tax rate reaches the level of the median voter.

83. We assume that any one family's impact on district wealth and tax choice is negligible.

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A Research Agenda

Appendix to "Toward Understanding the Equity Consequences of School Finance Reform"
by Lee S. Friedman and Michael Wiseman

Our paper was organized around three propositions about research needs in the area of educational equity: (1) More conceptual work would be useful to clarify diverse concepts of equal opportunity and to recognize relationships and inconsistencies among them; (2) little is known about how equality of educational opportunity, under any definition, has changed in response to school reforms; and (3) there should be more predictive theoretical modeling and empirical testing of hypotheses about the behavioral responses of school districts, firms, and families to school finance reform. In this appendix we list a research agenda implied from our arguments in each of these areas.

Conceptual Issues

- N.I.E. should fund a comprehensive review of the use and justification for categorical assistance programs for state aid to local school districts.

We concentrated most of our effort within the paper on the task of clarification of the various concepts of equity. If the taxonomy we have developed is adequate, then the important remaining conceptual issues arise in application of the concepts to real systems. With regard to immediate research needs, we believe the most pressing to involve the definition of what constitute categorical, as opposed to general purpose, assistance and expenditures in schools.

There is an increasing trend toward the use of categorical funds in school finance. This development is sometimes justified by reference to an output-oriented standard of equity, as in the *Lau* case. However, no objective basis for determining the "fairness" of the amounts so distributed seems to be available. Since in applying standard measures of input equity to school statistics such funds are generally excluded (they are associated with "exceptional characteristics"), this potentially gives legislatures a court-acceptable procedure for achieving any desired distribution of inputs, regardless of equal opportunity requirements. It is important that some logical basis be established to insure that equal opportunity goals can be met despite this source of potential obfuscation. This requires development of a standard for determining what funds can and cannot be legitimately classed as categorical. Categorical assistance is oriented both toward compensation of school districts for exceptional input costs (for higher costs due, for example, to student transportation requirements) and because of requirements for exceptional inputs (as for bilingual education in the *Lau* case). This distinction should be maintained in the study. The matter of input costs is closely related to the need for development of cost indices for equity evaluation; this is treated separately below.

The State of Equality of Opportunity

- N.I.E. should support intensive evaluation of the equity of school finance in a variety of states. These reviews should share the framework introduced in our paper, but should refine the methodology to take account of superior information on state finance programs and local school district characteristics.

One large-scale multiple-state comparison of equity developments in school finance should be undertaken soon, to focus on the near-term responses to different reforms (including no reform). In addition to this, probably most is to be gained by funding intensive study of individual state systems by knowledgeable persons connected with finance procedures at such locations. To be feasible, the multi-state project will involve primarily analysis of equity and behavioral consequences of school finance reforms on the basis of official descriptions of state aid procedures and the usual kind of district by district expenditure data that fits on computer tapes. More detailed study of selected state systems might provide better data on the nature of district-to-district cost variations, the operation of state categorical aid programs, district-to-district accuracy of property tax base assessments and other factors which will influence equity consequences of school finance reform and cannot be intensively investigated in a multistate study. Since such studies are intended to be complementary, individual state proposals should detail the ways in which the product will enhance what will be gained from multistate study. We would favor proposals which promise specific contributions in the area of (a) collection of better data on school district characteristics likely to influence spending behavior (recall the lack of data on the composition of the tax base in Illinois schools), (b) construction of a detailed cost index for schools, (c) collection of better data on actual allocation of expenditures. Attention should not be limited to states which have undertaken significant reforms; those states which have not done so might provide some insight into what is happening without legislative or court impetus.

- N.I.E. should continue to encourage improvement of the Title 1 eligibles count or any equivalent measure of the incomes of the families of school district children.

We have shown that the Title 1 count may be useful for simple neutrality tests of school expenditure equity. To this end (and for a variety of others), the count is usefully improved. One possibility in this direction is linking of data from the food stamp program to the Title 1 eligibles estimation process. The food stamp program potentially provides all the necessary ingredients for a reliable and up-to-date Title 1 count: (a) reporting of family income, size, and composition under circumstances more conducive to accuracy than the census, (b) reporting of family address, and (c) increasingly comprehensive coverage of the poor population. If the school district of children in food stamp households was recorded upon family registration and updated on an annual basis, such data may be a superior basis for allocation of federal categorical aid and for income-based simple neutrality evaluations than that currently available. Use of such data would provide an important local incentive to encourage eligible families to participate in the system. Food stamps have the advantage of being available to two-parent

families in all states. This is not true of payments under the A.F.D.C. program, part of the base for the current Title 1 eligibles count.

N.I.E. should be alert to possibilities for improving the data on which national aid allocation and equity evaluation procedures are based which arise in the course of construction of a new national welfare system.

- N.I.E. should support research on the disposition of funds made available through both categorical and general purpose grants.

Considering categorical funds first, it should be understood that for funds allocated on the basis of input price differences across districts, the two research questions of importance are (a) is there an empirical justification for the allocation procedures for such funds (i.e., do such cost differences actually exist), and (b) do expenditures in districts with exceptional costs exceed those in districts without. Both questions are matters that should be taken up in the state studies already proposed, and research in this area should probably be funded in that context. The appropriate test of (b) concerns district expenditures on the item with exceptional cost. If the object of the categorical aid program is to equalize input prices, then the effect of such aid should be to make the "price elasticity" of district expenditures on such items *unity*. That is, if transportation costs per student are 5% greater in District *A* than in district *B*, then expenditures on transportation should be 5% higher in *A* and expenditures on everything else no lower. Again, we emphasize that analysis of use and consequences of categorical assistance for input cost variations begins with verification and measurement of such differentials.

For categorical assistance made available to cover increased inputs for certain students, the issue of justification seems empirically almost intractable. At this time research money is probably better spent determining whether or not such funds are actually spent on the intended recipients. In cases in which categorical funds for purchase of additional inputs are "new money," evaluation must be made of the intention of legislators in making such funds available. Were they to be used to increase compensatory inputs over and above what districts would have provided in their absence, or were they to remove the financial burden of such expenditures from the local tax base? In other words, for all such programs an important research question concerns the degree to which the intended recipients actually receive the exceptional inputs the categorical funds are intended to provide. However, it is not always true that displacement of local expenditures for the same purpose is undesirable.

A related issue concerns the effect of both categorical and general purpose assistance on the composition of school district expenditures. It is quite possible that, while "new" money provided by reform equalizes the level of expenditures per student across districts, the composition of expenditures remains quite disparate. The nature of such differences and their persistence over time is an important concern in equity analysis. One matter that deserves particular study is the absorption of assistance funds into factor costs such as teacher's salaries. Development of a price index for deflation of outlays would help in detecting and adjusting for this problem. This is another reason for emphasizing the importance of creation of such an index in any intensive study of educational finance

and reform in particular states. As an example of a place to look for such effects, recall the dramatically different "matching rates" for state aid created from district to district in Illinois by the Hoffman-Fawell act. As a result of this variation, the cost of an additional dollar in expenditures was made substantially less in low-wealth than in high-wealth school districts. Assuming taxpayers' costs play an important role in determining school board resistance to wage increases, this implies that average teacher salary increases, all other things equal, should be greater in low-wealth Illinois school districts than in high-wealth ones. Is this the case?

Although we believe the matter has a somewhat lower research priority than the problems cited above, every question which arises in the context of interdistrict evaluation of school equity can also be investigated on an *intra*district basis. Here again, we think one or two intensive studies of large multi-school districts which concentrate on methodological issues, data collection, and precision would be more valuable at the present time than a large number of studies all funded at once and undertaken without the benefit of methodological improvements that are likely to be produced by a few more carefully done single district studies. The investigator should be sensitive in evaluation of expenditure neutrality within districts to legitimate expenditure variation associated with exceptional student characteristics.

Finally, the most important question currently unanswered concerns the output effects of the reforms observed so far:

- N.I.E. should support research on identification of effects on academic achievement of students of expenditure changes that have come about because of reform.

Predictive Modeling

- N.I.E. should encourage additional econometric studies of the demand for education.

The work of Ladd, Feldstein, and others on school district demand for education has been extremely productive. Since models of this type are essential for forecasting equity effects of school finance reform, work in this area should be encouraged. Greater confidence could be mustered for the conclusions of research on the demand for education if the estimates of price and wealth-income elasticities proved robust over a variety of samples (the existing research is based primarily on Massachusetts data). N.I.E. should encourage proposals that offer methodological improvements on the following fronts:

(1) *Supply versus demand.* The Ladd and Feldstein equations are called demand functions. However, output is measured in dollars of expenditure, and the price of "units" of education is treated as a function only of the matching rate for externally-provided grants and the composition of the tax base. In the short and possibly the long run, variations in demand for education may influence its price, as was the case in the teacher salary example cited earlier. Additional studies should offer methodological improvement in this area.

These supply price issues can be termed adjustment problems. To date very little work has been done on the issue of speed or pattern of adjustment of school finance systems to changes in independent variables. Normally, the variables cast as determinants of demand in the Ladd-Feldstein models change very slowly. Finance reforms, however, can be abrupt and substantial. Given the difference between such effects and the changes to which school districts have traditionally adjusted, we have little confidence in the appropriateness of estimates such as Feldstein's, which imply that districts require anywhere from 1 to 3 years to complete half of adjustment of expenditures from old to new, post reform, expenditure levels.[1] The actual speed of implementation is likely to vary with the magnitude of the change induced by the reform and the effect of such changes on supply costs. Additional studies also should offer methodological improvements in this area.

(2) *Model specification: functional form.* The demand functions estimated by Ladd and Feldstein are of the familiar "log-linear" form. The advantages of this specification are well known: the coefficients of the independent variables are interpreted as elasticities, at least in other applications the residuals from such functions tend to be more nearly homoscedastic, and it seems "right" that the effect of a change in independent variables like price should be proportional to initial levels of consumption. The disadvantage is that what seems "right" for price effects is not so clear for other expenditure-influencing factors like grants. When dollars of non-matching grants are used as an independent variable, a log-linear specification implies that the effect of X dollars in new money on expenditures will depend on the initial levels of overall expenditures and the ratio of the revised grant totals to the old amounts received. In general if the "real world" is such that $1.00 in new grants produces $.75 in expenditures regardless of the type of district to which the money is delivered, such effects cannot be detected, indeed will be seriously distorted, by the functions used for these studies. The same is true if grant effects depend on district characteristics. Since intergovernmental grants have been extensively used to equalize school expenditures, the effect of transfers on district expenditures is an important problem. The approach utilized by Ladd and Feldstein appears unduly restrictive.

(3) *Analysis of the demand for education using tax referenda data.* The theoretical basis of estimation of community "demand" functions is shaky, although an extensive literature exists on the subject. As the ratio of school children to population declines, the underlying political coalitions which produce the district behavior that Feldstein and Ladd model may become unstable, and behavior of districts may change as a result. No demand studies we have seen attempt to investigate the possibility of discontinuous effects of electorate composition on demand. Such problems may be best addressed through studies of tax and expenditure referenda before and after the introduction of finance reform.[2] Analysis of the price and income effects of reform should provide clues as to which districts should be expected to attempt and to pass tax changes. While we think the demand function estimation approach is a fruitful procedure for analyzing school district behavior, we do not think N.I.E. should ignore sound proposals for direct investigation of voter behavior through referenda study.

At least three problems will be encountered in attempting replication of the Ladd-Feldstein work, and N.I.E. should be prepared for these complications. First, income and demographic data for school districts are likely to be unreliable for intercensal years. The magnitude of this problem is unknown, but if relative incomes across districts do vary significantly over time, continued use of census data to serve as a measure of community income in demand studies creates an errors-in-variables problem that will hamper comparisons of income-wealth elasticities of demand from other studies to the Ladd-Feldstein work. A second "errors-in-variables" problem will be created by differences from one sample to the next in the accuracy of property assessment. The third problem is that it may be difficult to find other situations in which the price of education to local taxpayers varies as in Massachusetts. However, Ladd shows that variation in the shares of industrial and commercial property in the local tax base can under certain assumptions be treated as producing variation in the "price" of education. It should be possible to replicate her procedures even in the absence of matching rate variation. Although hazardous, it might be possible to construct a sample for this type of analysis from comparable school districts in adjacent states; variation in prices could in this instance be gained from variation in state aid formulas.

- N.I.E. should encourage the use of estimated demand functions in simulation studies of school finance reforms. Such simulations are probably best funded as byproducts of research on the demand for education *per se*.

As Ladd recognized, the obvious use for estimated demand functions is for simulation studies of alternative school finance reforms. We would modify her technique in four ways, and recommend such procedures to others.

(1) If estimated demand functions are to be used to evaluate the distribution of inputs under alternative finance schemes, the researcher should make sure that the properties of the functions are explained and justified under all possible combinations of community characteristics contained in the sample (e.g., the appropriateness of model predictions for other than the "mean" community). This is particularly important for features of the model, like the implied expenditure effects of grants, which directly affect the impact of the instrument utilized by the policy being simulated. Here, as elsewhere, better simulations require better models.

(2) Once the simulations are completed, the resulting data are usefully summarized using the same equity measures as were recommended for use in analysis of equality of expenditure distribution under existing systems.

(3) Since reform plans likely to be implemented include both "matching" and "non-matching" grants, simulation experiments can usefully involve policies with both price and income effects. An important output for any such calculation would be the ratio of change in aid to changes in expenditures for districts of various types. Such data would provide some clues with regard to the expenditure versus tax relief effects of reform.

(4) To the extent adjustment processes can be explicitly modeled, simulations might usefully include outputs intermediate between "impact" and full-adjustment effects of reform. Surely Keynes' famous dictum about the long-run

(that we will all be dead when it arrives) applies even more in education than in macroeconomics, and data on likely second- and third-year developments, while perhaps myopic, would assist in monitoring and interpreting changes in expenditures observed in the actual implementation process.

- N.I.E. should encourage theoretical and empirical work on the effects of educational finance reform on the location of firms.

The demand functions described as an important object for research in the previous section will inevitably be flawed by failure to incorporate effects of changes in school finance procedures on the location of firms and households. For firms, we think research could usefully be focused on development of theoretical models of tax effects on firm location with testable implications. While direct study of the siting of new plants may be extremely difficult, it may be possible to investigate the effect on the value of land zoned commercial or industrial of changes in finance procedure which differentially affect the level of property taxes across school districts.

- N.I.E. should support research on the consequences of school finance reform for residential location and property values.

Available empirical evidence on property tax capitalization suggests that school finance reform is likely to cause large changes in residential valuation. To our knowledge no evidence has been accumulated on the property value effect of large-scale reforms. Such research is important for two reasons specific to education. First, it provides an indirect test of the "maximizing voter" models that are the foundation of most studies of the demand for education. Second, property values are an important determinant of district educational expenditures in such models. Ladd's simulation results rest on the assumption that property value changes that result from imposition of new forms of state aid do not themselves shift the demand for education. The reliability of this assumption is usefully tested through measurement of the property value effect of reform in practice.

Changes in property values reflect changes in residential preferences of households in response to tax reform. Thus any analysis of the effects of school finance reforms on property values should involve development of a model to predict changes in household location patterns in response to the post-reform educational costs and opportunities.

- N.I.E. should support additional research into the reasons for the growth and changing nature of private schools and the relation, if any, of private school utilization and public school expenditures.

One problem encountered in analysis of the determinants of choice of private schools is that to date the proportion of families opting to enroll children in private schools is in many districts quite small. As a result, cross-section samples of households will usually involve too few observations on families sending children to private school to perform analysis of the choice. However, during the last two years statistical procedures have been developed to permit analysis of such decisions using samples that have been supplemented to increase representation of families making particular choices--in the school case, of families choosing to send children to private schools.[3] These techniques offer great promise and should be introduced into educational research.

- N.I.E. should fund research on the determinants of household use of privately-provided supplemental education by families with children normally enrolled in public schools.

The shift from public to private school is a costly and discontinuous adaptation of household education demand to perceived inadequacies in the public schools. It is possibly more common for families to adjust by purchasing or in other ways providing education from non-public school sources as a remedy for public school inadequacies than for families to change from public to private sources altogether. The extent of such activities, their association with variations in expenditures in public schools, and changes in these factors over time are difficult to analyze. But any information on this front would be valuable in assessing the implication of changes in school finance for the total supply and distribution of education received by children.

References

1. See M. Feldstein, "Wealth Neutrality and Local Choice in Public Education," *American Economic Review* 65 (March 1975): 75-89.
2. See A. Alexander and G. Bass, "Schools, Taxes, and Voter Behavior: An Analysis of School District Property Tax Elections," The Rand Corporation, R-1465-FF, April 1974, for an important example of referenda analysis.
3. See C. Manski and S. Lerman, "The Estimation of Choice Probabilities from Choice-Based Samples," *Econometrica*, forthcoming.

Chapter 4

Toward a Contingency Theory of Organizational Change in Education: Structure, Processes, and Symbolism

Terrence E. Deal and C. Brooklyn Derr

Introduction

Change and reform were among the most pressing educational issues of the 1960s. Unsolved then, the problems of changing schools have carried over into the present decade. In the 1970s, however, the optimistic spirit of innovation and reform has shifted to a tone of guarded pessimism. More importantly, the impetus for change has shifted from internal "change agents" within districts and schools to external economic, political and judicial forces. Educational organizations thus face pressures for change and reform over which they have little control.

Because the pressures for change and reform are external the key issue is how districts and schools can adapt. Adaption often involves changing internal structures and dynamics. The following illustrations underscore the potential impact of selected external forces on the internal workings of schools:

1. Declining enrollments create the need to close schools, to lay-off staff, to retrain high seniority teachers and adminstrators for new roles and responsibilities, to allocate human resources without jeopardizing the quality of existing programs, and to develop procedures for managing an endless series of zero-sum conflicts.

2. Competency-based learning involves a thorough overhaul of teaching approaches, changes the relationship between teachers and students, make obsolete existing staffing arrangements—such as departmentalization organized around traditional subjects—and creates the need for new systems of evaluating students, teachers and administrators.

3. The emphasis on accountability stimulates new approaches for evaluating teacher and administrative performance and for assessing educational outcomes. Evaluation results often call to attention inherent weaknesses in the decidedly

imperfect technology of teaching thus reducing staff morale and community support.

4. Collective bargaining laws which permit administrators to opt for supervisorial bargaining units change existing patterns of control and authority, encourage the formalization of district policies, and place a third party (usually an attorney) at the bargaining table to decide basic policy, as well as salary issues.

5. Court decisions to equalize educational opportunities, such as the Serrano decision, also have important organizational implications. For some districts, problems will revolve around the allocation of new resources. Should new staff be hired? Should new instructional materials be purchased? Or, should new monies be used to raise salaries of existing staff? For other districts, the decisions will involve cutting back personnel and instructional expenditures without severely harming top-quality programs.

This list could go on endlessly. The key point is that each change or reform that is implemented will affect dramatically the roles, relationships, processes, values, rituals, activities, and other existing features of districts and schools. Whether these changes and reforms succeed or fail, therefore, depends heavily on how well existing organizational patterns can be realigned. Whether the current change and reform agenda is implemented successfully or whether it is resisted, sabotaged, or transformed depends on the ability of school organizations to adapt.

Adaptations might be easier if the experiences of the 1960s had produced some guidelines for ways in which educational organizations could be changed successfully. But, at best, this earlier decade provided only suggestions about how such changes should not be made. The likelihood of implementing current changes and reforms successfully might also be higher if the structures, processes, and symbolic characteristics of organizations that must be altered to provide necessary support were identified. But, unfortunately this needed knowledge has not yet been produced and educational organizations are usually viewed in a simplistic, unilateral and business-industrial way.

Consequently, we enter a new period of educational change without sufficient knowledge about ways in which district, school, or classroom organizational patterns might be altered to provide appropriate support for externally produced changes. Additionally, we enter this new period without a clear research agenda which might help to identify these new structures and processes.

This chapter synthesizes existing organizational theories and the literature on change and planned change into a framework for viewing the internal organization of schools (as it is affected by external events). The conceptual framework provides both a means of analyzing schools and of suggesting a research agenda which may produce needed knowledge in the area of school organization.

This chapter is organized into five sections. Section I reviews the literature on change underlining the importance of the organization and illustrates where past efforts to deal with internal characteristics and dynamics have fallen short. Sec-

tion II outlines three conceptual needs that must be kept in mind if approaches to internal organization are to provide adequate assistance to schools as they grapple with change and reform. Section III outlines and discusses a framework that addresses these three needs. Section IV summarizes why the problem and the proposed framework are important for American education and emphasizes how society, parents, children, and professionals will be affected unless the organizational dynamics of change and reform are addressed. Finally, we suggest in Section V some directions for future research.

The Importance of the Internal Organization in Change and Reforms

Educational Change and Reform in the 1960s

In a broad historical perspective, schools have incorporated a number of changes with considerable success (Tyack, 1976). By contrast, change in the 1960s was plagued with problems and littered with innovations which never had their intended impact on schools. These "failures", where changes did not meet their intended mark, were documented in case studies at all levels of education. At the elementary level, attempts to change classroom instruction and the student-teacher relationship (Gross, et al. 1975), to change mathematics instruction (Sarason, 1971), to change staffing patterns (Packard, 1975), and to alter dramatically the main goals and structure of an entire school (Smith and Keith, 1971) ended in no change or organizational disaster. At the secondary level, attempts to implement differentiated staffing were unsuccessful (Wacaster, 1975), and alternative schools experienced difficulties which often lead to failure (Deal, 1975). At the level of higher education, Bennis (1975) describes the difficulties involved in changing the structure of a large university. Finally, Kirst (1975) describes the failure of PPBS to have any noticeable impact on evaluation processes in California school districts and Levinson (1976) descibes the modest effects of the voucher demonstration in Alum Rock.

These case studies and other literature suggest three main assumptions underlying previous efforts to change schools:

1. These efforts often assumed that individuals were the epicenter of change ignoring that educational change takes place in an organizational context.

2. These efforts often assumed that successful practices or models developed in one setting could be easily transported and implemented in another ignoring the important ways in which education differs from other enterprises—differences which make such a transfer exceedingly difficult.

3. These efforts assumed that innovations, once adopted, would automatically be implemented and easily become an integral part of the organization ignoring the complexities involved in moving beyond the adoption phase of the change process to implementation and incorporation.

While this analysis highlights invalid assumptions, it only hints at other assumptions that may be used to guide educational change in the 1970s. Current work suggests that the key to change may lie in the process of implementation (Berman and McLaughin, 1975; ABT Associates, 1977). The Berman-McLaughin study emphasizes the importance of climate, problem-solving and adaptation in the process of implementing changes. The ABT work suggests that structural and cultural variables play an important role in implementing change successfully. Both studies emphasize the importance of identifying internal characteristics of organizations that can be manipulated to help schools adapt to external pressures for change. As these elements are identified, it then becomes important to specify systematically the important relationships between these characteristics. At present, however, there is a considerable lag between theory and research and actual approaches that are used to change schools. Narrowing this gap is a primary goal of Organizational Development (OD) activities.

The Organizational Development Movement

As schools experienced the problems associated with change in the 1960s the Organizational Development (OD) movement became a source of ideas and techniques for conducting change in organizational settings. But as educators looked toward OD, they found a literature that was disparate, obtuse and difficult to document. Many successful OD specialists oriented toward action were satisfied to experience an organizational intervention without paying due attention to subsequent research and reporting. As a result, the OD movement is nearly twenty years old and has only recently begun to document seriously its theoretical premises and its untested assumptions (Friedlander and Brown, 1974; Alderfer, 1977).

A definition of Organization Development which seems to fit the current mood (as taken from the literature and interaction at professional meetings) is:

> Organization Development is the application of organizational behavior concepts and practices for the purpose of intervening to improve the human side of the enterprise.

This definition is different from those articulated early in the OD movement (Bennis, 1966; Blake and Mouton, 1968; Beckhard, 1969) which were biased towards change values, consulting practices, certain technologies of intervention, and applied behavioral science in the more narrow sense of group work.

The current mood is to view organizational behavior itself—an amalgamation of social psychology, organizational sociology, and management theory—as the discipline which drives the OD technology. This places a broader, more wide-ranging set of concerns and practices under the OD rubric. While still people-oriented, the emphasis in OD is now on total organizational improvement, a broader perspective than just changing human attitudes and practices (French and Bell, 1973; Galbraith, 1973; Roeber, 1973).

Many interdisciplinary and systemic interventions, viewed as heretic in the past, are now reported at national meetings. OD specialists currently possess a variety of backgrounds, not just those of behavioral science, and are bringing

diverse orientations to the issues of organizational improvement. While people problems are still the dominant concern, the interface with the information system, the control system, the external environment, the authority and power/influence system, planning and budgeting, are all important issues in Organization Development. There is a concern at present for acting appropriately to meet a specific situation rather finding a one-best-way approach. (Harvey, 1975; Shepard, 1975; Bolster, 1976).

Many of the founders of the OD movement are uncomfortable with this more inclusive approach (Miles, 1977; Tannenbaum, 1976). They are concerned that what used to be a unique perspective on organizational change via the consultation process has become distorted. OD, they argue, is in danger of losing its identity and becoming management development. Quality control is difficult when OD professionals are so broadly defined.

Nevertheless, it is possible to underscore some basic values, assumptions and practices of OD:

1. *Readiness*—The organization should experience some pressures or "pain" for improvement, should have the commitment and resources necessary to make appropriate changes over some period of time (3-5 years, for example), should have the active support of those at the top of the hierarchy and should already possess some norms of openness and exploration which allow for problem-finding and problem-solving.

2. *Collaboration*—OD specialists work together with persons in the client system to help them prepare to be self-sufficient and competent in directing their own organizational improvement effort. OD is value-biased towards collaboration. Its methods, such as conflict management via problem-solving, presume that people will discover intrinsic and extrinsic rewards as they expend the time and energy together.

3. *Systemic Orientation*—One thinks wholistically about the effects of an intervention in one part of the enterprise on other subsystems. It is important to develop the whole system if norms, processes, skills, and behaviors are to be changed and institutionalized. Otherwise, the employee lives in a schizophrenic world where he must use different organizational styles for different settings. A style appropriate for one part of the system may lead to harmful effects elsewhere in the same enterprise.

4. *Internal and External Consultants*—The model frequently articulated is that of consultant-client. Many OD specialists are external consultants. Theirs is a useful role which can be used in situations where employees would be the captives of their own organizational norms, policies, assumptions, and so on. For example, external consultants are marginal and can easily put their judgment on the line and leave if certain ethical or contractual principles are broken. External consultants are also more objective and less indebted to and captured by the organization. They are frequently high status professionals and

therefore, have better access to the people at the very top of the hierarchy.

Internal consultants or full-time employees, on the other hand, are more permanent and can work to institutionalize changes. They are more apt to know the real reasons for some organizational problems. Internal consultants are frequently those who actually help work out the details of the changes and build the new organizational structures, processes and values. OD realizes that its most successful efforts have had the benefit of both approaches working symbiotically.

5. *Centrality of Human Problems*—While organizational structure, management information systems, architecture, operations research, technology, and accounting are all seen as important and potentially complimentary management functions, OD focuses on improving human resources. Most OD theory is normative and biased towards worker creativity, openness, risk taking, collaboration and self-fulfillment as this meshes with organizational purpose.

6. *Importance of the Setting*—More recently the strong emphasis on a one-best-way technology based on a strong set of people values has given way to an equally important stress on environmental coping. Organizations must respond adequately to demands from the external environment.

One implication of emphasizing the envirdnment is to recognize the importance of a particular setting. Thus, OD in a bank may be performed somewhat differently, given the demands of the external milieu, than OD in an electronics firm. OD in schools may have significant differences from OD in hospitals. OD in an urban high school may be still different from its rural counterpart.

The OD movement in schools began in ernest about a decade following its earlier development in industry. Numerous OD professionals come to education from industrial settings, attracted by a new market and, in the mid-1960's, by ample funding for educational innovation and experimentation (Schmuck and Miles, 1971). Many of the OD value biases and assumptions also carried to education from industry. OD had its nexus in industry and that experience was often transposed onto schools by non-educator OD practitioners.

Schools in the sixties were being transposed upon in many ways. The abortive innovation effort of that period can often be traced to problems created by taking the "latest thing" from industry and adopting it, without much adaptation, in schools. Witness, for example, the PPBS movement, performance contracting, MBO, T-groups, etc. All of these ideas had their merits for education. Yet, they became "faddish" and were part of a period of short-term trials without adequate commitment or time for proper evaluation and modification (Jamieson, 1977).

In the early sixties, many institutions which trained the nation's school superintendents, had a well-known formula for mobility and succession. The new

superintendent was to be aware of new management technologies, curricular novelties and special programs, and to attempt to implement these quickly and without careful evaluation. For his efforts, the superintendent would gleen the public relations and ensuing reputational benefits which would catapult him into his next more important superintendency. It was almost expected that the former innovations would cease to exist with a new superintendent anxious to repeat the same pattern. Thus, innovations were not seen so much for system improvement as for their imagery which would lead to personal/professional success (Derr, 1976).

Schools may be different from many organizations in that a curricular bias exists for presenting concepts in neat packages which are easy to employ by educator-generalists. Thus, many of the management innovations most preferred by schools during this period were one-best-way approaches and easy for existing employees to use. They were the quickest, most general, easiest to apply and best presented.

Into this background came the OD movement in schools. OD also became a "fad," even though its theory should enhance long-term commitment. Much of what was labeled OD was really demonstration training (e.g., running effective meetings, goal-setting, communication) done after school or during one-day inservice workshops. The OD packages became one more program rather than a continuous process of organizational improvement. Educators returned to the university where inadequately trained professors—often themselves switching their interests from fad to fad—gave a one-course overview that would equip administrators to launch their districts into Organization Development.

The OD literature of this period tended to contain: evaluations of training events; thought pieces and theoretical propositions, or case studies (Derr, 1970, 1972, 1974; Schmuck, Runkel and Langmeyer, 1969; Schmuck, 1974; Runkel and Bell, 1976; Brown, 1976; Croft and Falusi, 1973; Alschuler, 1972; McMillan, 1975). The most popular book of the educational OD movement was a do-it-yourself handbook (Schmuck et al., 1972). The Northwest Regional Education Laboratory had considerable success packaging and publishing for distribution OD programs (Jung, 1976).

Organization Development seemed to be narrowly defined as training groups and human relations. There were few structural changes which accompanied new procedures. There was little emphasis on the interface between people problems and other systems or technologies.

Because of these limitations, the OD in schools movement is almost a past-tense phenomenon. While many school districts are still involved in some form of organization development, OD no longer occupies the "limelight" at professional meetings and in the educational literature. It has not endured in education as it has in many other sectors. What is taking place at the present time seems to be the use of OD concepts and practices to address important issues felt by educators. Thus, while the "OD in Education Movement" may be dead, organization development per se may be more alive than ever in the form it should have originally been intended.

In sum, the history of change and planned change has left a legacy documenting the importance of tailoring internal characteristics to support change, yet providing little guidance for which patterns might be useful or for ways that such patterns might be developed.

What Is Needed to Help Schools Develop Organizationally

A basic assumption of this paper is that organizational structures, processes, and symbolic characteristics—such as rituals and myths—contribute to and thus provide a way to attach existing educational problems and the difficulties inherent in change. Following from this assumption, theories of organizational behavior and research in organizations can offer some insights for developing and utilizing human resources in productive, efficient ways. Yet, such knowledge has not provided much assistance to schools for two reasons. First, theories of applying existing theory and knowledge to educational organizations lags behind the growth of the knowledge base itself. Second, theories of organizational behavior have been constructed on a foundation of research conducted principally in business organizations. There, in such organizations the conditions—and the problems—are much different from those that exist to teach and socialize the young. Nevertheless, adapting structures and processes in educational organizations to meet external pressures is an important issue. Applying organizational theory and principles to problems of change therefore appears a fruitful investment of time and resources. As this application is made, three conceptual "needs" become evident: (1) the need to recognize the unique characteristics of schools, (2) the need to incorporate new theoretical developments into traditional patterns of thought, and (3) the need to integrate perspective, tools and strategies into a coherent and unified approach.

The Unique Nature of Schools Must Be Taken Into Account

An adequate and efficacious theory and method of organizational change in schools will include assumptions unique to the nature of schools. Schools differ from other organizations in the following ways:

1. *Schools have diffuse, unclear goals.*

In business and industry, the primary goal of profit making is simple and is shared by all individuals who participate in the organization. In schools, goals are more diffuse, numerous, and conflicting. Schools instruct, socialize, certify, select, and provide custody and control of students (Spady, 1974). Schools also provide employment and social services. Unclear goals make it difficult to specify criteria for determining organizational effectiveness or for evaluating individual performances. What makes a "good" school or an "effective" teacher is therefore determined more by myth, belief, and confidence than more objective assess-

ments of effectiveness (Meyer and Rowan, 1977). The multiplicity of goals makes it difficult for schools to pursue a common direction. Many of the goals of schools are latent and when made explicit cause conflict and political struggle of educational priorities. Although these issues over goals arise also in business organizations, they are more pronounced in schools.

2. *Schools lack well-developed technologies.*

Many enterprises have well-developed rational procedures for transforming inputs into desired organizational outputs. Where procedures linking inputs to outputs are less susceptible to rational verification, their intended effects are supported by systems of belief. In both cases individuals are held accountable for performing tasks in a certain way. The linkage between teaching and learning are tenuous at best. Educational researchers have difficulty determining the connection between teaching and learning outcomes. So do most administrators and teachers. What constitutes an effective teaching or administrative performance must therefore be based principally on faith—that whatever is done will have some impact on learning (Dreeben, 1970).

3. *Schools are inter-penetrated and controlled by their environments.*

Although schools have a virtual "monopoly," they are heavily dependent on local communities for a large portion of their support. In the absence of clear measures of effectiveness or well developed technologies, schools rely heavily on public relations and attempt to maintain an image of a smooth-running, efficient, "professional" organization (Bidwell, 1965).

4. *The myth of "professionalism" makes school "employees" highly autonomous and immune from bureaucratic controls.*

Professionals place high value on working alone and using their "special knowledge" to solve problems on behalf of clients. Although the teaching profession exhibits few of the characteristics of a true profession—such as a specialized knowledge base and peer control—the image of being professional supports self-contained classroom units where teachers work outside the central hierarchal superiors. The autonomy needs and norms in schools are exceptionally pervasive and powerful (Lortie, 1969).

5. *The level of required interdependence in schools is low.*

Teachers work independently of each other and of other role groups such as specialists. When specialists do work in schools, they often function on a "pull-out" basis, removing students from classes for specialized instruction. Although teachers and other school "employees" are interdependent in the lunch room, the playground, on schoolwide committees or have common discipline cases, faculty parties, and inservice training, the levels of collaboration in schools is quite low compared to that found in other organizational settings (Derr, 1971; Schmuck and Miles, 1971).

6. *Alongside the myth of professionalism in schools is a civil-service mentality.*

In reality, schools provide a secure employment system. Teacher unions exert strong influence over teacher salaries and working conditions. Often, negotiated contracts stipulate working conditions which work to the advantage of

teachers as employees, not to the educational programs of the school (Cronin, 1970).

The important ways in which schools differ from other organizations have implications for change and reform in schools. Often approaches to assist schools in adapting internal characteristics to external pressures for change and reform do not take these special characteristics into account.

New Theoretical Development Must Be Included

Developments in organizational theory provide some interesting new insights as schools struggle to keep internal characteristics apace of external changes. Three theoretical approaches are worthy of special attention: the organized anarchy approach, the loose-coupling approach and the open systems approach.

Organized anarchies are a construct used by March and Olsen (1976) to describe organizations with unclear goals, undeveloped technologies and fluid participation of members. In organized anarchies, the norms of rationality emphasized by other organizational theorists (Thompson, 1967; Scott, 1975) do not hold. In such organizations, structure is not linked to outcomes. Through all organizations flow a constant stream of time, energy, and problems. Where these elements intersect in organized anarchies, arenas are created for the purpose of making decisions and solving problems. Unclear goals, undeveloped technologies and the fluidity of participation, however, make it impossible for complex problems to be solved or for critical decisions to be made. Rather, the decision-making or problem-solving arena becomes a "garbage can" (March and Olsen, 1976) into which participants, problems, energy, time and emotions are dumped. Once the can is filled it is tossed away. Conflict is reduced and participants feel more satisfied. Time, energy, and problems continue to flow through the organization. But structure is still detached from activity, problems are still disconnected from solutions, and activities are independent of outcomes.

The implications of this perspective for schools is interesting. Schools are characterized by unclear goals, and undeveloped technologies. Participation is quite fluid with the cast of characters involved in decision-making—teachers, parents and administrators—changing frequently. Schools are thus organized anarchies and viewing them from a more rational perspective assumes characteristics that do not exist. Problem-solving, decision-making, conflict resolution, formal meetings; policies and rules, and other aspects of structure and activity exist. But these serve different purposes than we may have heretofore imagined. The function of such elements or activities is essentially ritualistic. Ritual provides cohesion that holds the organization together. But, the basic nature of the educational enterprise makes it impossible to link rationally, structure with activity, problems with solutions, or activities with outcomes. Organized anarchies are controlled organizationally, but the basis of control is largely non-rational.

Loose-coupling is a diffuse umbrella-like term (Weick, 1976) used to describe structural looseness or the lack of coordination in organizations. Rather than viewing "looseness" as dysfunctional, however, many existing "loose-coupling" inquiries entertain the possibility that, under certain circumstances, structural

looseness may be quite functional or rational (Meyer, 1975; Meyer and Rowan, 1977).

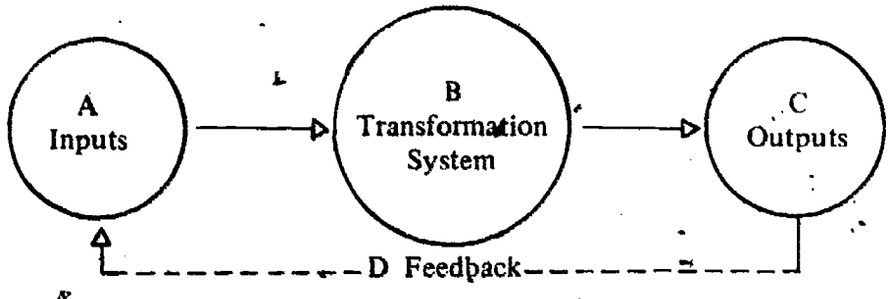
A recent longitudinal study of thirty-four school districts (Cohen, et al., 1976) and one hundred eighty-eight elementary schools has produced some empirical findings, (or non-findings), supporting the loose-coupling viewpoint. The study found that instruction and organizational patterns within classrooms—teacher teaming—are unaffected by organizational characteristics at the school or district level. The structural image emerging from these findings is one of double segmentation (Deal, Meyer and Scott, 1973). Schools operate independently of the district, and classrooms operate independently within the school. These findings are supported by analyses of variance which show that perceptions of individuals at various levels do not agree. These analyses also show that often within levels individuals do not agree about issues where consensus might be anticipated given a socially-cohesive organization (Meyer, Scott, Intili, Cole, 1976).

Meyer (1975) and Meyer and Rowan (1977) have developed a theory of institutional control of schools which explains these findings. They argue that, in schools, control centers around the categories of education: place, topics and roles. Tight controls would require that: there must be a place called school; there must be curricula; and there must be a basic differentiation of roles and responsibilities: pupil, teacher, and principal. The activities of the school or the classroom, however, are not controlled formally but operate on a "logic of confidence" which assumes that if the appropriate categories are in place, instruction is taking place and learning is occurring. Schools are thus controlled by institutional values and myths. Schools whose structures and activities conform to these myths receive social support: those that do not are sanctioned. Loose-coupling is thus [illegible]tional organization response because coordination or evaluation of activities or outcomes can yield conflict, costs, and the possibility of reduced support. Loose-coupling theory suggests two interesting propositions: (1) Successful educational organizations will respond to new environmental pressures by adding components or units but assuring that these are buffered from the existing structure or activity. (2) Successful educational organizations will resist changes which require evaluation of activities.

Finally, schools have been conceptualized as open-systems (Schmuck, et al., 1972; Jamieson, 1977; Deal and Rosaler, 1975). From an open-systems viewpoint, schools are systems of interrelated parts—or sub-systems—which carry out a continuous transaction with the environment. As shown in figure 1, schools receive

FIGURE 1

THE TRANSFORMATION PROCESS



inputs from the environment, transform these inputs in specified ways, and provide the environment with outputs. The transformation system itself consists of both technical procedures and an organization of human resources appropriate to the task. Under ideal conditions, feedback evaluating the input-output linkage provides the organization with guidelines for modifying the transformation process.

As schools struggle with the external pressures of the 1970s three aspects of the transformation process are often emphasized: (A) the nature of the inputs—clients, resources, pressures for change, (B) the outcomes of schooling, or (D) the relationships between the two. For example, the Serrano v. Priest decision in California is designed to redistribute monetary resources in an attempt to increase the disparity between the educational opportunities of students who attend districts in low-wealth districts and those who attend districts with ample monetary resources. Underlying the decision is an assumption that increases in organizational input will result in higher levels of organizational output.

Less attention has been devoted to the aspects of the transformation system itself (C): the technical procedures carried out by schools and various aspects of the organization—the structure of roles and relationships, human processes, and underlying symbolic characteristics such as beliefs, values and myths. As district wealth levels are affected by the Serrano decision, for example, poor districts will be able to purchase instructional materials and systems, add new teachers, create new school roles such as specialists and aides, provide inservice training, improve instruction, decision-making and evaluation, and provide staff administrators at the district level. Wealthier districts, on the other hand, may be forced with reassigning and retraining personnel, cutting back on all but necessary personnel, and trying to continue current practices with less adequate resources.

In either case, the key to the eventual impact of Serrano on the intended outcome of increasing educational opportunities and the achievement levels of children in low-wealth districts is highly contingent on the manner in which resources are used to redesign existing aspects of the organization—or the transformation system. In a similar fashion, schools that must now get by with less must have guidelines for retailoring technical processes, structural patterns, and symbolic elements such as values, beliefs, or myths in ways that do not unduly interfere with existing programs and effectiveness levels.

From an open-systems perspective (Katz and Kahn, 1966) organizational input and output are important issues. Both can affect the organization. Changing a school's clientele can have profound implications for existing organizational patterns. Important implications can also be produced by making public the performance of a school's students on state-wide reading tests. Equally important, however, are the aspects of the organization—technical processes, roles and relations, human processes, and symbolic features—which either change to accommodate and support these other changes or work to neutralize or redirect them (Meyer and Rowan, 1977).

Organizations have internal objectives of their own which may or may not be congruent with demands of the external environment. Increased resources from Serrano, for example, may be allocated primarily to teachers' salaries since schools are organized employment systems as well as "producers" of student

learning. Similarly, attempts to implement new evaluation processes may be undercut because schools need to preserve the "logic of confidence" which presumes that teaching activities are linked to outcomes in order to obtain continued community support (Meyer and Rowan, 1977).

Organizations often prepare themselves to respond to one set of demands and to resist countervailing pressures that would send them in a new direction. Environmental pressures are often confusing and contradictory and if schools reacted, in good faith, to each external stimuli it would be difficult to accomplish important educational objectives and tasks. Effective organizations build elaborate mechanisms to sense new environmental pressures, determine their importance, cope superficially with those not viewed as long-term, and stall for time when pressures are of significant consequence. Because schools exist in less well defined institutional environments than organizations operating in well developed technical environments, the mechanisms that delay, distort, and swallow environmentally induced reforms are particularly well developed. Such mechanisms may be necessary for continued survival and support (Meyer and Rowan, 1977).

Many "new" pressures for educational change get distorted or become lost and impotent at the thru-put stage. School organizations can be very vulnerable to external inputs; therefore, like other human institutions they have become expert at finding ways to protect themselves against environmental whim. New stimuli are unlikely to make a difference unless they can be integrated into the on-going structures, processes and symbolic characteristics of educational organizations in ways that do not create problems of environmental support and survival.

Moreover, those stimuli which are so momentous that the organization must incorporate them to continue to survive are likely to be integrated into the on-going structures, processes, myths and values rather than to drive the system radically away from its past.

Approaches to Organizational Change—Perspectives, Tools, and Strategies Must Be Integrated

Developing successful organizational responses to external pressures for changes and reform requires perspectives, tools and strategies that integrate the important aspects of internal organization.

Past strategies and techniques for organizational improvement in schools were heavily influenced by two separate streams of organizational theory. The first assumed that effective organizations could be built by improving communications, fostering trust and collaboration, encouraging widespread participation, and developing organic, fluid work patterns. The second assumed that effective organizations were produced by dividing roles and responsibilities, encouraging specialization and coordinating diverse efforts through planning, explicit, well-understood policies, clear channels of authority, and vigorous evaluations of performances and outcomes. Human relations approaches focused mainly on encouraging change; structural approaches emphasized implementation and institutionalization of change. Human relations approaches portrayed schools as bureaucratic institutions that needed "loosening-up." Structural approaches viewed

schools as loose collectives that could benefit from considerable tightening. In reality, however, educational organizations are composed of both processes and structures. Moreover, they incorporate symbolic systems of values, myths, and norms. Many of these evolve from the unique characteristics of schools; many are found in other organizations as well. The important point is that the social organization of schools is three-dimensional with three highly related aspects—structures, processes, and symbolic features such as values, beliefs, and myths. As schools adapt to external pressures, the equilibrium between these dimensions is changed. Successfully adapting to changes thus directs attention to three separate but connected elements. It also involves some knowledge about the important ways in which these three elements are related.

In summary, successful organizational change in schools will be uniquely tailored to particular types of educational enterprises; will pay adequate attention to current organizational theories, and will integrate the three important aspects of the management function: human and non-human technologies, structure, and symbols.

Toward a Contingency Theory of Internal Organization

Addressing the three areas of need is an important requirement in developing conceptual schemes for viewing the internal organization of schools. Here the three needs identified in the previous section are accepted as criteria for developing one such framework.

A Three-Dimensional Organizational Perspective

As noted earlier, all aspects of the transformation process—input, the transformation system, and output—are highly related. Changes in the nature of the clientele or level of resources, for example, put pressure on the existing transformation system in schools. Similarly, shifts in social expectations for what schools "produce" may require changes in educational techniques or in existing organizational patterns. The key to the ability of environmental pressures to have their intended impact (the ability of schools to successfully adapt to or to resist such pressures) is the nature of the internal organization, or transformation system. The outcomes of change and reform are highly dependent on the way in which the organization adapts.

Organizations are viewed from a number of perspectives. Each singles out certain aspects of organizations as more salient than others. Human relations approaches emphasize processes and interactions. Structuralists attend to regularized patterns of behavior. Phenomenologists examine the underlying symbolism of organizational patterns and activities. The problem is that rarely have these diverse viewpoints been integrated—even though each deals with an important aspect of organizational life.

Integrating these approaches into a single framework is necessary for generating pragmatic approaches to change and reform. In our view, organizations—educational or otherwise—consist of three dimensions: (1) structural, (2) processual, and (3) symbolic.

The structure of an organization incorporates both formal and informal properties. Structure refers to the way in which individuals and groups are configured to accomplish various organizational tasks. Structure consists of lateral and vertical role differentiation (e.g., specialists and aides, administrators, teachers and clerks), levels of interdependence (e.g., teams, collaboration, and task interaction), schemes and mechanisms for coordinating diverse efforts (e.g., policies, rules and authority patterns), time-space-facility arrangements (e.g., open-space, flexible scheduling, and multi-graded classrooms) and technologies (e.g., individualized instruction, community education, and teaching machines). Structure provides a context for both informal and task interaction and processes. Structure facilitates and constrains processes and symbolic interaction.

The structure of a typical elementary school, for example, would have the following characteristics. Roles would be differentiated into those of principal, teacher, student and support staff—secretaries, nurse, custodians and cafeteria workers. For the most part, the efforts of these individual roles would be relatively independent—particularly those concerned with instruction. Coordination levels would be low, accomplished loosely through the authority of the principal and a few diffuse policies and rules.

Although there is an obvious overlap between structure and process, processes can be characterized as the ways of interacting and accomplishing work. Like structure, processes incorporate both formal and informal interactions including: decision-making, goal setting, problem-solving, communication, evaluating, planning, meetings, team building, leadership and conflict-management. Processes are action expressions of the organizational structure.

In an experimental high school, for example, a number of these organizational processes might be expected to operate at high levels: meetings would occur frequently, they would have a particular form or procedure (e.g., town meetings), excessive conflict would require systematic conflict management efforts, high participation in a large number of decisions would occur and require sophisticated decision-making mechanisms, communication rates would be vigorous, and planning continuous.

The symbolic dimension is a constellation of non-rational, non-verifiable, self-reinforcing meanings and understanding often overlooked or ignored. It includes myths (professionalism of teachers, community control), rituals and ceremonies (lectures, recitations, evaluations, meetings, saluting the flag, the Christmas pageant, PTA), values and beliefs (homework increases learning, education is related to success, individualized instruction is desirable, meetings solve problems), and norms (teacher equality and autonomy, conflict should be overlooked or ignored). Symbolism overlaps with the other two dimensions. Policies can be mythical; meetings are often ceremonial; and evaluation serves as an important ritual.

The "myth" of teacher professionalism, for example, helps to justify current patterns of organizational "non-control" in schools and to reinforce high levels of

teacher discretion and autonomy. Similarly, equality "norms" make proposals for differentiated staffing and merit pay nearly impossible for schools to implement.

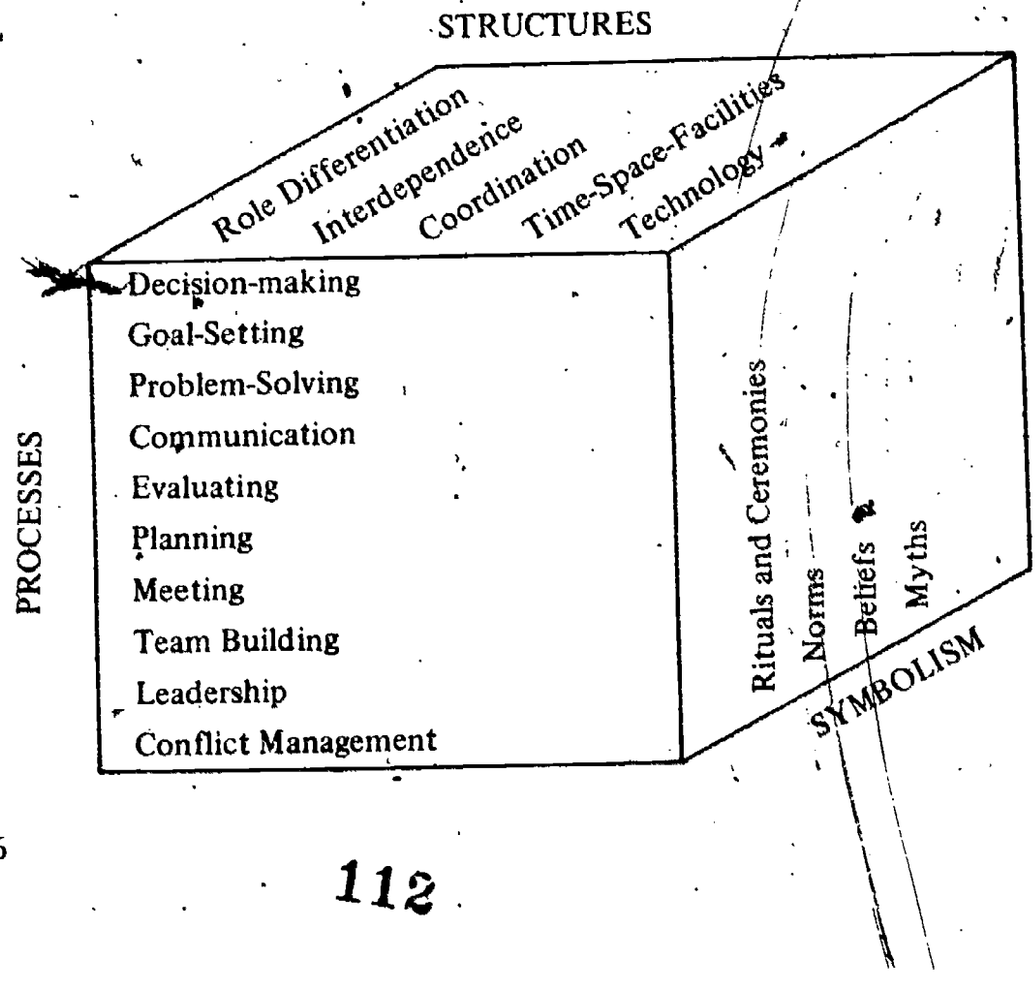
Behind "professed" symbols there operate a number of purposes and practices which may be more compatible with the "actual" purpose of the organization. Cronin's study (1970) of the Boston schools, for example, found that Boston was first an employment agency for the Irish-Catholics of the city and, secondly, an educational enterprise. There is indication that this phenomenon is true for Blacks in Detroit. Moreover, it can be projected that given the tight teacher-administrator job market in education and given the nature of teacher unions, employment is a primary value.

Among school personnel, employment may be more important to adults in educational systems than teaching. Indeed, schooling is good employment because it is relatively unsupervised and provides interesting work, good pay and benefits, compatible hours and long holidays.

Another myth which may be considered is that education and training are the primary concerns of parents. In reality, they may value day care services to free them from intensive parenting as much or more than the educational benefits to their children. Every parent, of course, hopes his child is learning and growing and no parent wants his child harmed. Few parents are willing to support, however, schemes for part-day or flexible scheduling—even when such ideas are educationally sound.

Together, these dimensions can be combined in a three dimension "cube" (figure 2) which provides a framework for viewing, in open systems terms, the transformation or throughput function of schools.

FIGURE 2



The Interactive Character of the Three Dimensions

The three dimensions of organizations overlap considerably. They are also mutually supportive and interrelated. Structures support processes. Processes give vitality to existing structures. Structures and processes provide the formal expression of symbolism. Symbols, on the other hand, provide meaning for structures and processes.

Because of these intricate interrelationships, reforms and innovations imposed on schools by the environment (or developed internally) and designed to affect a single dimension will affect the others. Structures, processes, and symbolic features exist in a certain equilibrium. Changing one dimension puts pressures on the others and affects the equilibrium. The other dimensions must either change to restore a new organizational equilibrium or the proposed changes will be absorbed thereby preserving the status quo.

Several examples illustrate these dynamics:

1. Teaching teams are a structural reform designed to increase interdependence and enhance coordination at the school. As teachers begin to work in teams, however, new processes such as interactive meetings, conflict management, planning and problem-solving are needed to make the team work. Processes which worked in isolated classrooms in which interdependence and the need for coordination is now manifest become obsolete. Similarly, the structural shift encroaches on existing autonomy norms and makes entrenched rituals—such as the school coffee room "chats"—difficult to maintain.

2. State legislation creating a new occupational role "learning disabilities specialist" (LDS) affects the level of staff differentiation at the local school level—a structural change. At the same time, new processes may be developed to integrate the role into the classroom and to link the joint efforts of the LDS to those of the classroom teacher. But such efforts quickly run afoul of autonomy norms and "mythical" conceptions that school is where a single teacher and group of students interact for the purpose of learning. This problem is alleviated if the LDS works outside the classroom and periodically takes groups of children away from the classroom for special sessions.

3. State resources that are allocated for staff development purposes often upgrade the skills of individual teachers. As teachers begin to develop new skills, such as working closely together in instruction, the structure of the classroom and of the school may be affected. Additionally, collaboration violates strong norms of teacher autonomy and may run counter to existing community expectations for school configurations (i.e., self-contained classrooms) and the learning process.

4. A shift in beliefs which suggests that children learn best when they are given considerable discretion in selecting instructional tasks changes the structure and processes of schools. New processes are suggested by this shift as large group presentations give way to working in small groups and with individuals. The basic structure of the classroom may vary as new roles (e.g., aides) are introduced to help individualize instruction.

Once again, environmental changes or pressures produce alterations in the structure, processes, and symbols of educational organizations. As one dimension

changes, it affects the others. In turn, these other dimensions must change or pressures will be exerted on the original alteration to conform to the pre-change pattern, thus restoring the equilibrium.

The interplay between the three dimensions of school organizations suggests that if changes are to be made successfully as schools adapt to environmental pressures, attention needs to be given to internal structure, processes, and symbolic characteristics as well as their relationships. This line of reasoning suggests an interesting proposition: insensitivity to these internal needs and a lack of integration among the three important aspects of organization—both conceptually and operationally—have contributed to the high failure rate of previous change.

Toward a Three-Dimensional Contingency Theory

Recent work in organizational theory emphasizes that there is no "one-best-way" to organize. Effective organizations are those that have configured work patterns and processes to fit contingencies in the external environment or the technical process (Burns and Stalker, 1961; Lawrence and Lorsch, 1967; Thompson, 1966).

In the past, the contingency approach has been thought-provoking but falls short (in terms of empirical evidence) of specifying and elaborating the relevant contingencies. And, even less has been done to explore the linkage between these contingencies and the three organizational characteristics suggested herein. Where such explorations have been made, the tendency has been to focus either on process or structure—not both. The symbolic side of organizational life, for the most part, has escaped the scrutiny of contingency theorists.

The three dimensional conception of organizations provides a basis for elaborating contingency theory in two important ways: (1) by incorporating symbolic characteristics into the environment-organizational relationship and (2) by viewing each of the three aspects of internal organization as important contingencies for one another.

The first contribution of the three dimensional conception of organization is an expansion of contingency theory to include symbolic characteristics. The traditional contingency ideas specify that successful organizations will adopt structures and processes to fit environmental demands. Successful organizations in complex, uncertain, turbulent environments will be those with highly differentiated, well integrated structures, and highly sophisticated processes for making decisions and resolving conflicts. By contrast, successful organizations in simple, certain, stable environments are those that exhibit relatively undifferentiated structures and that achieve integration traditionally through the hierarchy of authority (Lawrence and Lorsch, 1967).

These contingency ideas were developed by work conducted primarily in business organizations. Such organizations function in an environment where the technology is well developed and the goals relatively clear. In contrast, educational organizations function in a less technical "institutional" environment where both the purposes and techniques are diffuse and not well developed (Meyer and Rowan, 1977; Derr and Gabarro, 1972).

In institutional environments, such as schools, successful organizations may be those that conform to broad societal, or institutional expectations. Among educational organizations the most successful will be those that maintain and conform to a public image of what schools should be. Thus, ability of school personnel to use symbolism effectively is critical (Meyer and Rowan, 1977).

In short, the important environmental requirements may be different for educational organizations than for organizations that operate in more technical settings. The key contingencies may be highly symbolic. Successful educational organizations will therefore configure their internal structures, processes, myths and ceremonies to fit these symbolic cues from the environment. Where pressures for change and reform provide signals that run counter to these broad societal expectations, the "rational" organizations will rely heavily on their internal symbolic characteristics to project the "appearance" of change and reform (Campbell, 1971). Mandated guidelines for revamping evaluation procedures will result in new evaluation "rituals"; radical new approaches to education will result in new school-site "myths" that shroud a traditional reality (Smith and Keith, 1971).

The second contribution of the three dimensional view of organizations is a revised perspective of internal organizational dynamics. Traditionally, organizational structure and processes have often been seen as highly related. New structural forms such as team teaching, differentiated staffing and individualized instruction require new approaches to decision-making, evaluation, conflict resolution and coordination. Similarly, the adoption of new patterns of communication or new evaluation procedures may require new structural patterns for their continued support and maintenance. Structure and process thus represent important contingencies for each other. The successful implementation of new structures requires that some attention is given to the important aspects of processes and vice-versa.

The addition of a third organizational dimension—the symbolic side of organizational life—adds another important set of internal contingencies. Not only will the development of new structures require the creation of new processes, structural innovations may also require the development of new myths, rituals, ceremonies, norms and beliefs. Adding aides and volunteers to the classroom, for example, undercuts existing myths (the autonomous professional), beliefs (only teachers can teach) and norms (parents and community members should only be indirectly involved in instruction). Assuring that the structural change is fully implemented may, therefore, be as dependent on the creation of new myths and rituals as it is on the development of new patterns of communication, decision-making and problem solving. In the same way, the addition of a parent advisory council to a school's decision structure requires that new myths, rituals, and beliefs are created to supercede the old. Merely providing process training to the group members does not attend to the important symbolic changes that must be made if the structural reform is to succeed.

In sum, the internal dynamics of organizations are maintained by an equilibrium among structure, processes and symbols. Each dimension must be aligned to "fit" the others. If any one changes or is changed it upsets the organizational equilibrium thereby producing conflict, stress, and instability. In the absence of

changes in other dimensions, pressures are exerted to "unchange" the change, thus restoring the prior equilibrium.

Imbedding the three-dimensional conception of organizations in contingency theory produces some thought-provoking modifications of the theory. It emphasizes that schools may face a different set of environmental contingencies than those experienced by many other organizations. It emphasizes the internal contingencies among the three organizational dimensions themselves. Finally, it affirms that the environmental contingencies and internal dynamics are highly related. As schools respond to pressures for change and reform these considerations provide an important set of organizational constraints as well as opportunities. Successful adaptation is heavily dependent on configuring work patterns and processes to fit the relevant internal and external contingencies.

The Importance of Resolving Internal Organizational Problems

From many fronts, there are pressures for change and reform in schools. For the most part, these pressures are external and are not generated from within. At the same time, the central argument of this paper suggests the key to whether these intended changes succeed or fail is the internal configuration of educational organizations. In the past a general insensitivity to these internal configurations and needs has contributed to or caused the failure of many educational innovations. In the future, however, the problems may arise not so much from insensitivity but from an inadequate or fragmented conception of schools as organizations. As long as education takes place in an organizational setting the internal organization will need to be reckoned with. Without such attention nothing can be expected to result from change and reform efforts. Without knowledge of internal characteristics and how they are related, reform of the educational enterprise cannot be controlled. Nor can we affect the cause and direction of change. Despite the intensity of economic, judicial and political pressures in the 1970s, schools may leave the decade with essentially the same characteristics as they entered—even where the status quo may not reflect the current societal desires.

There are both real and potential costs to the society, parents, children and school personnel if we fail to effectively deal with the internal organization of schools. Following are some postulates, based on the framework, which serve to predict the natural responses of educational organizations to external pressures unless we are able to alter internal characteristics in accordance with various contingencies. At the present time, our knowledge of change dynamics in educational organizations is insufficient as is our knowledge of effective intervention strategies and techniques. Although the amalgamation of contingency theory ideas and the image of school organization which integrates structural, processual, and symbolic characteristics is in an embryonic developmental stage, it can be used to generate these postulates about change dynamics and intervention strategies.

Implications for Change and Reform: Some Postulates

1. The internal organization may experience conflicting environmental signals (e.g., from parents as opposed to the State Department of Education). Some demands for increased accountability, for example, may direct too much attention to changing the instructional process. Other forces may insist on maintaining a "traditional" instructional medium. Direct inspection of instructional activities may generate unwanted information for one of the environmental groups which leads only to conflict and decreased confidence (by some) in the instructional process.

2. The greater the discrepancy (conflict) between proposed changes between two rival environmental forces, the more likely it is that schools will make only symbolic changes. Competency-based instruction, for example, will be approached with a flurry of forms, rituals, position papers, and ceremonies. Behind this symbolic facade, however, teachers, students and administrators will operate as they did before the change was "implemented."

3. The greater the discrepancy between existing social expectations and the intended changes or reforms, the less likely it is that the changes will be fully implemented. Any external reform effort, for example, that changes the location of school, specifies new curricula, or involves alternatives in the credentialing function or otherwise changes the roles of various educational functionaries, will encounter considerable resistance.

4. On rational grounds, schools will find ways not to implement changes or reforms that, because they run counter to accepted social symbols, may jeopardize their continued levels of support. Merit pay for teachers, as one example, would be resisted because the issues involved in its implementation might expose the current inadequacies of judging teaching merit or undermine the myth of teachers as highly trained professionals. Controversy surrounding the issues might, therefore, undermine existing community support.

5. Changes in any one aspect of a district, school or classroom will generally push other forces to overcompensate and maintain the existing organizational equilibrium. Changed beliefs about the instructional process will be accompanied by structural and processual issues that are intended to restore the old belief system.

6. As a corollary, changes and reforms that deal simultaneously with all three aspects of the internal organization will be more successful than those that deal with one or two. Wisconsin's Individually Guided Elementary Education (IGE) program, for example, incorporates structural, processual, and symbolic features. The multi-unit structure, new evaluation and instructional processes, and an underlying scheme of myths, values, and beliefs provide an integrated approach to organizational change. The IGE approach even includes strategies for relating to and influencing the external environment (Klausmeier, Rossmiller and Saily, 1977). Such integrated reform efforts should be among the most successful.

7. The most promising targets of change and reform efforts may be broad social expectations and myths. Where these are changed it may make it easier to promote other structural and procedural improvements. If communities were to

believe for example, that learning occurred best in the community itself, attempts to scale the walls of the classroom would be more easily implemented.

Implication for Intervention: Some Postulates

Several postulates can also be developed about attempts to intervene in order to alter existing organizational patterns in some desired direction. Inappropriate intervention may be as important a reason for the apparent failure of planned organizational-educational changes as the lack of conceptual knowledge. Following are some postulates about intervention strategies:

1. Of the three organizational dimensions it is easiest to intervene into the structural sphere and most difficult to change symbols. Thus, educators frequently respond to external pressures for reform by setting up new roles, rules, policies, facilities, (new machinery) which are infrequently implemented. A more profound intervention would attempt to improve how the work is accomplished, such as developing more effective meetings. Few interventionists have tackled the most inexplicit and buffered parts of the core system—its symbols.

For example, consider an intervention attempted in a small school district in California. The ostensible mandate was to review the personnel evaluation system and then to jointly decide (consultant and client system) how to best improve it. Most of the suggestions for improvement coming from the group were structural: change the rating forms, implement a management-by-objectives orientation, visit more classrooms, build in pre-conferences, include peers in informal discussion groups. Some were processual: provide training on giving and receiving negative feedback and make the evaluation a two-way communication. Only once did the group surface the organizational myth which everyone later agreed was at the heart of the issue: do not make information available to the Board which will be used detrimentally against an individual. Thus, the real issue was how to separate out the formal and public part of the evaluation from the informal and voluntary learning-from-experience dimension.

2. Because the more the three aspects of internal organization are balanced and integrated the more successful the change will be, one of the tasks of interventionists should be to explicitly integrate organizational structure, processes and symbols in order to maximize the targeted impact.

In the above example, once the operating myth was made explicit, the structures and processes supporting it were designed into an integrated plan. It is supposed that the newly integrated personnel evaluation system might now make a difference.

However, the above approach is inherently conservative because matching structures and processes to existing myths serves only to buttress the status quo. One way to get at real reform is to import into the organization new persons and myths to support new structures and processes. For example, newly trained teachers preferring intervention and inter-dependence could be imported to work in a team teaching structure wherein collaborative processes were employed. Or, another option is to intervene into the symbolic sphere via educational and value probes and then adjust the structures-processes to fit the newly created myths.

3. Some changes need not be profound and should be designed to impact superficially on all or one of the three situational variables. Some OD processes are taught superficially as skill training. They have their own value which can overlap into the classroom, the school or other non-professional aspects of a person's life. Communication effectiveness, problem-solving and conflict resolution are examples of skills which are useful for their own sake and which have some organizational overlap.

One author, for example, helped a group of elementary teachers decide how to structure themselves for the forthcoming year. They chose a team teaching K-3 multi-aging structure. Yet, their definition of teaming was simply to exchange children rather than to plan or teach together. Moreover, they did not seek to improve their processes. It turns out that such a structure supported an underlying myth: teaching very young children is a somewhat boring and taxing job (like babysitting) and we should make it as interesting and enjoyable as possible. Thus, the new structure would give teachers an occasional break, provide a variety of teaching (grouping) situations, allow them to interact with a partner they enjoyed and, because they were happier, probably impact for good on the children as well.

4. The intensity and direction of intended change depends in large part on these three dimensions as they interrelate internally within the interventionist as well as within the client organization. Or, what ultimately happens will also depend on the structure, processes and symbols of the reformers and change agents.

New legislation may allow for cosmetic effects because this permits the public servants to appear to their electorate as if something is happening while still retaining the powerful support of the teacher organizations. A change agent may be content to perform process-structure interventions which support the status quo because his own value is: I should get as much remuneration as possible from this client system so I am willing to collude with them according to some very broad limits (ethics) in order to prolong the contract. Examples are legion of academics, consultants, legislators, agency reviewers and inspectors willing to intervene superficially or into only one mechanism in order to meet their own objectives.

These propositions concerning the dynamics of change and attempts to change organizational patterns in schools are not exhaustive. They are illustrative of the direction in which this line of thinking may carry us in research and in action.

Research Implications and A Preliminary Research Agenda

This paper has outlined the rudimentary features of an integrated model of educational organizations that includes structural, processual and symbolic characteristics. Some speculative thinking suggested how these three aspects of organization might be related and how these, in turn, might be related to the

external environment. One important thrust of the paper, however, is to outline some needed research in the area of organizational change and intervention. Without such research, large and expensive projects for reforming education will undoubtedly experience difficulties—from which we will learn very little.

Within this paper, it is impossible to lay out a complete research agenda in the area of school organization. But based on the preceding discussion we feel comfortable, if not compelled, to outline some ideas of some preliminary steps that need to be taken. Essentially, three general areas of inquiry seem logical: (1) developing theory and concepts, descriptive studies and measurement tools, (2) developing and conducting some relevant experiments, and (3) developing and conducting some small-scale field interventions. These three areas are not seen as necessarily sequential and this makes it difficult to establish priorities. Nevertheless, the following order is a logical order of research leading to theory.

Developing Theory, Description and Measurement

1. The three dimensional perspective on educational change presented in this paper requires considerable development to refine the concepts, to specify relationships, and to identify the conditions under which these would be expected to hold.

2. The literature on change in educational organization could be organized into the three dimensional framework. This reorganization of the literature could be used to develop hypotheses for future research but could also produce some interesting new interpretations of past change efforts.

3. New developments in sociology, psychology, organizational theory and management science need to be incorporated into theories of educational change. The three dimensional model provides an integrating framework. Drawing upon the other behavioral sciences would be a useful way to increase the elegance of the model.

4. At the present time case studies of elementary schools, high schools and school districts could be useful in clarifying and refining concepts in each of the three domains presented in the model. Case studies would also be helpful in identifying the interactions between structure, processes, symbols and the various ways in which an equilibrium among them is maintained in various settings.

5. In addition to intensive case studies there is room for some surveys at various educational levels to describe existing organizational patterns and processes in schools. This need is especially crucial at the high school level.

6. Conduct some comparative studies. Many of the issues identified in the paper can only be explained by comparing organizations in varying environments.

7. New outcome measures are needed to help determine educational effectiveness. At the present time attention is concentrated almost exclusively on learning outcomes neglecting other possibilities such as participant morale, support for schools, or more precise measures of learning outcomes than are now yielded by conventional standardized tests.

8. Work is needed to develop stronger measures of formal structure, organizational processes, and myths. The need is most critical in the symbiotic area of myths, values, ceremonies and rituals. Our current knowledge of the place of these in the day-to-day life in schools is almost exclusively speculative generated to explain non-findings in various research studies.

Designing and Conducting Experiments

As noted earlier, there are a number of environmental pressures—political, economic and judicial—that are theoretically disruptive to the organizational equilibrium that schools have established between structure, processes, and symbols. As schools respond to statewide reform mandates, desegregate under court order, confront the problems of shrinking enrollments, a careful investigation of the strategies and techniques that are used to alter or maintain the internal equilibrium could add to the knowledge of organizational change. The three-dimensional theory could be used to predict possible consequences under different environmental conditions. Some patterns of structural, processual, and mythical characteristics may be more effective than others. If ample variation can be found in the organizational configurations of schools and their relevant environments and if some more sensitive measures of organizational effectiveness can be developed, then we may be able to develop better ideas about the organizational-environmental fit.

Where field experiments are impractical or impossible, laboratory experiments might provide insights into how teachers, administrators, and other participants would react to change under a variety of simulated conditions. Laboratory experiments could help isolate important interactions between structure, processes, and symbols.

Designing and Evaluating Interventions

In addition to natural experiments, assessing the impact of planned interventions on the three components of school organization would be instructive. Such experiments could be designed to assess the impact of purely structural, processual, or symbolic interventions. In addition, it would be useful to design, conduct, and evaluate interventions aimed at all three aspects of school organization.

Conclusion

As schools confront the various pressures for change in the 1970s there is considerable need for knowledge that will provide guidelines for thoughtful adap-

tation. Similarly, consultants who assist schools in making changes successfully need a more sophisticated understanding of the various aspects of school organization—structure, processes and myths—and how they relate to one another. Without such knowledge and understanding, many of the difficulties involved in educational change efforts of the 1960s will undoubtedly be repeated in the 1970s. Educational organizations are highly complex—even more so than we had previously thought. The primary purpose of this paper has been to develop a way to capture this complexity. Hopefully, the three-way contingency framework will stimulate some additional thinking and research. Even more hopefully it will lead to some better approaches to organizational change in schools.

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Chapter 5

Organizational Scale and School Success

James W. Guthrie

Introduction

Over the last half century, the school and school district consolidation movement in the United States has resulted in ever larger educational organizations. This metamorphosis was propelled historically by those who argued that larger schooling units would enhance economic efficiency and bring added educational benefits to students (Callahan, 1962; Tyack, 1974). Contemporary forces provide little prospect of dampening the movement's momentum. Enrollment declines and intensified fiscal pressures are frequently viewed as justification for closing and combining small and allegedly costly schools (Shapell, 1978). Also, present day education finance reformers sometimes propose school district consolidation as a means of achieving a more equitable geographic distribution of taxable resources (Pincus, 1974).

The trend toward ever larger units of school "production" continues in the absence of persuasive analyses that the movement has achieved the objectives held either by its past or present advocates. Indeed, the trend persists despite evidence that it may have produced few, if any cost savings or educational gains and may have damaged citizen allegiance to and lay control over public schools.

Even in the face of diminished or stable enrollments, schooling in the United States is likely to remain a large and costly undertaking for decades to come. Similarly, regardless of scholarly findings concerning instructional effectiveness, the larger society is likely to continue viewing schools as one of the major "engines" for socializing and training the young. In short, schools have been and are likely to continue to be too important to the individuals who attend them and

the society which supports them simply to be subjected to the "tug" of those who would subordinate significant questions of size, organization, and governance to more transient matters of economic efficiency and fiscal equity. At a minimum, the empirical relationship for schools between organizational scale and economic efficiency justifies far more intensive and objective examination than has heretofore been the case. Perhaps more important are questions regarding the relationship between school organizational scale, on one hand, and student outcomes and public control and participation, on the other.

In discussing public schools in the United States there exist three major organizational levels, the school district, school, and classroom, at which policies and practices regarding scale are most likely to be important. The mixture of functions varies for each level, and an assessment of the effects of size needs to separate each tier for analytic purposes. For example, school districts are significant as governmental units primarily because of their financial and political functions. School *district* size is not as likely to influence instructional outcomes. In contrast, *school* size may be connected with instructional outcomes but its association with revenue and political conditions is probably less than for districts. Also, the sequence and intensity of effects may differ for elementary schools relative to secondary schools. Lastly, class size may principally affect instruction and secondarily be connected with finance and politics. Much research conducted up to now confounds the size effects of school districts and schools. The discussion which follows attempts to separate the issues where possible. However, the construction of future research agendas regarding scale effects should be particularly careful to distinguish between the various levels and functions of organizational units.

The purpose of this article is to explore unanswered questions regarding the scale of schooling. The paper proceeds (1) to describe the historical trend toward large organizational units for schooling, (2) to sample the evidence regarding the economic, instructional and political effects of such a movement, and (3) to suggest a series of research strategies and questions deserving of attention in order better to exert influence over the future.

Bigger Is "Better"

The evolution of ever larger schools and school districts is traced in detail in other publications (Callahan, 1962; Tyack, 1974). It is sufficient here to describe the circumstances in brief.

The school consolidation movement perhaps reflects one of the most awesome and least publicized governmental changes to occur in this nation during the 20th century. Table 1 below displays the number of public schools and school districts in the United States from 1930 to 1972. School districts declined during this period almost eightfold from approximately 128,000 in 1930 to less than 17,000 in 1972. Similarly, the number of schools decreased from approximately 262,000 to 91,000 over the same period, an approximate threefold reduction.

The overwhelming proportion of the decrease in schools is accounted for by the elimination of one-teacher schools. From 1930 to 1972, the stereotypical "little American school house" faded by a factor of ten, from 149,000 to less than 1,500.

TABLE 1

Year	School Districts	Schools	Schools (1 teacher)
1930	128,000	262,000	149,000
1940	117,000	210,000	114,000
1950	84,000	152,500	60,000
1960	40,000	117,700	20,000
1970	18,000	91,400	2,000
1972	16,960	90,800	1,475

Table adapted from data provided in 1974 edition of the *Digest of Educational Statistics* (Washington, D.C.: Office of Education, National Center for Educational Statistics, 1974).

Shrinkage in the number of schools and school districts occurred over a period of time during which the nation's total population increased 85 million from approximately 123 million to 208 million. Enrollments increased over the same span from 25.7 million students K-12 to 50.6 million (National Center for Educational Statistics, NCES, 1973). The interaction of population growth and school district decline resulted in larger organizational units. For example, in 1930, the mean number of students enrolled in U.S. school districts was approximately 200. The mean school size was less than 100 pupils. There was then and still is, wide variation. There remains today in the U.S. a few districts with more school board members than pupils. At the opposite extreme are gargantuan units such as New York City and Los Angeles. Nevertheless, by 1972 mean school district size had increased to almost 3,000, a fifteen-fold increase in less than fifty years. Similarly, mean school size rose fivefold to approximately 550. (The average secondary school had increased in size to approximately 1,000.) The full numerical effect of this consolidation is difficult to assess without more complete data regarding the range and dispersion of enrollment by school. A relatively small number of extraordinarily large units can distort a mean figure. We do know, however, that students attending one-teacher schools, the modal experience in 1930, had been reduced to an insignificant number by 1972.

The Justification

What persuaded the American public and its officials to undertake such a dramatic alteration in the size and form of one of its major institutions? The contention of consolidation proponents was two-pronged. They argued that the educational opportunities of students in small schools and small school districts would be measurably enhanced for at least two reasons. Small schools, when

collapsed into larger units, would have classes of sufficient size to justify more specialized personnel. For example, the high school which could not afford a chemistry teacher for only five pupils could combine with two schools of like size and, for fifteen students, hire such a teacher. Also, larger schools and school districts, it was argued; could more easily attract higher quality teachers, both elementary and secondary.

Equally effective as an argument for consolidation was the "obvious" proposition that larger districts and schools could be more cheaply operated. By collapsing several schools or school districts into a single organization, the positions of one or more administrators, librarians, custodians, etc., could be eliminated.

These arguments for economic efficiency and instructional effectiveness were put forth widely by professional educators in the first quarter of the 20th century. This took place at a time when educational administration was a fledgling field anxious to demonstrate its rigor. By making the case for higher productivity, school administrators hoped to share the mantle of respect so widely enjoyed by the "efficiency" experts then popular in the private sector. The consolidation proposals of this former era were aimed primarily at rural districts. Their residents, on occasion, protested the loss of an institution they valued but their opposition was relatively ineffective when pitted against the professional expertise and efficiency arguments of school administrators and leading educational spokesmen. If bigger was better in the private sector, it was assumed to be the same for schools. Subsequent generations of administrators were inculcated with the same ideology. The underlying tenets were almost never subjected to systematic examination. Thus, the concept of larger is cheaper and better became the professions' conventional wisdom.

Beginning in the early 1970's, school consolidation advocates adopted a different justification. Post World War II school enrollments peaked in 1970 (Fishlow, 1977), and, thereafter, school districts, ironically, now mostly in urban areas, began to experience "excess capacity," classrooms with empty desks and buildings with empty classrooms. Proponents of efficiency argued that economies could be effected by closing schools and collapsing students into fewer buildings. Cost savings from items such as reduced utilities, maintenance, and administration would, again, be obvious. Also, school closings would enable districts to operate larger schools and thereby permit continued employment of a variety of specialists. Thus, as was the case fifty years before, the U.S. public prepared itself at the onset of the last quarter of the 20th century for yet another round of school consolidation, all in the interest of saving money and better education.

Now, as before, questions arise as to efficacy of such a strategy. Do larger schools lead to better instruction? Do larger districts lead to scale economies? What about parents and citizens? Is public allegiance to schools influenced by the alteration of boundaries and increases in size? These are the kinds of questions on which attention is focused in the following section.

But Has It Worked?

In this section, an attempt is made to assess findings of appropriate empirical inquiries regarding consequences of school and school district scale. Even though in some instances research results apply to more than one topical category of school scale consequences, findings have been separated into those which pertain to (1) economic efficiency, (2) instructional outcomes, and (3) political effects. In each category, those studies which apply to schools are distinguished from those in which school districts were the unit of analysis. Also, no claim is made for having exhausted all the appropriate research studies. The body of literature relevant to this topic extends into every social science area, as well as fields such as business administration, industrial engineering, and educational administration. This article is able only to sample from among a huge number of such studies. A complete review and analysis of related literature should be commissioned.

Economic Efficiency

From 1930 until 1972, the time frame used previously for calculating numerical consequences of school closing, expenditures in U.S. public schools increased from less than $90 to almost $1,000 per pupil (NCES, 1973). Even when discounted for inflation, this is more than a fourfold increase. School expenditures grew faster than Gross National Product during this period. It would appear that, in the aggregate, the consolidation movement had litfle success in dampening costs. Purported measures of service quality, such as class size and number of specialized personnel, evidenced substantial growth over this period. However, there exists scant evidence to suggest that pupil performance increased. Thus, it is unclear whether school cost increases led to increases in "quality" or output.

The potential complexity of the question of scale is illustrated by the analyses of Elchanan Cohn (1975) in "A Proposal for School Size Incentives in State Aid to Education." Cohn reports that "Although there are differences in methodology and ultimate results, most of the studies indicate a U-shaped relationship between per-pupil costs and school size, measured by enrollment. It follows that most schools are either too large or too small, resulting in considerable waste of resources to society." (Cohn, 1975, p. 214). To determine the extent of size economies, Cohn regressed school cost data on a quadratic function of school enrollment and a collection of other variables included in the regression equation to account for interdistrict cost differences due to input-output variation. Cohn derived the following table (table 2) by this method.

Thus, the optimum school size based on cost factors (secondary school) according to Cohn is 1653 pupils.

There are, however, other significant dimensions which should be assessed in each case where a consolidation decision is under consideration. School costs are influenced by many forces, e.g., labor market conditions, regional geography,

TABLE 2
ADJUSTED COST FOR SELECTED SCHOOL SIZE

School Size	Adjusted Unit Costs
100	372.84
500	314.73
1,000	266.25
1,500	244.63
1,653	238.09
1,750	242.88
2,000	249.90
3,000	340.90

client tastes, and educational fads. Consequently, an effort to deduce the existence of school and school district scale economies needs to be more specific than Cohn's study permits. Regardless of desirability, such precision is hard to come by. Studies of scale economies have seldom been conducted in a comprehensive fashion. For example, the typical analysis fails to distinguish, either by population density or some other measure, between school operating costs in rural and in urban areas. The distinction is crucial. In a city, where children can walk to school, it may be financially cheaper to operate one school, even it if is larger, than to operate four. Cost savings are possible on items such as utilities, maintenance, and administration. However, in a rural setting, one large school may not be cheaper than several small ones; the critical factor is transportation.

A recent NIE-sponsored study of rural school district consolidation asserts that the overwhelming majority of scale economy studies using rural schools have failed adequately to take transportation costs into account:

> When transportation diseconomies are included in the determination of overall educational costs in rural areas, the economies from consolidation tend to decrease markedly, or vanish altogether. As transportation costs increase, small school districts in sparsely settled areas are becoming even more economically advantageous. (Sher, 1976, p. 6.)

A recent economic analysis of school costs in rural areas buttresses the above conclusion. As reported by Sher (1976), White and Tweeten examined data for Oklahoma school districts and concluded, absent any consideration of transportation, that the optimum school district size was 800 pupils. When transportation costs were included in their analyses, optimum district size declined to 675 (White and Tweeten, 1973).

No study of scale economies of rural schools has attempted to account for increased student transportation time as a consequence of consolidation. In many rural areas collapse of small schools into larger units has resulted in students riding the school bus up to 60 minutes in each direction. If a price were attached to their time, cost savings in larger rural districts might decline substantially.

Another frequently held virtue for rural consolidation is the prospect of cost savings derived from centralized purchasing. By pooling purchasing power and buying in bulk quantities, rural school districts are presumed to benefit from lower unit costs. This, too, is frequently fraught with diseconomies. What is gained by bulk purchasing is subsequently lost by increased school district distributional costs.

After assessing these conditions, Zymelman writes:

> Administrators should carefully consider the full costs of central purchasing because savings might not exceed the added costs of distribution. There are also possibilities of delays and loss of flexibility involved in central purchasing. Finally, there is the use of scarce administrative manpower to manage a purchasing and distribution system that could be operated in the private sector. (Zymelman, 1973, p. 274.)

Thus, for rural districts, the evidence regarding economies of scale is not persuasive. Cost savings which are held to result from larger size are frequently eroded by added expenses of transporting pupils and supplies. However, it may still be the case that, absent problems of geographic sparsity, bigger operating units, schools and school districts, might be more economical.

School scale economy studies regarding urban areas have focused upon school district rather than school size. Analyzing urban school districts' operating costs is made difficult by the environment of cities. For example, city building maintenance and repair costs are influenced greatly by higher rates of vandalism. Such a factor is difficult for analysts to "control" when attempting to identify scale economies. Because of such uncertainty, scale economy studies of city schools tend to concentrate upon two factors, administrative costs and purchasing of material supplies.

A study by Kahn and Hughes (1970) utilized data from a sample of 1,800 school districts and found that administrative costs were inversely related to district enrollment. The proportion of district expenditures utilized for administrative purposes ranged, on the average, from 8.8 percent in districts of less than 300 students to 3.8 percent in districts with enrollments exceeding 25,000.

Interestingly, Kahn and Hughes' analyses uncovered no administrative economies after districts reached 25,000 pupils. This finding is confirmed by more recent figures for large unified districts in California. Table 3 displays administrative costs for five of California's big city school districts. Los Angeles, by far the largest district in the state, is second only to San Francisco in the proportion of its total operating budget allocated for administration. Oakland and San Diego, much smaller districts, have lower administrative costs. At least for large urban districts, administrative economies appear blurred by factors other than scale.

Whatever the findings of such scale economy studies, they do not promise much hope for reducing school costs. First, the type of study is particularly subject to inaccuracy. For example, the data displayed in table 3 were taken from state reports; however, careful analysis of the figures for one of the five districts revealed that 200 teachers were assigned to administrative duties but their salaries

were charged to "Instruction." Thus, administrative costs were substantially understated. Even if the figures were accurate, administration accounts for such a small percent of total expenditures as to make great savings highly unlikely.

TABLE 3

SCHOOL DISTRICT ADMINISTRATIVE COSTS

District	Percent of Total Current Expense Allocated to Administration
Los Angeles	4.51%
San Francisco	4.54
Oakland	3.93
San Diego	4.02
Long Beach	4.20

Findings derived from data presented in California State Controller's *Annual Report of Financial Transactions Concerning Districts of California: Fiscal Year 1975-76.*

No systematic data are available regarding the cost saving advantages of centralized purchasing in large districts. Quantity discounts to school districts are readily acknowledged by vendors. The extent to which such savings are eroded by intradistrict handling and distributional costs is not known. The supply system of New York City's schools and San Francisco are generally renowned as inefficient. In contrast, the central warehouse for the Los Angeles Unified School district is often cited as a model for private industry.

Evidence in favor of cost savings associated with larger size schools and school districts is, at best, ambiguous. In the instance of rural schools, the setting where consolidation has been most dramatic, it is exceedingly unclear that efficiency favors larger organizations. Transportation appears to make the difference. In urban areas, the evidence is thin, but slightly favors the view that larger districts, up to 25,000 students, have lower administrative overhead. However, there is nothing to suggest that huge districts, the size of many cities, save money for the taxpayer. A great deal more must be known before advocates of greater district size can easily claim that, in terms of dollars alone, their way is less costly.

Instructional Outcomes

Aside from whether or not larger school units are more economical to operate, what is their effect upon students? As mentioned previously, traditional advocates of consolidation built their case on grounds that larger schools would lead to expanded course offerings, higher quality teachers, and instructional personnel with greater specialization. These conditions, in turn, were presumed to benefit students. Perhaps no proponent of such reforms was more influential than James

Bryant Conant. In his bestselling book, *The American High School Today* (Conant, 1959), the former Harvard president and chemistry professor, searched for the "lever" which would make America's secondary schooling more effective. By writing in 1959, during the period when the public was still alarmed by initial Soviet space successes, he found an eager audience for his improvement prescriptions.

Conant had advice to offer on a number of school dimensions, but he believed enrollment size to be the major determinant of quality. He wrote, "The number of small high schools must be drastically reduced through district reorganization. Aside from this important change, I believe no radical alteration in the basic pattern of American education is necessary in order to improve our public high schools" (Conant, 1959, p. 15). For Dr. Conant, bigger was better. This philosophy was consistent with the solution frequently posed to meet another problem of the 1950s and 60s, the necessity to desegregate school systems. Large schools, sometimes called "Education Parks," were suggested as means to overcome the effects of neighborhood racial segregation. In the decade which followed, the number of districts was sliced by more than half, from 40,000 in 1960 to 18,000 in 1970. Few would doubt Conant's influence. However, a question remains as to the validity of his recommendation.

For one group of students, larger schools do appear to make an important instructional difference. We refer here to badly handicapped students for whom the majority of small districts are incapable of providing adequately. Children suffering from conditions such as deafness, visual impairment, and mental retardation require specialized teachers and equipment in order to benefit from schooling. Large school districts generally have sufficient numbers of students in need of such specialized services to bear the added instructional costs. For decades, school districts in large cities have been magnets for handicapped students because of their specialized offerings. Regional institutions have recently begun to fulfill a similar function for rural areas. The existence of scale economies and instructional benefits in these instances appears reasonable.

Do schools and districts of larger size aid the performance of "normal" students? Here again, the evidence is ambiguous. The advantages of size so strongly proclaimed by consolidation advocates are seldom supported empirically. Even Conant, who believed strongly that a high school with a graduating class of less than 100 students was disadvantageous, was unable to prove his point. In a recent re-analysis of Conant's data, Jonathan Sher demonstrates that small high schools, less than 100 in their senior class, fared as well by Conant's rating scheme as did large schools (Sher, 1976, p. 20).

A more recent study of Vermont's high schools also fails to support Conant's magic "100" figure. Vermont has 59 public senior high schools; 34 have less than 100 in their senior class. Sher reports that of the ten Vermont high schools with the highest percentage of graduates entering college, six were small, less than 100 seniors (Sher, 1976, p. 22).

Conant's data are almost two decades old and Vermont is a small and unusual state. What do other empirical inquiries reveal regarding the effects of school size upon student performance? In studies which have controlled for student charac-

teristics, e.g., socio-economic status, small schools have the edge. The Coleman team reported a negative correlation at the 12th grade between size of graduating class and student verbal achievement. Each increment of 200 students was found to be associated with an approximate one-fifth diminution in grade level achievement (Coleman, 1966). Kiesling also found a negative correlation between school size and student achievement (Kiesling, 1962). A recent study generally judged to have been carefully conducted (Summers and Wolfe, 1975) found a positive correlation between attendance in small schools in Philadelphia and higher achievement. This was true both for elementary and secondary schools. The trend of findings continues for many more studies of student achievement, both those utilizing cross-sectional and longitudinal data.

One of the most complete studies of the effects of school size upon student outcomes was conducted by Barker and Gump (1964). These researchers took as their focus student participation in a variety of leadership and extra-curricular activities, e.g., student government, journalism, music, and athletics. Their finding is that students in small schools are strongly advantaged over their big school peers. Barker and Gump report that student participation in non-academic or extra-curricular endeavors reached its peak in schools with 61 to 150 students. In small schools, the proportion of students participating in extra-curricular events ranged from three to twenty times as great as for large school settings.

Barker and Gump offer an attractive explanation for their finding. They contend that there exists but a limited number of student positions in a high school calling for participation and leadership. In a small school each student is substantially more visible and thus under greater teacher and peer pressure to fill one or more of the student roles upon which the extra-curricular life of the school depends, student body president, cheerleader, editor, etc. In large schools, the greater number of students available to fill such roles relieves the pressure and reduces the participation probability for any one student.

There exists an indirect route through which school size can affect student outcomes. We refer here to the impact of school size upon school employees. Does the scale of a school influence teacher morale or productivity? This, too, is an area worthy of substantial additional study. However, there are research findings that suggest small schools may be associated with lower rates of teacher absenteeism and, possibly, with higher teacher morale. Winkler (1977) reports from his analyses of Wisconsin and California data that teacher illness leave is positively associated with school size; this is particularly the case with Friday-Monday absence which can be interpreted as a reflection of teacher job dissatisfaction.

From the social sciences there exists a body of literature on the effects of organizational size in settings in addition to schools. Also, psychologists and sociologists have conducted research studies on the effects of population density and crowding. The early interpretations of both these lines of analysis were to the effect that workers and residents in small scale organizations and settings were more productive and exhibited less anti-social and neurotic behavior. Much of the research has been re-interpreted by Jonathan Freedman (1975) who asserts that higher population density may not be the cause for the distasteful conditions

previously blamed upon it. Whether Freedman or his predecessors are correct, we cannot say; but this type of psycho-social analysis is badly needed on the topic of school scale.

Though there certainly exists no definitive study regarding the effects of organizational scale upon schooling outcomes, there is sufficient evidence to suggest that the quality of school life for students is not always made better by attending schools that are bigger. At the least, from available research findings, one would have to counsel school decision makers to examine closely their motives for consolidating or closing small schools, be they situated in rural or urban settings.

Participation Outcomes

Increased organizational scale appears capable of altering school relations on at least two other important dimensions, parent participation in the life of their children's school and the general public's participation in school governance.

Political science research regarding school issues is relatively undeveloped. What little is known confirms the general perception that public participation regarding school matters is seldom intense (Wirt and Kirst, 1972). Participation does become more heightened during periods of community conflict. Subsequently, however, it reverts to its previous, usually low, level (Kelly, 1966). Whether or not this condition is a consequence of the widely-held view that schools and politics do not mix, reflects general public satisfaction or lack of concern with schools, or results from structural and procedural barriers to participation is not known (Guthrie, 1978).

Has the increase in school and school district size had any effect upon political participation over school matters? This is difficult to answer because of the intervention of a number of additional forces during the period of school consolidation. For example, it is widely held that increasingly school decisionmaking discretion has been withdrawn from local governing boards and exercised by state officials (Van Geel, 1976). To the extent to which this is true, it may have dampened public interest in local school governance. (Presumably, it may have had the counter effect of intensifying public conern for state government action on school issues.) Also, over the school consolidation period professional educator influence has increased. Professionalization may have contributed to a perception of citizen impotence.

In addition to the previously-mentioned confounding forces, structural arrangements for school governance may contribute to low citizen participation. Prior to the consolidation movement, each U.S. school board member, on the average, represented 250 constituents. This was in a time when the major portion of the American population still resided in small towns, and it is conceivable that a substantial amount of personal contact with one's elected local representatives was possible. Even if election turnouts were not high, knowledge of and access to decision makers was probably considerably easier than is the case today. Presently, school board members continue to be the most numerous category of local govern-

ment officials. However, the average school board member now represents in excess of 2,000 constituents. The opportunity for face-to-face interaction would appear to have been substantially reduced over that which existed a half century ago. Does this reduction in representativeness and political access matter? Do interest groups compensate at the local and state level for the absence of personal contact? At the moment, it is possible only to speculate about the answers to such questions.

Despite the availability of empirical findings on the topic of political access and participation, there has evolved a perception among public officials that schools have become too distant from their constituents. There have been three types of response to the problem. One is the so-called "accountability" movement wherein what schools do with the resources they are provided is supposed to be "audited" more precisely. The accountability movement has relied on numerous technocratic mechanisms and strategies adopted from the private sector, management by objective (MBO), PPBS, PERT, and intensified use of tests are frequent components of accountability schemes. Whether or not this strategy will have prolonged effect cannot yet be ascertained. However, as noted in other publications, the movement has many of the earmarks of other short-lived management fads which have characterized education in the past (Garms, Guthrie, and Pierce, 1978).

A second strategy for linking schools more tightly to their constituents is to employ market-oriented techniques. A few experiments in the late 1960s and early 1970s were sponsored by the Office of Economic Opportunity (OEO). These took the form of profit incentives for higher productivity. Also, a voucher project was undertaken in one California school district. The effectiveness of these strategies is debatable, but among educators the political reception for the market inducements was undeniably low (Cohen and Farrar, 1977). Recent Congressional interest in tuition tax credit plans has again energized voucher advocates. One political effort is attempting to provide Californians with a serious voucher proposal at the 1980 November election.

The free market strategy retains an additional spark of life in present efforts to utilize consumers' views regarding schools. Under labels such as "user evaluation" and "parent evaluation," a modest number of school districts continue in an effort to solicit citizen feedback. The accuracy of client perception regarding school performance, the uses to which consumer information is put, and the effect of school size upon such endeavors is unknown.

Yet, a third strategy for rebuilding the link between schools and their publics is the injection of a larger measure of political representation into school decision-making. This has come about because of the efforts of both federal and state governments. For example, in the early 1970s, Congress enacted provisions within the ESEA calling for parent advisory councils at both the district and school level. These were intended to provide better advice to professional educators regarding the nature of compensatory programs. Subsequently, a number of states have adopted similar programs. The amount of decisionmaking authority vested in such bodies varies and the mechanisms for citizen groups to enforce their views are

seldom powerful. For example, taxing authority and personnel decisions have nowhere been allocated to these new citizen bodies.

The advent of this political strategy may have expanded the number of citizens participating in school-related decisions (Jennings, 1968). The effect of these new bodies is not well known. Who serves on them? Who selects them? What advice do they give? How much are they listened to? How much do their members actually participate? These are the questions which need to be explored in order to know better whether or not these devices have altered the course of school politics or compensated for the changes in organizational scale to which we have been referring.

A School Scale Research Agenda

The school consolidation trend has slackened in this decade, but it has not stopped. Moreover, enrollment decline is likely to refuel the efforts of those who contend larger schools are more efficient or in some other way better (Abramowitz and Rosenfeld, 1978). Rather than continuing to permit school and school district size to be a function of inappropriate considerations, it would appear useful to understand better the various effects of organizational scale. It is not possible to describe every research endeavor in this regard which is worthy of pursuit, but in the pages which follow an attempt is made to illustrate the range of such studies. Moreover, the last section of this paper contains an overarching strategy to encompass this agenda.

This paper has suggested three categories in which school scale might have significant effects. The first of these was economic efficiency. In referring earlier to scale economy studies, this paper emphasized the piecemeal and inconclusive nature of this line of research. It clearly justifies greater effort in the future. How much money, if any, can be saved through future school closings and district consolidations? Whatever this amount, it will need to be weighed against the findings regarding the effects of school and district size upon other dimensions such as student outcomes and public participation. Research of this nature should assess the relative utility of schools within schools, so-called mini-schools. If it is the case that small schools are "better," it would be useful to know if it is the actual scale of the physical plant and the size of its population or, in contrast, the size of the organization in which the pupils and staff participate (Kimberley, 1976; Child, 1973). If it is the latter, it might be possible to salvage big schools by dividing them into mini-schools (Larson, 1949).

While acknowledging that added research is needed both on scale economies and upon student performance, we leave the detailed design of this part of the agenda to those more intimately concerned with matters of economics and school sociology. Similarly, the effect of organizational size upon parent participation in their own child's schooling and the consequences of organizational scale for racial and social integration of schools are topics which fall more readily to sociologists and social psychologists.

In attempting better to understand the linkage between organizational scale and political participation over school issues, it appears that categories of future research such as the following would be useful:

1. *Historical Studies.*

What was the nature of political participation both at the school and district level, prior to the consolidation movement gaining momentum? Particularly interesting would be a comparison of public participation before the widespread promulgation of the apolitical ideology of schooling. Differential studies of participation rates by various social strata and geographic regions prior to consolidation should also be undertaken.

2. *Cross-Sectional Analyses.*

Research on contemporary patterns of public participation in school governance could take into account present differences between large and small districts and schools. Measures could be taken on dimensions such as voter turnout, votes for non-incumbents, number and length of school board meetings, characteristics of school decision makers, interest group activity, degree of partisanship, and linkage of the school political system to the wider political sphere would prove profitable in understanding the effects of size. Here again, disaggregation of participation patterns by economic, demographic, and geographic factors should take place.

3. *Specific Analyses.*

An effort should be made to understand better the effect of newly-created participatory mechanisms such as parent advisory councils. Who serves and with what effect is the basic question. Within that framework are a myriad possible inquiries such as those listed in the two categories above.

A complete understanding of the effects of scale upon schooling cannot be secured efficiently by conventional, highly compartmentalized, discipline-dominated studies. As this paper has emphasized, questions of scale relate to a variety of outcomes, instructional, political, and economic. It will be necessary to mount a well-integrated research effort in order to achieve a useful explanation.

A multi-disciplinary approach might well concentrate upon the development of school size data bases from which policy analysts and social scientists of many stripes could draw. By contracting for studies utilizing a common format and data, greater cooperation and social science integration might be induced. Such research to be effective, will necessitate both cross-sectional and longitudinal analyses.

Toward what practical end should studies of school size be directed? Beyond the goal of explanation, what should be done with the information studies accumulated? One appropriate audience for such research results is comprised of state and local school officials. By drawing upon this information, they could make better informed judgments when constructing policies affecting school and school district size. Indeed, a product from school scale studies should be a publication for local decision makers, both professional educators and lay people, which enables them to understand the tradeoffs involved when deciding upon larger or smaller schools and school districts.

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Chapter 6

Demography and Changing Enrollments

Harriet Fishlow

Introduction

Over the next fifteen years American schools will experience a series of fluctuations in their enrollments because of demographic changes. Even as high school enrollments were slowly reaching their (probable) twentieth century peak in the mid-1970s, the overall enrollment began to decline. In the 1980s elementary enrollments are expected to increase, while secondary school enrollments continue to decline. These changes in enrollments are the result of past changes in the birth rate—the baby boom and bust—and expected future changes. Demographers expect a rise in births shortly as the aging "baby boom" babies begin to settle down and have children of their own. This will affect elementary school enrollments in the 1980s.

This chapter outlines the certain and probable scope of enrollment fluctuations at the national level and suggests some general actions school systems might take to accommodate themselves to these changes. Principal suggestions are to increase flexibility both in physical plant and staffing and to protect the interests of those likely to be affected by change. Planning for flexibility ten to fifteen years from now is perhaps not too difficult; preparing for more immediate changes, mostly declines, requires greater effort. The alternative almost certainly involves considerable strife and controversy, as school systems are forced to close schools and lay off teachers. Careful preparation may not avoid all strife and controversy, but it should ameliorate the situation. Lack of preparation for decline will also surely contribute to a great waste of resources, particularly human resources.

Enrollment fluctuations of course will vary among school districts depending on past and current migration patterns (and, to a lesser extent, on local differences in fertility and school attendance). For example, secondary school enrollments are expected to decline nationally about 25 percent between 1976 and 1990. However, areas now experiencing in-migration may well show an increase, while other places have an even more precipitous decline.

Both long-range plans and more immediate preparations depend on reasonably accurate estimations of probable enrollment changes. Although the national trends in enrollment are fairly well known, it is the local districts which will have to deal with the changes. The development of state and local capabilities in enrollment estimations and forecasting is, therefore, strongly recommended.

Demographic Background of Recent Trends

Virtually everyone is now aware that school enrollments are, or shortly will be, declining from record levels. Enrollments in the elementary schools (K-8) peaked in 1969, high school enrollments (9-12) are expected to peak this year in 1977, and enrollments at the post-secondary level will probably reach their highest level within five years and then decline.[1] The reason for the enrollment increases and subsequently observed or shortly expected declines is also well known, at least at the K-12 level.

America experienced a record high level of births from 1946 to 1964. In each of those years, births exceeded any year previous to 1946, usually by at least 25 percent; from 1954 to 1964, the number of births exceeded 4 million each year. The maximum number born was, however, recorded in 1957. The decline, barely perceptible at first, began to accelerate in the early 1960s. The last year of 4+ million births was 1964. There was a sharp 7 percent drop in 1967 and the decline in numbers born continued (with the exception of a small upturn in 1969 and 1970) to 1973. Since then, the level has held fairly steady at just under 3.2 million a year (see table 1 and figure 1). This represents a decline of over 20 percent in just one decade (1964 to 1974).

Enrollment Prospects Based on Births Which Have Already Occurred

Enrollments for the nation as a whole, at least at the K-12 level, can be predicted with some degree of certainty into the 1980s and beyond, depending on the grade level, because most of the births that form the basis for these enrollments have already taken place.

TABLE 1

ENROLLMENT IN GRADES K-8 AND 9-12, FALL 1964 TO 1984
(in 000s)

Year	K-8	9-12	K-12
1964	35,025	12,691	47,716
1965	35,463	13,010	48,473
1966	35,945	13,294	49,239
1967	36,241	13,650	49,891
1968	36,626	14,118	50,744
1969	36,797*	14,322	51,119
1970	36,677	14,632	51,309*
1971	36,165	15,116	51,281
1972	35,531	15,113	50,644
1973	34,953	15,277	50,229
1974	34,419	15,337	49,756
	Projected		
1975	33,800	15,500	49,300
1976	33,300	15,600*	48,900
1977	32,600	15,500	48,100
1978	31,800	15,400	47,200
1979	31,100	15,100	46,200
1980	30,900	14,600	45,500
1981	30,800	14,100	44,900
1982	30,900	13,600	44,500
1983	31,200	13,300	44,500
1984	31,500	13,300	44,800

*Peak year.

Source: HEW, *Projections of Education Statistics to 1984/85*. p.18. Numbers include public and nonpublic schools.

Enrollments in elementary schools. Table 1 displays the enrollment projections from the HEW publication *Projections of Education Statistics to 1984/85* published in 1976. These are displayed graphically in figure 1. Enrollments in the elementary grades (K-8) are projected to decline 16 percent between the peak year of 1969 and 1981 (from 36,797 to 30,800). A slow upturn is shown after 1981. However, careful analysis of the figures used in the projection show it to be based on an assumed upturn of about 2½ to 3 percent in births in 1975.[2] In fact, the numbers of births declined slightly in 1975 (there were an estimated 3,149,000 births in 1975 compared to 3,166,000 in 1974), and the decline continued through the first eight months of 1976. Enrollments at the K-8 level should, therefore, continue to decline to 1983, if not beyond.

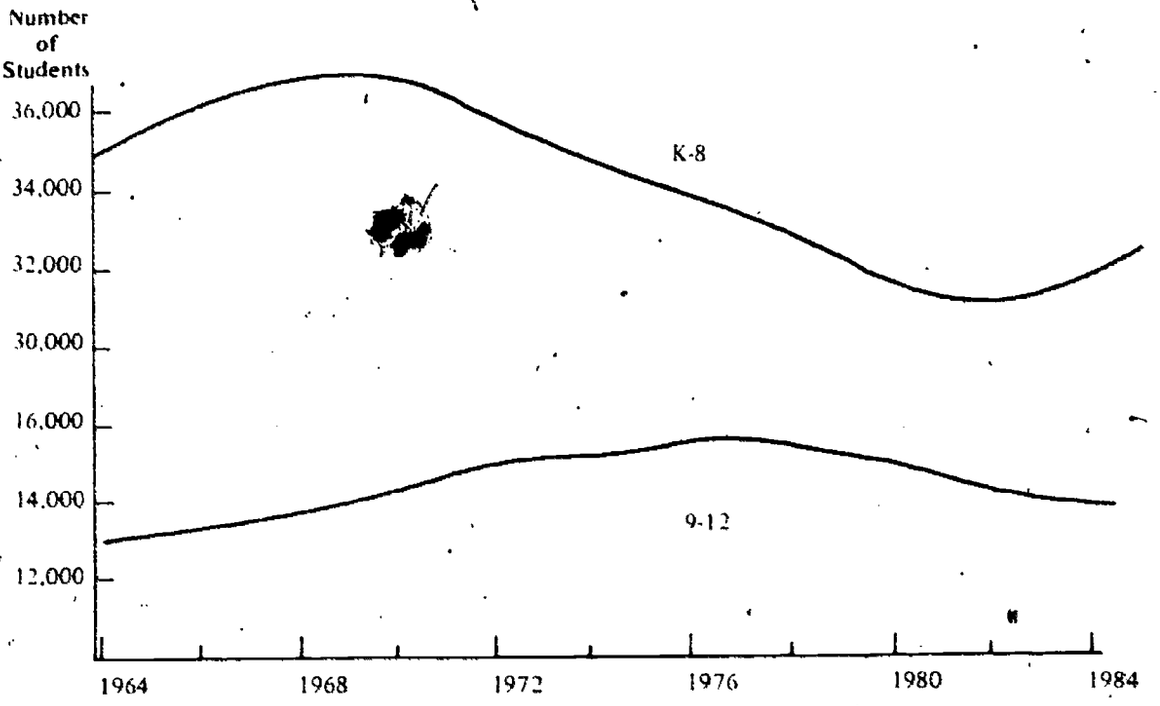
Enrollments in secondary schools. As can be seen in table 1 and figure 1, secondary school enrollments are projected to decline steadily from their projected high of about 15,600,000 in 1976. The projection is taken to 1984, when

enrollments drop to about 13,300,000, a 15 percent decline in the e[illegible]ars from 1976 to 1984.[3] (The average rate of decline betweeen the two years is 2 percent per year.)

Since births have continued to decline to the present, secondary school enrollments cannot be expected to significantly increase before the early 1990s. All things being equal, secondary school enrollments should reach a low point of about 12,000,000 in 1989, when the students in grades 9-12 will come from the small birth cohorts of 1972 and 1975. These birth cohorts were 25 percent smaller than those of 1959 to 1962, which form the basis of current secondary

FIGURE 1

ENROLLMENT IN GRADES K-8 AND 9-12, 1964-84, ESTIMATED AND PROJECTED BY HEW



school enrollments. (Enrollments in 1990 could be even lower since there were fewer children born in 1976 than in 1972.)

To predict elementary school enrollments on the basis of numbers of births a certain number of years earlier is fairly reasonable, at least at the national level, since virtually all children attend the elementary grades and automatic promotion based on age has been customary for two decades. Secondary school enrollments, however, include a discretionary factor which complicates projections. There is some evidence that the continuation rate has declined in recent years in some places.[4] In New York City, a rising proportion of drop-outs is attributed to a changing ethnic mix in the city, since blacks and Puerto Ricans are said to have a much higher drop-out rate than whites, and these groups have increased proportionately to whites in recent years. To the extent that this differential is true nationally, and to the extent that these differential rates persist over the next fifteen years or so, continuation rates would tend to fall because non-white youth will comprise an increasingly larger percentage of persons 14-17 years of age.

National figures, however, do not show any downward trend in high school continuation rates, so the problem may be one for local planners only.[5]

Enrollment Prospect Based on Fertility Projections

Enrollment forecasts beyond the early 1980s become increasingly dependent on fertility projections. The entering kindergarten class of 1982 will not be born until 1977; the same birth cohort will enter ninth grade in 1991 and colleges and universities in 1995. Thus, discussion of enrollments for those levels of education much beyond those years depends on what assumptions are made about future fertility behavior.

Projected Fertility in the U.S., 1975-2000

The Bureau of the Census issued its latest detailed population projections in 1975.[6] To provide a range, they made three separate projections based on three different assumptions about completed family size. (Mortality and migration assumptions were the same for all three.) Series I is based on the assumption that the completed families of young women now of reproductive age and those who will bear children in the future will average 2.7 children; Series II is based on an eventual completed cohort fertility of 2.1; and Series III on one of 1.7. Series II is generally considered the most reasonable, partly because it conforms most closely with recent surveys of birth expectations.[7] Two additional projections were made using an eventual completed family size of 2.1, but introducing certain variations in the assumptions underlying the projections. These illustrate the result of certain trends which, although considered possible by demographers, are felt by the Census Bureau to be somewhat less likely than those assumed to underlie Series II.

Series II L is based on a later timing of births than Series II. It assumes a sharper decline in teen-age births between 1974 and 2000 than does Series II, and a substantial increase in births to women in their thirties. This would move the mean age of childbearing from about 26 to 28. The actual age-specific fertility for 1974 and the two projected sets for 2000 are shown in table 2.

The pattern of age-specific fertility in Series II is similar to that of many moderate-to-low fertility European countries, including England, France, and

TABLE 2

CHILDREN PER 1000 WOMEN, BY AGE

	10-14	15-19	20-24	25-29	30-34	35-39	40-44	45-49
1974	1.2	59.9	116.1	106.8	52.7	21.4	5.3	0.4
2000 (II)	0.3	46.4	144.2	143.6	62.2	19.6	4.7	0.3
2000 (IIL)	0.2	24.6	99.8	149.9	102.5	31.8	4.9	0.3

Belgium. These countries have historically had lower teen-age fertility than the United States and somewhat later marriages. The pattern in Series II L is closer to that of Spain, Italy, and Ireland, countries which have maintained moderate levels of fertility during much of the twentieth century through late (and non) marriage rather than through the extensive use of contraception. While it is certainly possible that the United States would move to that pattern, the Series II pattern seems somewhat more probable given our past history and heterogeneous population. The age pattern of the decline in American fertility between 1960 and 1974 lends some support to the Bureau's preference for Series II, as table 3 indicates.

Teen-age fertility has been the slowest to decline, fertility of women over thirty-five the fastest (although the fact that women over thirty-five in 1974 were the mothers of the baby boom probably has something to do with the latter.) Series II L is interesting, however, because it demonstrates the effect on *annual* fertility of a delay or stretching-out of cohort fertility. *Annual* fertility is lower under Series II L than under Series II because each cohort's contribution to the total of any one year is less.[9] Since such a pattern is a possibility, its implications are worth considering.[10]

TABLE 3

PERCENT CHANGE IN AGE-SPECIFIC FERTILITY RATES, UNITED STATES 1960-1975[8]

Under 15	+62
15-19	-37
20-24	-56
25-29	-44
30-34	-53
35-39	-65
40-44	-70
45-49	-67
Total	-51

Series II R, while based on an eventual cohort fertility of 2.1, does not assume this rate will be achieved by persons born in the twentieth century since it assumes that only cohorts of 3.8 million will have replacement level fertility. Cohorts of 4.2 million, like those of the later baby boom years, are expected to average 1.9 children, while the smaller cohorts of the early 1970s will average 2.2 to 2.4. Successive cohorts will tend to over- and under-shoot 3.8 million and thus have lower or higher completed fertility. The projections oscillate past the end point year, 2050.

As shown in both table 4 and figure 2, the number of annual births, determined by the projected age-specific birth rates considered plausible for each hypothetical completed family size and by the projected female population in the childbearing ages, is projected to increase in the 1980s in all five projections.[11]

TABLE 4

ESTIMATES AND PROJECTIONS OF THE AVERAGE ANNUAL NUMBER OF BIRTH: SELECTED YEARS, 1940 TO 2000

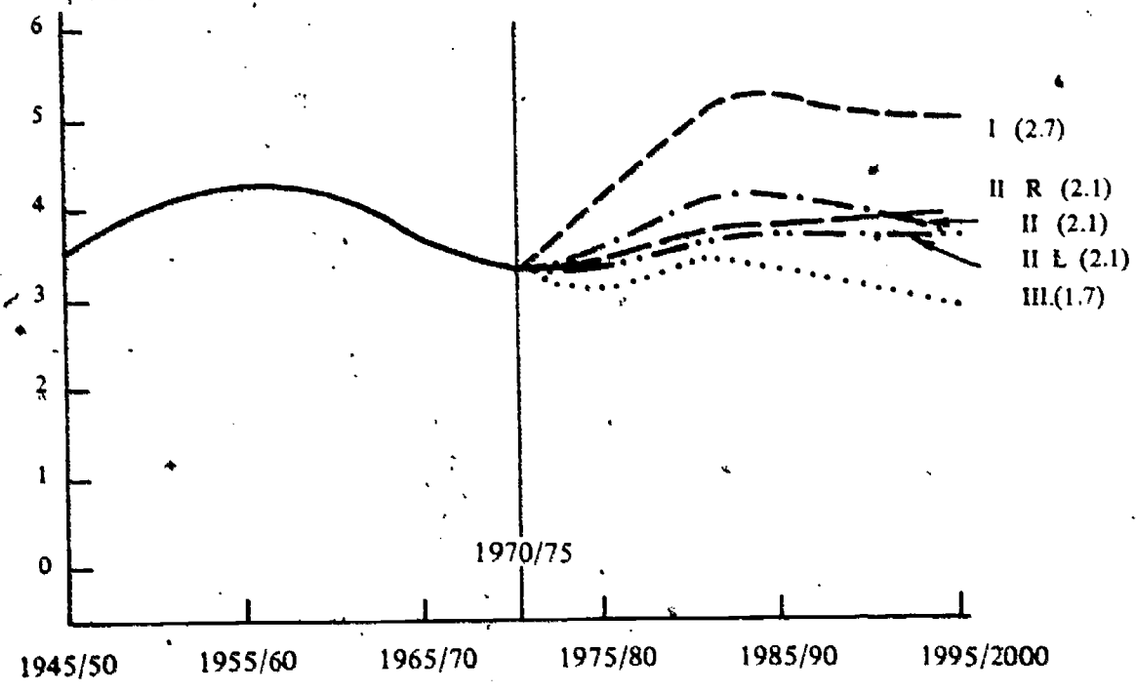
(in 000s)

Years (July 1/ June 30)	Series I (2.7)	Series II (2.1)	Series IIL (2.1)	Series IIR (2.1)	Series III (1.7)
Estimates					
1940-45		2,903			
1945-50		3,555			
1950-55		3,949			
1955-60		4,274			
1960-65		4,171			
1965-70		3,613			
1970-71		3,709			
1971-72		3,408			
1972-73		3,191			
1973-74		3,112			
Projections					
1974-75[1]		3,187			
1975-76[1]		3,126			
1976-77[1]		3,265			
1977-78[1]		3,298			
1974-75[1]	(3,372)	(3,178)	(3,172)	(3,163)	(3,049)
1975-76[1]	(3,679)	(3,285)	(3,240)	(3,234)	(2,946)
1976-77	3,932	3,425	3,310	3,333	2,958
1977-78	4,156	3,575	3,377	3,435	3,092
1978-79	4,356	3,720	3,440	3,528	3,223
1979-80	4,539	3,865	3,498	3,611	3,323
1980-85	4,958	4,088	3,625	3,781	3,416
1985-90	5,243	4,146	3,717	3,875	3,376
1990-95	5,093	3,949	3,719	3,906	3,173
1995-2000	5,076	3,783	3,778	3,951	2,944

[1]Estimate of 3,187 for 1974-75 and 3,126 for 1975-76 is from National Center for Health Statistics, *Monthly Vital Statistics Report* 25, 6 (August 27, 1976): 1. These estimates became available after the projections were made, not present in figure 1.

FIGURE 2

BIRTHS BY PROJECTED LEVEL OF COHORT FERTILITY (5-YEAR AVERAGES)



Fertility projections under the assumptions of Series I (2.7 children). Were average completed family size to equal 2.7 children, the increase in births would continue until the late 1980s and remain high for the rest of the century. The peak number of births in the baby boom years—4.3 million in 1957—would be surpassed within two years (by 1978-79), and numbers of births would increase another 20 percent in the 1980s. Numbers of births would equal or exceed 5 million for twenty years (1980-2000).

Fertility projections under the assumptions of Series III (1.7 children). Under the Series III average of 1.7 children, annual births would drop slightly below 3 million and then increase to 3.4 million in the mid-1980s before beginning a long-term decline that reflects the sub-replacement level fertility assumption. (Actual declines in total population would not occur until after 2020.)

Fertility projections under the assumptions of Series II (2.1 children). The average completed family size which is considered most probable by Census Bureau demographers is the replacement level 2.1. The expected annual births for this size family would increase steadily from the 3.1 million recorded in 1974 to over 4 million in the 1980s (a rise of 30 percent) and then would drop slightly below 4 million in the 1990s. In terms of *numbers* of births, the 1980s and early 1990s would be close to the baby boom years of the 1950s and early 1960s.

Fertility projections under the assumptions of Series II L (2.1 children, late childbearing). If the mean age of childbearing moved from the mid- to the late twenties, births would, under the cohort fertility assumptions of Series II, rise to about 3.6 million in the early eighties and maintain a level of about 3.6 to 3.7 million to the end of the twentieth century.

Fertility projections under the assumptions of Series II R (cohort fertility connected to size of cohort; eventual achievement of 2.1). Under the assumptions of Series II R numbers of births would rise steadily throughout the rest of this century and beyond, reaching a peak of about 4.1 million in 2007.

The first year of projection was 1975/76, for which a preliminary estimate of number of births is now available.[12] At 3,126,000, it falls between Series II (2.1) and Series III (1.7).

Implications for Elementary School Enrollments

As indicated in table 5, the population aged 5-13, which forms the basis for elementary school enrollments, will decline about 10 percent between 1975 and 1980 under all the projected Series, including Series II and II L. The decline to 1980 is certain since nearly all the children who would be 5-13 in 1980 had been born when the projection was made (mid-1975).[13] In 1980, that age group will be just 83 percent of what it was in the peak year of 1970, or approximately what it was in 1957.

Little change in the number of children 5-13 is projected between 1980 and 1985. Under the straight Series II projection, the number of elementary school children will creep up less than ½ percent between 1980 and 1985; if couples delay childbearing as projected in Series II L, it could decline by about 3 percent. The following ten years to 1995 will see growth, however. In the five years between 1985 and 1990, the number of children of elementary school age will increase 12 percent, if the most likely age pattern of childbearing is followed.[14] There will be another 6 percent increase between 1990 and 1995. In 1995, there will be about as many children of elementary school age as there was in the peak year, 1970. Thereafter, the size of this age group will decline again for fifteen years. The decline will continue to 2010, when the projection shows a total of 34,335,000 children age 5-13 (about the level of the early 1960s).

In summary, the decrease in elementary school enrollments which has occurred since 1970 will, under Series II fertility assumptions, continue into the early 1980s. There will be no substantial turn around until after 1985. However, growth after that will be fairly rapid, since children of elementary school age will increase 12 percent in the next five years and 20 percent in the ten years between

TABLE 5

ESTIMATES AND PROJECTIONS OF THE POPULATION BY SELECTED AGES: 1950-2000

(in 000s)

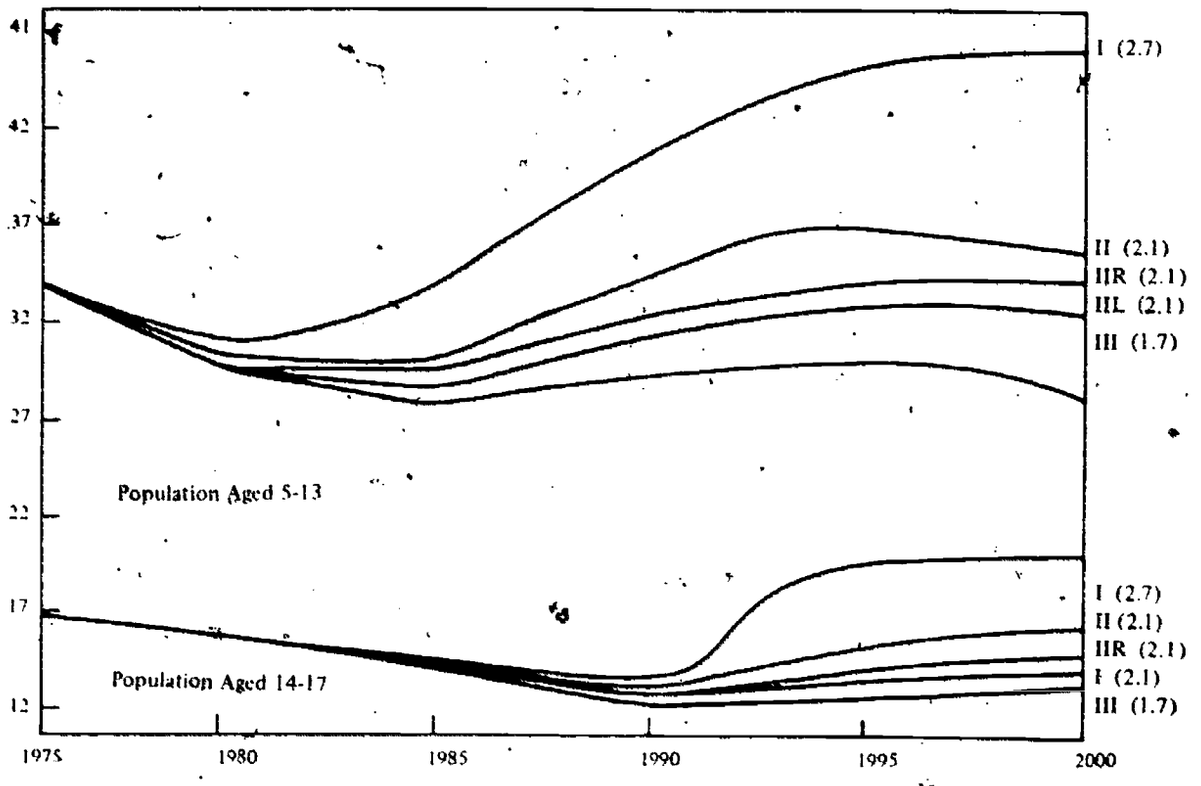
Ages 5-13

Years (July 1/ June 30)	Series I (2.7)	Series II (2.1)	Series IIL (2.1)	Series IIR (2.1)	Series III (1.7)
1950		22,423			
1955		27,925			
1960		32,965			
1965		35,754			
1970		36,636			
1975		33,441			
1980	30,441	30,246	30,235	30,228	30,112
1985	33,330	30,380	29,378	29,645	27,954
1990	41,282	34,643	31,434	32,379	29,383
1995	45,725	36,799	32,868	34,179	30,320
2000	45,923	35,963	33,213	34,758	29,119

Ages 14-17

1950		8,444			
1955		9,247			
1960		11,219			
1965		14,153			
1970		15,910			
1975		16,923			
1980		15,753			
1985		14,388			
1990	13,538	12,941	12,879	12,867	12,463
1995	17,912	15,317	14,055	14,439	13,210
2000	20,575	16,752	14,873	15,432	13,915

FIGURE 3
POPULATION PROJECTION BY ASSUMED LEVEL OF FERTILITY



1985 and 1995. The fifteen years after 1995 will again see a decline, but only between 6 and 7 percent.

It is the difficult task of educators to prepare not only for the substantial and inevitable decline of the next few years, but also to plan for a reasonably certain upturn thereafter. *There are very likely to be as many children in elementary school in 1995 as there were in the peak year of 1970, before the current decline.* Fortunately, there will be some advance notice of the upturn, as there was of the downturn, given the lag between birth and entrance into school. If births start to increase rapidly toward the end of the 1970s, as they will if Series II fertility is a reasonable choice, then educators should begin to inform the public of the need for increasing resources, preparing teachers, etc.

Implications for Enrollment in Secondary Schools (9-12)

Table 5 also presents estimates and projections of the population aged 14-17, which is the basis for secondary school enrollments. The steady decline of nearly 25 percent from the current high of close to 17,000,000 to under 13,000,000 in 1990 is certain since most persons who will be that age in 1990 are already born. Growth after 1990 will be rapid, however. Between 1990 and 1995, numbers in that age group will increase nearly 19 percent under the Series II assumptions. Even under the less likely pattern of later childbearing in Series IIL,

those five years would see a 9 percent increase in young persons of secondary school age. Series II R fertility would cause a 12 percent increase.

Another 10 percent increase between 1995 and 2000, projected by Series II, would bring that age group nearly back to its peak 1975 level. All in all, if Series II is reasonably correct, and enrollment rates remain equal to current ones, we can expect a 30 percent increase in secondary school enrollments in the decade of the 1990s. Should childbearing be delayed (Series II L), the increase would be a more modest, but still substantial, 16 percent (20 percent under the pattern of II R).

The upturn in secondary school enrollments will be even sharper than that in elementary schools, just as the downturn will be, because of the smaller age span involved. However, the advance warning will be greater, too.

Probable Level of Future Fertility

The decline in fertility in the last decade has been attributed to a variety of causes. The various factors which have caused people, especially younger people in the prime reproductive ages, to lower their fertility can be conveniently resolved into two categories. These are: (1) the recent restriction in the opportunities for *economic advance* which may be bearing most heavily on younger persons; and (2) the extension to women of the long-time American ideal of *individualism*, and, possibly, the intensification of this ideal among men. (By individualism, I mean the beliefs that adults should be independent "captains of their fate" and that they have the right to pursue self-fulfillment through a wide range of personal choices.)

One manifestation of the growth in individualism is an increased questioning (if not outright rejection) of the social norms about the desirability of marriage and childbearing, especially as an exclusive life goal for women. This questioning appears to have intensified the dampening effect on fertility that economic adversity normally has in modern, industrial societies. People who are not sure what they want in life and who are also uncertain about their economic future are usually not inclined to found families. Even those who decide they do want children would be likely to have fewer than in more affluent times.

If trying to choose a likely level of future fertility, therefore, it is necessary to consider whether and to what extent heightened individualism and a difficult economic situation will continue into the future. Will the questioning attitude about the desirability of domestic commitments be transmitted to those who will be twenty to twenty-nine in the mid-1980s, or is it a current fad? Will such an attitude become entrenched among all social groups or will it be confined to a few? Will the state of the economy improve sufficiently to restore a sense of expansiveness and confidence among younger people? (The uncertainties in predicting even the ultimate level of fertility, not to mention the yearly fluctuations demonstrated by the above variables, show how fortunate educators are that they have several years to plan after the actual number of births become known.)

The Economy

Predictions about the future of the economy are no more noteworthy for their accuracy than are predictions of future fertility behavior. A couple of observations are possible, however. Current economic problems apparently stem, at least in part, from the sudden ending of an era of cheap energy, which in effect has reduced real national income. How disastrous the rising cost of fuel turns out to be depends in great measure on our political skill, both foreign and domestic. Nevertheless, it seems safe to say that the situation does not augur well for economic expansion and the growth of real income, at least for the next few years. I do not want to imply that growth in real income will cease, but that it may be slower and more difficult to achieve than in the past. New discoveries and technologies may change conditions, but probably not within the next decade.

Another factor arguing against a rapid return of economic expansion and the associated attitudes among younger persons is the relative size of the cohort to which they belong. Persons born from 1946 to 1964, the "baby boom" cohorts, have formed a bulge in the age structure, which first influenced the sale of baby goods and suburban houses, then put a strain on the capacities of the school systems, and is now pressing on the ability of the economy to provide entry level jobs. Youth unemployment has risen steadily since the late 1960's.[15]

The possibility that persons belonging to the large cohorts of the baby boom will have relatively fewer opportunities and relatively more difficulties in advancement than smaller preceding cohorts, and consequently, lower fertility, has been suggested by several researchers.[16] The effect of relatively stiff competition for jobs and subsequent promotions can only be exacerbated by the high proportion of young people with college degrees, because the number of jobs which require a college education do not appear to be growing as fast as the number of degree-holders.[17] To be thwarted in attaining expected positions is not conducive to settling down and making long-range commitments, even if one attains an ample income.

If, in fact, the economic situation does not improve greatly in the next decade or decade and a half, and if the relatively larger size of the baby boom cohorts causes their individual economic opportunities to be less than those enjoyed by their parents in the 1950s and early 1960s, then it seems probable that fertility will remain at low-to-moderate levels through much of the 1980s.[18]

By the latter 1980s, the smaller cohorts born after 1965 will begin to reach job entry and family building age. If their relatively small numbers cause an expansive outlook for them, they may opt for greater domestic commitments and fertility would rise. If the general economic situation improves at the same time, earlier cohorts might revise their desired family sizes upward, causing fertility to rise very sharply indeed.

Interest in the possible influence of the size of a cohort on its fertility (because of relative economic opportunities) has been sufficient among economists and demographers to cause the Census Bureau *to project fertility on that assumption.* The projection, called Series II R, was, however, keyed to eventual replacement level fertility; i.e., an average of 2.1 children per woman at steady state. This

avoids the theoretical difficulties associated with projecting infinite growth or decline, but constrains the projected fertility for most cohorts below what has been the experience in the recent past. For the purposes of the projection, it was assumed that a cohort size of 3.8 million would have replacement level fertility. Cohorts of 4.2 million, like those of the late 1950s and early 1960s, would average 1.9 children while the smaller cohorts of the early 1970s would average 2.2 to 2.4 children.

Technical problems of projections aside, I believe the acceptance of 2.1 children per woman as the most reasonable by the Census Bureau rests more with their assessment of the significance of social trends categorized as "individualism" than with economic factors.

Individualism

The assertion that many of the trends associated with and presumed to underlie the recent decline in fertility are manifestations of an expanding individualism is not easy to prove. It is certainly not amenable to the quantitative proof which demographers prefer. At least two of these trends—the increased participation by women in the labor force, and increased proportions of single women under the age of twenty-five—could well have other causes, such as economic need or uncertainty. Yet, inspecting the range of social variables that have changed so conspicuously during the period of declining fertility—the labor force participation of women, including married women with children, the entry of women into careers that require serious, full-time commitments, the divorce rate, the proportion of women under twenty-five who are single, the use of more effective contraceptives, the acceptance of abortion—does suggest that many people, and especially many young women, have decided to pursue interests other than, or in addition to, marriage and childbearing. Furthermore, this decision clearly arises from an increasing recognition of the right of women to make choices based on individual goals.

In the last decade or so, the right of women to choose careers other than marriage and motherhood and to have individual interests in addition to their family roles has been publicly legitimized. Indeed, in some social groups, the cultivation of interests outside the home has come to be regarded not only as legitimate, but as desirable. Childrearing is competitive with non-familial interests, especially for women, on whom the burden of children has traditionally fallen. Fertility will tend to decline as non-familial interests assume greater salience. Few people wish to deny totally themselves the satisfaction of family life, so those couples where both partners wish to pursue non-familial or individual interests will tend to have the minimum number of children customarily thought acceptable—two.

A society in which *all* women are highly motivated to limit themselves to two children will fall short of replacement because not everyone finds him or herself in a position to have children, nor is everyone capable of having two children. However, some couples will probably want more than two, so it can be reasonably

assumed that most cohorts will approach replacement level fertility. Current cohorts of young women show signs of adopting this pattern. Recent survey data collected by the Bureau of the Census about the birth expectations of young wives eighteen to twenty-four show that 58 percent expected just two children, and nearly 75 percent expected two or fewer.[19] If these young wives do achieve their expectations (and other fertility surveys have shown a fairly close connection between the number of children women say they want and how many they eventually have[20]), they will experience 2.17 births per woman, on average. Since women who are married at eighteen to twenty-four tend to have more children than the cohort average, this works out, after the appropriate adjustment, to just about replacement fertility.

At present, it appears that the increasing acceptance of the rights of women to give priority to individual, non-familial interests, combined with a lessening of economic opportunity, will lead to replacement level fertility—the Census Bureau's Series II, or some variant thereof for the *current* cohort of young women.

Can we expect women to *continue* to develop their individual interests at the expense of their family roles, or is this a temporary fad, encouraged by economic adversity? After all, the previous low in completed fertility, 2.18, or just above replacement; was experienced by women who were of peak childbearing age during the Great Depression (the cohort born from 1906 to 1910). Completed fertility of younger women through the birth cohort of 1941 to 1945 is or will be in general much higher.[21]

Will good times in the future cause later cohorts to return to three- and four-child families and the traditional male/female roles that implies? I do not think so. On the contrary, I believe we can expect a diminishing proportion of women to devote themselves solely to the roles of wife and mother, whatever the economic situation. In my opinion, this development is a natural one for American society which has always placed a high value on independence and individual freedom. While the principal adherents to these values were assumed to be men, independence has generally been admired in women as well, except in their relations with their immediate families, especially their husbands. The dependent status of wives has been the only adult dependency that American society has validated. However, women have not necessarily been raised to be dependent, or more accurately, feminine upbringing for dependency and its inevitable inferiority of status vis-a-vis the husband has not been thoroughgoing. Boys and girls have been accorded a *relatively* high degree of equality for a long time in American society, particularly in basic education. Access to secondary schooling has been fairly equal for the sexes for decades, and education has usually been co-educational. More recently, at least since the fifties, middle-class families have tended to send both sons and daughters to college, though males were, on average, favored.

Adulthood, however, marked an abrupt change. Men went on to careers and jobs as independent persons, but women were expected to accommodate to, and indeed to strive for, a socially and economically dependent status at variance with their previous training. The consequent inherent strain in the role expectations of American women was specifically noted as far back as 1942 by Talcott Parsons.[22]

Stressful situations are usually accepted only if there are no reasonable alternatives, actual or perceived. The recognition of women's right to choose individual goals and the consequent pressure to expand opportunities for women has provided alternatives, especially for younger women. The possibility of such alternatives seems to have actually increased the strain of the traditional role for many women.

It is not suggested that all women would choose full-time, lifelong careers instead of motherhood. Such an idea is simply the obverse of the old coin of socially, rather than individually, determined lives for women. Rather, the majority of women will likely try to combine a life commitment to work outside the home with childrearing. However, they manage—through interrupted careers, part-time work, childcare, equitable sharing by husband and wife—it is not conducive to large families. I expect for most couples, two children would be the limit and that cohort averages will tend to approach the replacement level of about two [illegible]er women.

Post-Secondary Enrollments

A principal cause of uncertainty in post-secondary enrollment projections, unlike those for elementary and secondary schools (at least at the national level), is estimating the proportion of young people who will choose to go to college. The past, especially the recent past, has seen continual growth in both the rate of attendance and the size of the population of college-going age (usually considered to be from the ages eighteen to twenty-one or eighteen to twenty-four).[23] The result has been academic and financial planning based on the presumption of continual growth. However, a careful consideration of probable future trends, including a known decline in population, indicates that the extraordinary growth in post-secondary enrollments of the recent past will, at best, slow down substantially and may well cease. The changes began to appear in the early 1970s.

From the mid-1950s to the end of the 1960s, a period of about a dozen years, American higher education experienced unparalleled growth. Between 1960 and 1970, the total number of students in degree credit courses more than doubled, rising from 3.6 to 7.9 million.[24] Around 1970, however, the rate of enrollment increases began to slow down. Between 1970 and 1974, total degree credit enrollment increased at an average annual rate of only about 2 percent a year, compared to 8 percent yearly during the preceding decade.[25]

The Rate of Attendance

Carnegie Council studies have shown that 55 percent of the increase in the 1960s could be attributed to increasing rates of enrollment and 45 percent to increase in population in the college-going ages. The greater importance of increasing rates of attendance in the achievement of enrollment growth has been

true during most decades since 1870, although the college-age population has been increasing as well.[26] The decrease in the rate of enrollment increases after 1970 has been largely because of an actual decline in the rate of college attendance among young people eighteen to twenty-four years old.[27]

The recent decline in the rate of attendance has been attributed to a number of factors. The Carnegie Council book, *More Than Survival*, lists four as the most significant:[28] (1) the abolition of the draft; (2) sharply rising costs of attendance; (3) job market changes, especially for school teachers; and (4) liberalized rules permitting deferred admissions and leaves or "stopping out." These reasons suggest the possibility that the current decline is a temporary setback in the long-run growth of attendance. Relative costs of attendance can change as can job market prospects, and a steady stream of deferred admittees and persons returning from "stop-out" periods would eventually correct the early deficits.

Assuming that the *decline* is temporary, however, does not necessarily imply an expectation that the previous rate of increase will resume. An important segment of current opinion proposes that, even though the decline of the last few years has been caused by factors presumed temporary, an increase in the proportion attending college, especially the rapid rate of increase experienced through the 1960s, will not occur again.[29] The principal grounds for this belief include forecasts of the educational requirements of future employment, the already high proportion of persons who graduate from high school, and of high school graduates who go on for higher education, and the financial uncertainties surrounding higher education.

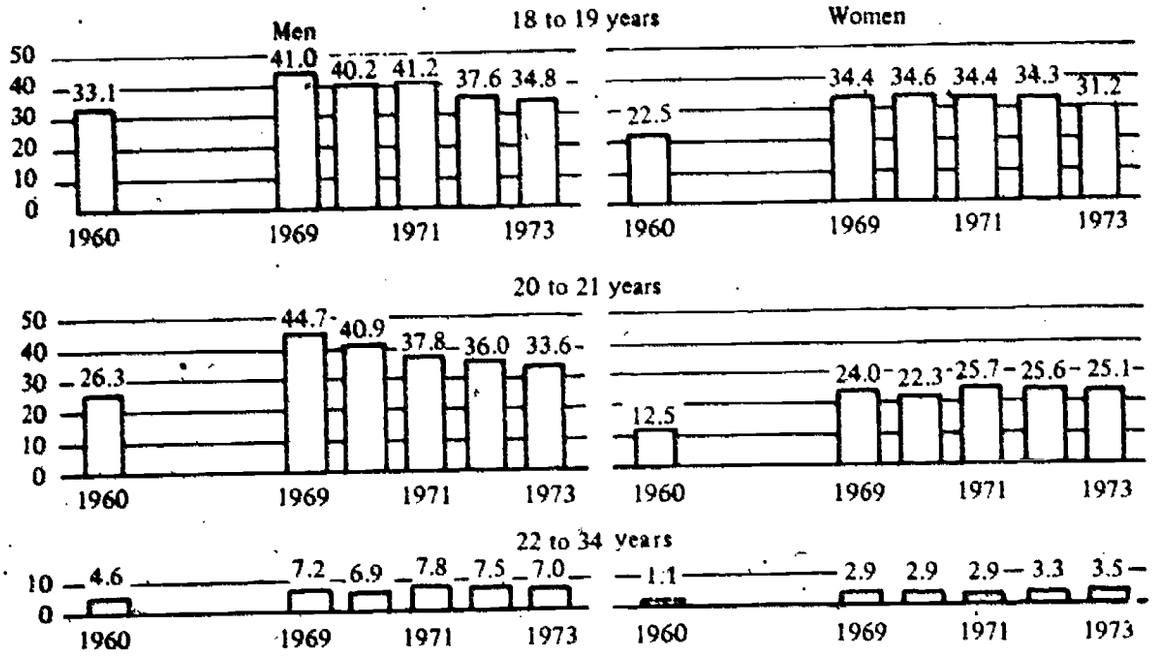
The idea that there is a fixed proportion of jobs requiring a college education and that the market is or is about to be saturated with degreeholders is challenged in the Carnegie volume, *New Students and New Places*. The authors point out that there are social and cultural, as well as economic reasons for college attendance and that employers are likely to raise their requirements in tight labor markets.[30] Nevertheless, the authors include the problem of job market saturation among the factors causing uncertain enrollment predictions.[31] It is given a less ambivalent and more central position in many other studies.

The argument that the high proportion of college-age persons currently attending renders a continuation of past increases unlikely is based on the assumption, usually implicit, that not everyone can benefit from formal education after high school and that the larger share of those who can are already attending. Whatever the merits of this viewpoint, the fact is that the *historic rate of increase* in proportion attending college has a *necessary limit*, even if all were to ultimately attend. In the 100 years between 1870 and 1970, the proportion of persons eighteen to twenty-one attending college increased from 2 to 35 percent a year, an average increase in proportion attending of about 2.9 percent a year.[32] Were that rate to continue, there would be 100 percent attendance by the year 2006, at which point further increases would obviously cease.

Any assumption of less than 100 percent ultimate attendance brings the date of cessation of increase closer. More important for short-term enrollment projections, current social and economic conditions suggest that whatever the proportion attending may ultimately be, further increases will be more difficult to achieve

FIGURE 4

ESTIMATED UNDERGRADUATE DEGREE-CREDIT ENROLLMENT AS PERCENTAGE OF POPULATION, BY AGE AND SEX, 1960 AND 1969-1973



Source: Adapted from U.S. Bureau of the Census data by the Carnegie Council.

than were past increases, and hence will come about more slowly. The *rate of increase* (if any) in the future *should therefore be less* than it has been, until it finally ceases.

The authors of *New Students and New Places* "expect the percentage of college-age population (which they define as eighteen to twenty-one years of age) actually in college . . . to level off at about 50 percent in the year 2000 . . ." Their calculation of a 50 percent attendance rate assumes that persons from the lower half of the socioeconomic scale will attend at the same rate as those from the upper half and that all states will achieve the high school graduation rate (90 percent) and the high school graduate college entrance rate (75 percent) of the current top states. The implicit assumption is that the attendance rate of the more favored sections of society has reached a maximum practical level (either because of ability or employment requirements or both) and that the only possible growth can come from those now unable to attend because of lack of opportunity. An increase to a 50 percent attendance level by 2000 from the 35 percent in 1970 would require an increase in proportion attending of about 1 percent a year, distinctly smaller than the 2.9 percent average of the previous century.

Population Growth

The increase in the proportion of persons of college age (18-21 or 18-24) who enroll in institutions of higher education has been the most important factor in the historic growth in college attendance, but increasing population in this age

group has also played a part.[33] This was especially true of the rapid growth of the 1960s, when sheer increase in numbers of eighteen to twenty-one year olds accounted for 45 percent of the increase in enrollments.[34] In fact, that decade saw the most rapid growth in the numbers of persons eighteen to twenty-one years of age since the middle of the nineteenth century—an increase of about 44 percent. While growth rates of this magnitude could conceivably recur, they will not do so in the next two decades, nor well beyond that, assuming Series II fertility. (Between 1970 and 1980, the number of persons of that age will have risen nearly 15 percent. In the decade of the 1980s, it will decline nearly 15 percent; the subsequent five years will see a further 6 percent decline.)

The rapid increase in the college-age population during the 1960s was the result of the juxtaposition of the low fertility of the depressed 1930s and the postwar "baby boom." Between 1960 and 1970, the number of persons eighteen to twenty-four, perhaps a better age span to nominate as the "college-age population," rose 53 percent. Persons eighteen to twenty-four years old in 1970 were born from 1936 to 1942, which were predominantly years of low fertility. Those eighteen to twenty-four in 1960 were born from 1946 to 1952. Many couples who had postponed childbearing in the 1930s or during the war years "made up" the deficit then. The year 1947 had the highest birth rate (26.6/1000) in more than twenty-five years, one not matched since. Numbers of births fell a little over the next five years and then began to rise again in 1951, reaching an all-time peak of 4,308,000 in 1957. However, the rate of increase after 1951 was less than that from the end of the 1930s to 1947.[35]

The estimated number of persons eighteen to twenty-four and the numbers projected under Series II fertility is (000):

Series II:	1960	16,128
	1965	20,293
	1970	24,683
	1975	27,575
	1980	29,441
	1985	27,834
	1990	25,162
	1995	23,641
	2000	26,328
	2005	29,164
	2010	29,198
	2015	27,848

Between 1980 and 1995 there will almost certainly be a 20 percent drop in this age group (the projected figure for 1995 is virtually identical under all Series because most of the persons who will be eighteen to twenty-four in 1995 have a already been born). Increase thereafter will be rapid, although that age group will not approach the 1980 level until after the turn of the century. The fifteen-year decline from 1980 to 1995 will be nearly made up in the following ten years (1995 to 2005), but increases of the magnitude experienced in the 1960s are not expected. (If Series II fertility projections are correct, the number of persons eighteen to twenty-four will increase about 23 percent in the ten years between

1995 and 2005; from 1960 to 1970 numbers of persons in that age group increased 53 percent.

Institutional Response to Enrollment Decline

The combination of a declining population in the usual college-attending ages and a slowly increasing proportion attending, at best, almost certainly portends a decline in enrollments in higher education in the 1980s. The average annual decline in the eighteen to twenty-four-year-old group will be about 1.8 percent a year in the twelve years from 1980 to 1992. The increase, if any, in the proportion attending is unlikely to offset that.

The effects of this probable decline, however, will not be felt equally by all institutions of higher education. The differential effect by type of institution is the subject of the Carnegie volume *More Than Survival.*[36] Carnegie does not predict much, if any, decline in overall enrollments because they assume a rising enrollment rate. This prediction is based on past trends for:

- Part-time students
- Non-degree credit students
- Students older than twenty-two
- Graduate and first professional degree students
- Women, blacks, and other minorities

It also assumes a return of white, male enrollment to its 1960s peak and an increase in student aid. Some of these underlying assumptions appear a little optimistic and the authors acknowledge that a decline in total enrollments could in fact occur. Their analysis of the differential effect by type of institution, however, holds for stability or decline.

The authors distinguish six categories of institutions of higher education: (1) universities; (2) comprehensive colleges and universities (the latter with limited doctoral capacity); (3) highly-selective liberal arts colleges; (4) less highly-selective liberal arts colleges; (5) public; and (6) private two-year colleges.

Enrollments were projected according to several assumptions about the future shares of total enrollments each of these categories would get. Five different models were used. The first three models involve simply projecting forward different current values (current shares would remain constant or the rates of change in shares from 1963 to 1973, or 1968 to 1973 would continue). This is essentially the method used by HEW in their *Projections of Education Statistics* series; that is, a straight-line projection based on recent rates.

The second two models are more analytic. The first assumes that changes in total enrollment shares are determined by four *external* factors—population change; the market for teachers; the interest in part-time non-degree credit study; and public policy toward the support of private institutions. Examination of these factors indicates growth for public community colleges, retention of the status quo for the highly-selective liberal arts colleges, and relative loss for all the other categories.

The fifth model assumes that future shares will depend on the ability of the institution to make selective adjustments to future change, in particular, to decreased enrollments and to financial stringency. The characteristics which are thought to determine the degree of possible institutional flexibility, with the advantageous qualities for those characteristics in parenthesis, are:

- Size (large)
- Location (urban)
- Competitive status (relatively lower tuition)
- Reputation (well-defined mission or ambience)
- Age (well-established)
- Past decisions, especially those of the 1960s (modest, planned, not over-optimistic growth)
- Graduate enrollment and specialization (less)
- Health professions (established programs)
- Financial condition (healthy)
- Management and attitudes toward reality (willingness to face facts, share information, and be accountable to those who provide support)

On the whole, the authors believe the universities and public community colleges have above-average adjustment capability, while the less highly-selective liberal arts colleges, the private community colleges, and some of the comprehensive colleges and universities (especially the private ones), have below-average ability to adjust. The highly-selective liberal arts colleges and the better-managed comprehensive colleges and universities, especially the public ones with the lower tuition, are considered average.

Synthesizing the analysis of the several models, especially the last two, to forecast enrollment shares, the authors conclude that public community colleges, universities, and highly selective liberal arts colleges will do relatively well in enrollments and institutional well-being, while the other categories will, as a rule, do less well. Among the reasons for their conclusion is the greater possibility of flexibility in the former institutions, especially the multi-campus universities, in facing declining and fluctuating enrollments.

Preparing for Enrollment Changes

It will not be sufficient to simply monitor the national birth rate to ascertain probable enrollment changes within local school districts. Growth, like decline, will occur more sharply in some areas than in others. Aside from any local differences in birth rates, there are important differences in migration, both regionally (e.g., the movement from the northeastern and north-central states to those of the southwest and west) and within regions (e.g., from central cities to suburban rings and to rural areas).

Unfortunately, projections of local populations by age are not easy to do because they depend so heavily on estimating future migration patterns. The demographic art of local migration forecasting is considerably less satisfactory than even annual fertility forecasting. In many instances, it is not even possible to

know with any exactitude what recent *past* migration patterns have been, much less to forecast them. On the whole, U.S. migration statistics are awkward to collect and to analyze because we do not keep registries of movement as we do of births and deaths. We depend on the census, including special surveys, and on retrospective reporting to understand migration patterns. Often all that can be known is the net migration; the exact numbers of immigrants and emigrants which compose that number can only be surmised.

Nevertheless, planning for future flexibility in meeting enrollment change and for handling present crises requires some idea of the likely shape of future enrollments in local districts. It is, therefore, essential that administrators have access to the best estimate of their district's *current* population by age and that they develop their capacity to estimate its likely future configurations. While many of the largest school districts have staff who project enrollments—usually by the grade progression method combined with a local birth rate-based estimate of future kindergarten entrants -few are equipped to estimate the range of probable enrollments as much as five years, to say nothing of ten to fiteen years in the future. State governments should provide such demographic projection services to local school districts, perhaps in cooperation with the Bureau of the Census, which has done some work in the field of small area census in a project called the "Dual Independent Map Encoding File" supervised by Jacob Silver. Given the difficulties in predicting small area migration patterns, such services need the input of persons familiar with local problems and prospects. As part of the service, school administrative staff could be trained to monitor the situation from year to year and make tentative corrections based on current experience.

No state government agency is providing demographic services to local school districts anywhere at present, so far as I know. The state of California's Population Research Unit is probably one of the best state demographic units. It estimates population for approximately 500 cities and counties every year, does a range of projections (using several fertility and migration assumptions) by county and age every two to three years, and assists localities other than counties to do special censuses when requested. Since 1970, it has provided technical assistance, including final tabulations for special censuses, for twenty school districts which requested special financial assistance from the State Allocation Board because of sudden population increases. The Unit also projects education statistics, using the grade progression method and age participation rates. Enrollment projections at the K-12 level are done for the state as a whole; numbers of high school graduates are projected by county. The latter is a major input in the enrollment planning of the three segments of public post-secondary education in the state. Work is currently underway to develop projections for community college districts, because recent state legislation requires an annual estimate of the adult population for these districts.

Unfortunately, neither the projections nor the technical assistance of the Population Research Unit has been well-utilized by California's school districts. Many districts seem unaware of the Unit's work, others unsure how they can use it. At present, the Unit does not have the resources to conduct an outreach program or assist any great number of districts. The development of such a program

would be a useful contribution to educational planning in California and elsewhere if it were used as a model for programs in other states. California would probably be the best location for a pilot project in the development of state-assisted local population/enrollment projections, because the state already has an active demographic unit.

The development of a pilot project to help school districts project their future enrollments and thus plan rationally would be one of the most productive uses of educational research money that agencies such as the National Institute of Education (NIE) could make. It would greatly aid administrators in developing long-range plans.

Suggestions for Responses to Fluctuations in Enrollments

Aside from developing procedures for *determining* likely local changes, it would be useful for educators to consider strategies of flexible *response* to rapid enrollment change. That rapid change is occurring and will continue to do so is virtually certain. Some areas will be more affected than others, but nearly all will be affected to some degree. Two possible examples of flexible response to rapid enrollment change include lease arrangements for prefabricated buildings centrally owned by the state or by a group of districts, and one year certification programs for holders of appropriate bachelor's or master's degrees.

The idea of a flexible physical plant arises from accounts of school closings and the attempts of school districts to sell or rent the property. Apparently it is not easy to find alternative uses for school buildings. In fifteen or twenty years, many districts may again need more classrooms, although differently distributed than those in existence today. This need, because of fertility change or migration, could be fairly short-lived.

While discussing strategies apparently designed to increase rapidly the supply of teachers seems a little out of place in today's market, the one year post BA or MA credential system actually allows for both rapid increase and contraction of the supply of new entrants. A person need not make a firm commitment to the field until the senior year and thus one need not predict the [illegible] market four years in advance. Plans for encouraging a rapid increase may n[illegible] entirely out of place, however.

A study by Peter Morrison points to the possibility of a teacher shortage sometime in the 1980s.[37] The factors thought to underlie this possibility are:

1. The poor job market, which is discouraging students from entering teacher training.
2. The age structure of the current stock of teachers and especially of the reserve pool of teachers, which consists of former teachers who left the field to raise families and whose children are either grown or considered old enough to leave. By the late 1980s, a large number of these persons, products of "the initial large cohorts of teachers produced in the mid-1950s, will be reaching advanced ages."

3. A projected annual increase of 2 to 3 percent a year in school enrollments, due to begin in 1985.[38]

I do not intent to defend either of the above strategies. I suggest them merely to urge educators to plan for flexible response to future ups and downs in enrollments. This should include somehow minimizing lengthy investments by the taxpayers in physical plant and by individuals in preparing for careers.

Planning for the projected growth of the late 1980s and 1990s and possible later downturns will be relatively simple compared with managing the downturn which is now underway and which will continue through the 1980s. The buildings are already there and so are the trained teachers committed to a career. The present crisis requires immediate action and outside technical assistance could be of particular help.

A number of districts have already faced the problem of surplus plant. It would be useful to collect accounts of how this problem has been coped with in various places, along with general advice from such professionals as architects and economists or others with background in property management. Advice in the political management of school closings might be even more valuable. These case histories and suggested solutions could be collected in a manual to guide other districts facing decline. Perhaps task forces of persons with experience in such matters could be set up to supplement the manual and to aid districts faced with the necessity to close schools.

More serious than school closings, in my opinion, is the possibility of layoffs for teachers who have invested years in their professions. While some districts may be able to handle the matter through attrition, reassignments, and austerity (and could be helped to do so with experienced outside advice), others will not. The virtually certain 25 percent decline in secondary school enrollments will not be evenly distributed across the nation. Because of local age structure and migratory movement, some districts may well have secondary school enrollments drop to half, or less, of the current level. Teacher organizations and other groups should consider ways to ameliorate the impact of such layoffs, possibly including clearinghouses for employment outside as well as inside the profession, and insurance schemes to subsidize retraining. Such activities would be a useful addition to job security clauses in labor contracts because they would improve the future prospects for persons now teaching. At the least, teachers should be advised of the possibility of future employment problems because of projected enrollment changes in their districts and of ways to reduce the harm to themselves as individuals.

Even those districts which do not have to resort to layoffs will not be in a position to do much new hiring in the 1980s. While no scheme will make up for the loss of youthful energy and enthusiasm, "teacher renewal" programs could help moderate any tendency toward stagnation. For example, well designed in-service training could help keep experienced teachers up to date on promising new teaching methods and developments in their academic fields.

At the university level, the sabbatical system helps refresh teachers and also brings in new faces as their replacements. It is doubtful that most districts could afford a sabbatical program, but federally-financed fellowship programs might

help. Another way to achieve some turnover would be for teachers to occasionally switch places. Ideally, a school which has developed an exciting new program would "lend" teachers to neighboring districts, who in turn would send some of their teachers to the innovative school. Opportunities for retraining and placement in other jobs might encourage those tired of teaching to leave the field, opening positions for the more enthusiastic. With the expected reduction in teacher turnover, another fruitful field for revitalization would be to establish teacher renewal centers. I do not intend to argue for the above suggestions, but I do encourage educators to plan for virtually certain future circumstances.

References

1. *HEW, Projections of Education Statistics to 1984/85*, 1976, pp. 18, 21. See table 1 and figure 1.

2. *Ibid*, Table B.1, "School-age Population Ages 5-6 and 5-13: US 1964-1985," p. 153.

3. In fact, that drop is shown to occur in seven years; 9-12 enrollments in 1983 equal those of 1984. Recall the temporary increase in births foureen years before these dates (1969 and 1970).

4. *New York Times*, Sunday, October 17, 1976. Personal communication, Isabel Hambright, Population Research Unit, California Department of Finance, in relation to their projected numbers of high school graduates.

5. Percent of persons 14-17 enrolled in school rose from 93.1 to 94 between 1964 and 1969, and then fell to 92.9 in 1974. Such variation could easily be caused by changes in age distribution in the 14-17 year old category. (*Current Population Reports*, p. 20, no. 278, Table 2, p. 4.) The same publication shows an increasing convergence in white and non-white rates of attendance in this age group. By the early 1970s, the rates were virtually equal.

6. *Current Population Reports*, Series P-25, no. 601. "Projections of the Population of the United States: 1975 to 2050" (1975).

7. *Current Population Reports*, Series P-20, no. 277, "Fertility Expectations of American Women: June 1974" (February 1975).

8. Calculated from HEW, National Center for Health Statistics, Advance Report Final Natality Statistics, 1975, vol. 25, no. 10, December 30, 1976, p. 10.

9. One can see this intuitively by thinking of each cohort's life-long contribution to fertility as a piece of taffy. If each cohort stretches out its contribution, the sum of the cross-sections in any one year will be less.

10. Timing decisions cause another uncertainty in the projection of annual fertility. Given the high level of control exerted by American couples over their fertility, yearly fluctuations in response to economic conditions, etc., are virtually certain. Since such circumstances, and the level of response to them, are unpredictable, the authors simply assume a smooth trend in annual total fertility rates toward the ultimate cohort fertility in each series. Quinquennial averages after 1980 avoid some of the problems of yearly fluctuations by arbitrarily assigning the presumed five-year average to the entire half decade.

11. SOURCE: *Ibid*., p. 6, Table 3, and National Center for Health Statistics, *Monthly Vital Statistics Report*, vol. 25, no. 6 (August 27, 1976). Births are calculated by fiscal years beginning July 1.

12. *Monthly Vital Statistics Report*, vol. 25, no. 6 (August 27, 1976).

13. *Current Population Reports*, Series P-25, no. 601 (1975).

14. If later childbearing becomes the mode, growth will be much slower, but it would continue into the twenty-first century. Under Series II L, there would be 19 percent more children age 5-13 in 2000 than in 1980; under Series II, there would be 19 percent more. Series II R is

somewhat similar to Series II L in the sense of continuous growth beyond 1995, but the rate of increase is faster. If its assumptions were correct, there would be 15 percent more children in that age group in 2000 than in 1980 and the growth would continue into the first two decades of the twenty-first century.

15. It is, however, difficult to separate the cohort effect from poor economic conditions in general. Between 1970 and 1975, the unemployment rate for men twenty-five to thirty-four rose more rapidly than that for men twenty to twenty-four. *Current Population Reports*, Series P-20, no. 292, "Population Profile of the United States: 1975 (March 1976), p. 30.

16. Richard A. Easterlin is the most prominent. See his "The Conflict Between Aspirations and Resources," in *Population and Development Review*, vol. 2, nos. 3-4 (September-December 1976), pp. 417-25.

17. The proportion of persons twenty-five to thirty-four with college degrees increased between 1970 and 1975 from 15.8 to 21.4 percent. About 30 percent of persons twenty and twenty-one years of age were enrolled in college in 1975, so the *proportion* of degreeholders in the future does not appear likely to diminish. (CPR Series P-20, no. 292, p. 19). The *numbers* of persons in the twenty-five to thirty-four age group will continue to rise into the 1990s. Thus, the numbers of persons under thirty-five with college degrees in the 1980s and 1990s will be considerably larger than those aged fifty and older, whom they will seek to replace in the promotion ladder. For a discussion of the falling returns to a college education see: R. Freeman, *The Declining Economic Value of Higher Education and the American Social System*, Aspen Institute for Humanistic Studies, Occasional Paper, 1976; S. Dresch, "Demography, Technology and Higher Education: Toward a Formal Model of Educational Adaption," *Journal of Political Economy*, p. 83.

18. i.e., at the level generated by Series II and its variants or by Series III, actual hardship or lesser opportunities might not be necessary. Richard Easterlin made the point that the parents of the baby boom children had been raised in hard times and did not, therefore, have high levels of material expectation. They could satisfy both a desire for children and raise their standards of living in the expansive 1950s. R. Easterlin, *Population, Labor Force, and Long Swings in Economic Growth: The American Experience* (New York: Columbia University Press, 1968), Chapter 5. For many people born in the 1920s and 1930s, the decision to have a third and fourth child came while they were enjoying a level of prosperity and security they had never imagined possible in their childhood.

19. *Current Population Report*, Series P-20, no. 228, June 1975.

20. This has been observed in a number of follow-up studies. An interesting example is found in the *Current Population Report*, Series P-20, no. 277 (February 1975), Table C, p. 4.

21. By 1975, women in the cohort born from 1941 to 1945, who were then thirty to thirty-four years old, had already borne 2.209 children. The completed fertility of the 1906-1910 cohort (at age 50-54 in 1960) was reported to be 2.18.

22. T. Parsons, "Age and Sex in the Social Structure," *American Sociological Review* 7 (October 1942): 604-16.

23. The population of college-going age declined in the 1950s as a result of the low fertility of the 1930s; however, the increase in the rate of attendance more than compensated, so that enrollment increased.

24. U.S. National Center for Educational Statistics, *Projections of Educational Statistics, 1979-80* (1971), p. 23, and *1981-82* (1973), p. 24.

25. U.S. Census Bureau Survey, reported in the *Chronicle of Higher Education* (March 17, 1975), p. 1.

26. Carnegie Council, *More than Survival* (San Francisco, Calif.: Jossey-Bass, 1975), p. 29.

27. *Ibid.*, pp. 31-32.

28. *Ibid.*, p. 31.

29. *Ibid.*

30. Carnegie Commission, *New Students and New Places* (October 1971), pp. 53-54.

31. *Ibid.*, p. 3.

32. *Ibid.*

33. Over the long-term, the relative role of increasing population has not been great. Between 1870 and 1970, college-age population almost quintupled. (Based on persons 15-24, *Historical Statistics of the United States, Colonial Times to 1957* (Washington, D.C.: 1960), Series A71-85, and *Statistical Abstract of the United States* (Washington, D.C.: 1972). During this period, total degree credit enrollment increased nearly 150 times, from 52,000 to 7,760,000 (Carnegie Commission, *New Students and New Places*, p. 128).

34. Carnegie Commission, *New Students and New Places*, p. 127.

35. Numbers of births were 57 percent higher in 1947 than in 1937–3,817,000 compared to 2,413,000. This implies an average annual rate of increase of 4.6 percent. Between 1947 and 1957, the increase was 12.9 percent (from 3,817 to 4,308,000 births) or an average annual increase of 1.2 percent).

36. Carnegie Council, *More Than Survival*, p. 3.

37. Peter A. Morrison, *The Demographic Context of Educational Policy Planning*, Rand Paper Series, P-5592, p. 17-22.

38. *Ibid*, pp. 18-19.

Chapter 7

Time and How It Is Spent

Charles S. Benson

Introduction

The question this chapter addresses is whether time budget studies of households should be included in the research agenda of educational finance and productivity.[1] The nature of "time budget studies of households" will be made clear in the discussion to follow; here, suffice it to say that the term refers to the collection and analysis of data on uses to which children, their parents, and other children and adults closely connected with a given family's life, put time. In our version of such studies, attention is focused primarily on the activities of the school age child; the time uses of other persons are examined mainly in relation to activities jointly pursued by the given child and those other persons (parents, the child's peers, other closely-connected persons). The work "activities" deserves emphasis—we identify types of activities pursued by the child in the home and outside; when an activity is jointly undertaken, we identify the persons with whom it is pursued; we note how the child gets to the place of activity, if it takes place outside the home; and we examine the conditions under which different activities are taken up and put aside.

These data are the bare bones of our version of time budget studies. Data on activities of the child are, in our view, analyzed properly in a multi-varied context. The context includes information on the education, income, and occupation of parents. It recognizes data on family structure: whether there are one or two parents in the household; whether there are zero, one, or two parents employed; number of siblings, sibling order and spacing, etc. The context provides data on the characteristics of the child's neighborhood and school. It contains data on the child's school performance. Time-consuming activities of members of households are constrained in varying degrees by the inter-related variables of socio-economic status, family structure, neighborhood, and school.

To understand better how the context of family life shapes patterns of time uses, we collect, as well, information on child and parent attitudes. Opinions are sought on definitions of success and failure, educational aspirations and expectations for the child (as reported by parents), preferences for different types of activities (as reported by the child and his parents), desire to spend more or less time with parents (as reported by the child), feelings positive or negative toward different aspects of neighborhood and school (as reported by the child and his parents), etc. These are the softest data in our kind of time budget study and are mentioned last for this reason. Attitudinal surveys have their own uses even when nothing is being surveyed except attitudes, but we believe that reported attitudes are best analyzed in concert with data on activities. What is challenging is to try to understand inconsistencies between reported attitudes and the uses to which time is actually put, on the one hand; and, on the other, to try to understand differences of opinions towards life chances, neighborhood, and school as expressed by members of the same household. These are preliminary comments about the context of time budget studies of households, as we define the term.

Defining "Out-of-School" Environment"

Schools exist to help transform children into adults, i.e., into persons capable of working for money, of engaging in home production, and of making satisfactory use of the residual of non-work time. Schools find it appropriate to make a *rank ordering* of children in terms of the probability of their success as *future beings*. The ranking is influential in indicating how long a given child should expect to attend institutions of formal education, the types of institutions he should expect to attend, and the kind of employment he should eventually enter. Admittedly, "success," as the term is used in this context, is defined primarily by market criteria. Schools do not commonly rank children for success in moral behavior or artistic creativity.[2]

From the middle sixties, research has shown that school performance of children and the consequent rankings assigned to children by schools are strongly influenced by the "home background" of children.[3] This finding is one of the most robust in social science research. Home background is conventionally measured by SES variables relating to parents, such as income, education level, and occupation of head of household. Though the finding is robust, it has not yet proven to be terribly useful for policy purposes. Indeed, its most direct translation to action, given the emphasis placed in this country on the importance of eradicating school failure, is sterilization of low-income, poorly-educated adults or forced adoption of poor children by upper-income families. To say the least, neither of these policies is acceptable. Even to mention them is to ridicule social science research on the charge of false determinism.

Rather than speak of family background, let us consider "out-of-school environment" (OSE). It is reasonable to surmise that OSE has effects upon what

children do in school. Indeed, the research cited above about the relation between home background and school performance, however imperfectly the concept of background is defined and measured, supports this point. In relating OSE to school performance, we must admit the entanglement of a large number of variables, which, for convenience, we place in the following taxonomy:

SES variables, per se. The fact that certain children have parents of *high income* may mean simply that those children are favored in terms of their health and nutritional status, in access to private lessons complementary to school lessons, in being given instructional toys, etc. These conditions may be considered separately from the possibility that children of *highly educated* parents hear a more acceptable standard of English spoken in the home (acceptable, that is, to teachers), are encouraged in reading by the example of their parents, etc.[4] Thirdly, there may be effects of the *interaction of income and education* variables. Highly educated parents may know better how to obtain information on out-of-school programs for their children (the effect of parental education) and to be able to afford the requisite fees, costs of instruments, transport to the instructional center, etc. (the effect of parental income). Thus, the combined effects of SES establish opporunities for, or likelihood of, certain behaviors on the part of parent and child that could influence the child's school performance.

Family structure variables. In the U.S., as is well known, the number of single parent families, both female and male headed, is on the rise. Furthermore, in a growing number of two parent families, both parents are working outside the home.[5] These developments affect the amount, if not the quality of time that adults spend with children. As we shall see below, there is some evidence to indicate an association between the amount of time parents spend with their children and the I.Q. of children. (This association is not necessarily causal.)

Recognizing that family structure variables are related to SES variables, the association is confounded by the fact that when a woman in a two-parent family goes to work, household income ordinarily rises; furthermore, the woman, by the fact of being employed, may receive an additional amount of education. The rise in SES level of the family should have a positive effect on the progress of children, offsetting in some amount any deprivation suffered by loss of parental attention. The net results in any case might be related to the initial SES level of the family, i.e., the level of income and education prevailing before the wife entered the labor market.

To make this suggestion is to expose our ignorance about quality of parental time imputs. Fifteen minutes a day given by an alert, sensitive parent may offer more to a child than two hours a day given by a listless, inattentive one. There is no reason to suppose, moreover, that quality of parental time is constant from one period to the next; it may well be affected by changes in family structure.

So far, we have considered possible effects in parent-child relations when the mother in a two-parent family enters the labor force. The changes may be of a different order when a single parent goes to work, in that uncertainties about

holding on to a job, and the weariness of carrying alone the burdens of work and home, may introduce strain and testiness into what had previously been a warm parent-child relationship. Accordingly, such families might be described as "time poor" as well as money poor: indeed, some families, no doubt, strive to pull themselves out of financial poverty by throwing themselves into the category of time poverty.[6]

Having said this much, we must in honesty admit how little we know about parent-child interactions. In single parent families, so defined at a moment of time, there may be parent substitutes available, and the patterns of time during which such substitutes are present may have important effects on children. Likewise, it may be an important matter as to the age of a child when single parent status of the family was achieved. The proximity to the absent parent and the amount of time the absent parent may be willing to devote to his (her) child may be a significant variable. So may the patterns of work of the remaining parent, i.e., whether full-time or part-time, professional or non-professional. One recent study indicates that the simple fact that a mother is working has little to do with her child-rearing practices. "... working and non-working mothers, who are of similar cultural background and family circumstances, are very much alike in philosophy, practices, and apparent relationships with their children."[6a] This assertion calls for further investigation.

Additionally, there are studies to indicate that number of siblings in a family and sibling placement are significant variables in determining school achievement. It would seem important to understand better how family structure variables interact with SES variables in determining parental attitudes toward their sons and daughters and attitudes of siblings toward each other.

Family attitudes. Families may differ remarkably in their attitudes toward success, the root causes and probability thereof, and many other things. One set of parents may feel deeply about the importance of spending time with their children and another may not. One set may be thorough in selecting among alternative types of parent-child interactions and another may not. One set may set to exert close control over their child's use of time and another may not. Children, even children in the same family, or even the same children at different times, may have different views toward the importance of school success and toward the relative worth of an hour's time spent in "study," meaning any future-oriented, preparatory kind of activity, and an hour spent in immediate, mindless pleasure.

It is reasonable to suppose that such attitudes as these affect school progress. But we know very little about how attitudes are developed. One might assume that they are related to SES and family structure; this seems implicit in the sociological literature that associates school performance with "family background" as measured by family income, occupation, etc. Thus, one might say that welfare mothers have bad attitudes to support their child's progress, and successful, middle-class parents have good attitudes. It may be morally important, however, to distinguish between attitudes of caring and attitudes of what we may call "projection." A welfare mother may care as deeply for her child as a middle-

class parent and make sacrifices of time and money for the child exceeding that commonly seen in middle-class homes. Yet, the welfare mother may, at the same time, fall into attitudes of hopelessness and despair in thinking of what kind of life her child will lead, i.e., the projection tends toward what her own life, and the lives of adults with whom she associates, is like. Such feelings of hopelessness might be conveyed to the child and dampen his enthusiasm for school work. If this indeed is what is happening in some families, we should not, of course, charge the mother with fecklessness. Conversely, children born into families of higher levels of income and education may be surrounded by attitudes that *assume the probability* of success in educational endeavors and that extol their worth.[7] These attitudes may impel middle-class children forward in school simply by making them more confident.

We can imagine the shape of parental attitudes and their effects on children's motivations, but we really know very little of the whole topic. We do not know much about what parental attitudes in different families are, how they change over time, what is the nature of their inconsistencies, and we certainly know little about how parental attitudes affect school age children. About measuring attitudes and assigning causal power to them, it is necessary to be modest.

We do know this much, however. Although educational productivity is class related, it is not perfectly enjoined to SES of the student's household. Some high-income children do poorly in school and some low-income children do very well indeed. We should examine the characteristics of these "outlyers" in the expectation of discovering patterns in their out-of-school lives that are basically different from other children in their SES categories. Thus, the main focus of the research proposal here is examination of processes of child development within—not between—SES categories. As we do so, we may be able to offer policy guidance on such major reform proposals as Burton White's suggestion that we should intervene in the child's intellectual and social development between the ages of 6 months and three years, on the one hand, and William Rowher's argument that we should postpone content mastery in school until after elementary school years.[7a]

Neighborhood variables. So far, we have discussed variables relating to the individual family: SES, family structure, and family attitudes. But there is interaction among sets of families and the children of which they are a part, with the sets being defined as families who live in a given neighborhood. For children, more than adults, neighborhood establishes a physical boundary of existence.[8] Children lack the means to drive themselves about in their own automobiles. They are limited, in the main, to bicycles, public transport, and being chauffeured. The last assumes inputs of parental time and capital and is presumably related to SES. Public transport schedules are geared more closely to the needs of commuters than to children—in most metropolitan areas, at least. Bicycle range is often limited by hilly terrain and by major traffic arteries.

The physical size of neighborhood may be different for one group of children than another, but we would generally expect its limit to be rather well defined and to represent only a very small part of a given metropolitan area.

(In small towns, of course, children may wander over the whole town's area, but most children in our country live in metropolitan regions, not in small towns.)

In any case, what exists is a set of conditions for the child's activities outside his own home. If the neighborhood contains playgrounds, children will likely engage in physical games appropriate to the playgrounds (except that playgrounds may be effectively closed to younger children if they are subject to physical harassment by older children). If the neighborhood contains a shopping center, children will hang around the stores. If the neighborhood contains a plaza and if the plaza is a site for mime and jugglers, children in the neighborhood will see them. If the neighborhood contains vacant, undeveloped space, children will allow their imaginations to roam in building shacks and staking out "secret places." If the neighborhood contains a library, a zoo, or a museum, some children, at least, of the neighborhood will attend. When any of these spaces or facilities in a neighborhood are lacking, most children in that neighborhood, so our preliminary evidence suggests, will *not* engage in those kinds of activities appropriate to that space or facility.

What is true of one child, thus, is true of most children in a neighborhood—the neighborhood sets a physical boundary of a child's life. Hence, any one child's playmates and friends are likely to live in his same neighborhood. In this country, we have a lot of residential clustering by social class, so any given child's peers are more likely than not to live in households in which income, education, and occupation are similar to that of his own parents. Insofar as SES of household has an effect on the child's attitudes toward, among other things, school, this kind of peer relationship might be thought to reinforce and strengthen the attitudes that are otherwise developed at home.

Further, the general milieu of a neighborhood may provide incentives and distractions to children that encourage or impede schoolwork. To be speculative, imagine it possible that middle and upper-class children find their neighborhoods sufficiently quiet that they are not unduly distracted from lessons, homework, and, in general, human capital building exercises, while at the same time children who grow up in slums may find sufficient stimulation and excitement in the crowded daily life around them that they find it hard to spend time to provide for tomorrow's enjoyment (at the expense of today's). In middle-class neighborhoods, what adults do to make a living is likely to be concealed from the eyes of children; its mystery may serve to reinforce the idea that a long period of education is required of one to enter the world of work. In lower-class neighborhoods, the details of performance in adult roles are likely to be in plain sight of children, and children more easily move into and out of adult roles even at rather young ages. Thus, the need to engage in a long, strenuous period of preparation for adult life would be less strongly felt.

A Restatement of the Basic Question

We do not suggest that the above list exhausts the variables that determine OSE of children, but the list is intuitively obvious and sufficiently complex (both at once) to accommodate preliminary analysis.

To shift the metaphor, let us say that each year children attend school, they bring with them a lot of "baggage:" their health, energy levels, knowledge and skills acquired in formal learning and in informal activities, tastes, attitudes, and expectations. Presumably, some of this baggage is helpful to a given child in his schoolwork, and some is not helpful. We know very little about how to sort the baggage into categories (helpful, unhelpful, or of no consequence). Also, we know little about how the baggage is acquired by a child or, in some cases, forced on him. What we do know is that the differences in school performance of children is greater than can be explained by the initial intellectual endowments and that the gap in performance tends to get wider the longer children attend school.

Our policy objective, let us assume, is to raise the average and reduce the dispersion of school achievement scores. To restate the basic questions of the paper, now in two parts: given the policy objective, we can learn useful things by studying the association between patterns of child life outside the classrooms and patterns of school achievement, i.e., is it appropriate to try to understand the nature of the baggage that children bring to school and its effects on their school performance? If the answer is yes, are household time budget studies, as broadly defined above, an appropriate, though not necessarily an exclusive, research strategy to gain that understanding?

An alternative to try to understand OSE and its effects, presumably a cheaper alternative, is the attitudinal survey *per se*. Our belief—and we admit our bias—is that children communicate more clearly and thoroughly through what they say they do than through whay they say they feel (though analysis of attitudes helps illuminate the significance of activities). We suggest the worth, then, of making observations of what children *do*. Next, we examine patterns of behavior in terms of their associations with SES, family structure, and neighborhood variables. Finally, we attempt to relate all of the above to school performance.

The intent is to explore the question of "joint causality." Suppose one finds an association between SES and parental interaction with children, such that when middle-class parents behave in certain ways, their children typically score higher in school than their initial endowments would suggest. Suppose a group of low SES parents have behavior similar to those of the "successful" middle-class parents, but let the low SES children appear to receive markedly different amounts of benefit from their parents' behaviors (benefits being measured by gains in school performance). In other words, for similar parental behaviors, dispersion of school scores becomes greater as we move down the SES scale.

One could then seek to discover whether there were combinations of parental action and school or neighborhood variables that produced school success. Parental action might be of relatively weak effect unless the school had a warm atmosphere, minimized tracking, etc. Likewise, a "favorable" school environment might have a relatively weak effect unless parental behavior supported school performance. Certain particular neighborhood conditions might override the effects of parental and school efforts both. Up to now, studies of deter-

minants of school performance have not given much scope to study of joint causality.

The policy implications are along the following line. The chief thing we know about determinants of school performance is that it is closely associated with differences in "home background," where home background is measured by parental income, educational level, and occupation. It is not possible easily to change the levels of parental income, education, and occupation in the short run—nor is it at all certain that changing such descriptions of family life would have any desirable effects on the child in the short run. So public policy, as it has shaped up, seeks to "compensate" for deficiencies in home background, but this compensation is offered in the absence of knowledge of precisely what is lacking in the home, toward which compensation should be made.

Now, if it is parental behavior that affects school performance, we would expect some of the achievement-stimulating kinds of behavior to be in the grasp of parents of whatever level of SES. This seems to be the assumption underlying parenting education. But we cannot be certain that given types of parental behavior will work well with all types of children. More important, we do not know much about how different kinds of parental behavior are reinforced or cancelled by different kinds of neighborhood conditions and by different kinds of institutional behaviors. In the absence of such information, policies toward children are fragmented—and, in attacking the problems of school failure, ineffective. A more comprehensive approach to studying OSE might allow us to design a better integrated and more effective set of policies toward youth.

The remainder of this paper is written in several parts. First, we shall describe the theoretical work of Gary Becker on the "economics of time." The articles that Becker has written represent an ideal starting point to pose hypotheses about intra-familial allocations of time, at least some of which, as we shall see, are subject to empirical analysis. Becker's concern is to show that people's behavior, in the uses to which they put time, is explained by such economic variables as the market prices of different commodities and services, wage rates, and household production functions, the latter determining, *inter alia*, the net benefits of investments in future productivity in market and non-market activities. Children are seen to have their own individual production functions, under which they transform today's time into tomorrow's utilities.

Second, we shall note and discuss briefly several empirical studies of the uses to which members of households put time. This section is intended to describe the state of the art of time budget analysis and to indicate some major gaps in our knowledge about the effects of different time use patterns on children.

Thirdly, we shall propose a new line of investigation, somewhat more comprehensive than studies so far undertaken. With few exceptions, the studies to date have obtained data on the activities of adults only. We shall propose studies that draw information on activities of children and of their parents and that these data be analyzed in a context of information about family SES, family structure, family attitudes, characteristics of the neighborhood in which the child lives, characteristics of the school the child attends, and his school performance.

Lastly, we shall pose some policy issues that this new kind of research may illuminate.

The Theory of Time Allocation

In conventional demand theory, consumers maximize utility functions of the form

$$u = u(y_1, y_2, \ldots, y_n),$$

subject to a resource constraint

$$\Sigma\, p_i\, y_i = I = W + V$$

where y_i are goods and services purchased in the market, p_i are the prices of those goods, I = money income, W = earnings and V = income from capital. The consumer is in equilibrium when, for each pair of market goods, $\frac{p_2}{p_1} = \frac{mu_2}{mu_1}$, where mu_i refers to the marginal utility of market good i. Conventional demand theory does not deal explicity with any of the following matters: (1) the fact that goods and services can be produced in the home as well as the market; (2) the fact that different goods and services obtained by the consumer, whether in the market or at home, require different amounts of time for their use or enjoyment, i.e., some goods are time-intensive in their use and some are not; (3) that the relative prices of time-intensive goods presumably change from one period to the next; (4) that goods and services may be used to enhance one's future productivity in the market and as a consumer; and (5) that different members of different households face different opportunities to exchange time for money income. Yet, these previously-omitted phenomenon may be crucial in explaining the behavior of parents toward children, of children toward parents, and, ultimately, in casting light on the school performance of children and youth.

Fortunately, these omitted phenomena of consumer behavior have been addressed in a theoretical way by Gary Becker.[9] Becker postulates that households act as producing units, to provide themselves with utilities, Z_i under a household production function of the form

$$Z_i = f_i(X_i, T_i),$$

where X_i is a vector of market goods and T_i is a vector of time imputs. Thus, households are "both producing units and utility maximizers." The utility function to be maximized becomes

$$U = U(Z_i, \ldots, Z_m) \equiv U(f_1, \ldots, f_m)$$
$$\equiv U(x_1, x_2, \ldots x_m; T_1, T_2, \ldots, T_m),$$

subject to a budget constraint

$$g(Z_i, \ldots, Z_m) = Z,$$

where g is an expenditure function and Z is a bound on total household resources.

If wages and prices are constant for a given household, i.e., are independent of the choice of a particular set of Z_i, then the market goods constraint can be written as

$$\sum_{1}^{m} p_i x_i = I = V + T_w \bar{w},$$

where p_i is a vector representing the unit prices of market goods X_i, T_w is a vector of hours spent at work, and $\bar{w}$ is a vector of wages per unit of T. The time constraint is

$$\sum_{1}^{m} T_i = T_c = T - T_w,$$

where T_c is time spent at consumption and T is total time available. Thus, the full cost of a unit of Z_i can be expressed in time, for time is the ultimate resource. The full cost is the sum of the time spent in consumption, T_i, i.e., time spent in non-market production, and of time used in market production, T_w to acquire from the market goods and services x_i, that are complementary to T_i in the creation of utilities Z_i. According to Becker, both time spent directly in consumption and time spent in acquiring goods for consumption represent earnings foregone or "lost." This reasoning seems to accord a higher place either to market work or to savings than to work done in the household or to the satisfaction of the household's needs generally--puritanism run rampant! Which is to say that work and saving become ends unto themselves.

Does Becker's extension of consumption theory cast light on such a seemingly distant phenomenon as performance of students in school? Can it conceivably be a starting point in analysis of why students from certain classes of families systematically succeed in school and why children from other classes of homes systematically fail? Let us consider the following points.

The Family as an Allocating Mechanism

Members of households allocate money income and time to the production of utilities. Each member of the household has a claim to the time of other members; that is, each member gives time *to* other members and receives time *from* others members. In child bearing families, moreover, let us assume that parents are solely responsible for the allocations of money income to the individuals of the household; however, it is safe to say the parents have but limited control of the utilities created by their children. They do not control fully the child's allocation of the child's time that is used in combination with market goods and services for production of the child's utilities.

Time, along with goods and services, may be allocated to future utilities as well as present. Letting, as before, T_w represent time at work and T_c time spent in consumption, then T_e might stand for time spent in education, broadly defined. Total investment in the ith period of time would be $t_{ei}\ x_{ei}$, where x_{ei} represents the market goods input toward building human capital.

Households make investments not only to improve their productivity as workers in the market but also to improve their effectiveness as consumers. Some Z_i are time-intensive in the present period, but some Z_i also require considerable prior investment of time (and possibly of goods) for their creation. For example, it takes considerable investment of time to learn to play the piano well. On the other hand, it takes almost zero investment of time in previous periods to be able to create Z_i by watching television.

In Becker's view, children, as seen by parents, are time-intensive goods. That is, they require parental inputs of time to maintain their present health and happiness, but this is not all: both parents and children make time investments in the children's future production and consumption utilities.

Time Responses of Parents

According to Becker, there is a life cycle determining allocations of time to T_w, T_c, and T_e, largely determined by market wage rate. At younger ages, the possibilities for earning money are nil. As one becomes older, one can enter the market, but one's wages are low. In the early stages of life, then, it makes sense to use time in consumption and investment (schooling). In the middle years of life, wage rates for the individual are at a peak, and this condition raised the yield of T_w relative to T_c (unless, of course, a major shift in tastes has occurred in the meantime). T_e once past the younger years may decline in value, monotonically, because of decline of strength in the learning function, because of the shortening of the period of years in which new skills may be used, etc., or it may rise in value because knowledge already acquired creates demand for new knowledge, especially in complementary consumption fields. But, in any case, in the middle years, high wages draw time to the workplace and away from consumption. In latter years of life, time shifts from T_w to T_c as levels of wages obtainable by the individual fall.

The basic pattern of time use cannot but seem to affect the ways that children grow up. During the younger formative years of the child's life, the head of household is most intensively involved in work, and work is something that is done outside the household. The child is deprived not only of the attention but even the presence of the working member(s) of the household. Family structure in the U.S. does not any longer allow grandparents to share in child rearing, though, presumably, grandparents—under Becker's theory of the life cycle—would have time for child rearing tasks. Next, children live through their early formative years at a period in the lives of their parents when time is spent mainly on the present, T_w and T_c. Parents are unlikely to set an example of investment in the future, though much investment is what parents demand of their children.

The pressure on adults of child bearing age to devote time to work, T_w, is a function of the life cycle of wage rates. The pressure to devote remaining time to consumption, T_c, instead of investment, T_e, is, according to Becker, a result of a relative decline in the price of time-intensive consumption goods. What were formerly luxuries are now widely distributed among the population; examples are ski holidays, foreign travel, air planes and sail boats. This brings us to an interesting point regarding time allocations made by mothers. We have noted earlier that a rising number of mothers in two parent families are entering the labor force.

This may be seen partly as a response to a relative rise in the real wage rates of women. But, paradoxically, it may also be a response to the relative fall in the prices of time-intensive consumption goods. Other things equal, such a relative fall in price should cause people to withdraw time from the work place and devote it to consumption. But the possibility exists that the wife will place a low value on T_c during the working day, during, that is, those hours when the husband is away from the house. She might then prefer to go to work to help finance the purchase of time-intensive consumer goods that she and her husband can use together on weekends and holidays. If the consumption activities take place outside the home, the child may lose twice: the attention of his mother in the afternoon and the attention of both parents on weekends and holidays.

These are extensions of an argument that sees human action as determined by the "economic approach," i.e., as action which is always calculating and rational—and, hence, basically selfish. As Becker says, "When an apparently profitable opportunity to firm, worker, or household is not exploited, the economic approach does not take refuge in assertions about irrationality, contentment with wealth already acquired, or convenient *ad hoc* shifts in values (i.e., preferences). Rather, it postulates the existence of costs, monetary or psychic, of taking advantages of these opportunities that eliminates their profitability—costs that may not easily be 'seen' by outside observers."[10] The economic approach is thus broad enough to accommodate other explanations as to why married women enter the labor market: to heighten their self-esteem, to help a family maintain a middle-class income position in times when the given family is, relatively speaking, a victim of inflation and not a beneficiary, etc. But the basic point in this: adult members of households allocate goods and time to their children strictly on the basis of the "profitability" of those allocations to the adult's own private, utility-maximizing welfare function. Economy-wide changes in our society can enhance that profitability or reduce it (unless we make the unlikely assumption that individual preference functions are immune to economic change); ultimately, changes that affect the profitability of the household contributions to children should be reflected in school performance of children.

Time Response of Children

For younger students, which is the class I am writing about here, Becker assumes that there exist no opportunities to spend time at work, i.e., $t_w = 0$ because $\bar{w} = 0$. Hence, child's time is divided between consumption, T_c, and education (equals investment in human capital) T_e. The optimality conditions for the allocation of the child's time are derived as follows:

$$U_i f_{xi} = \lambda \frac{1}{(1+r)^i},$$

where

$$f_{xi} = \partial f / \partial x_i \quad U_i = \partial U / \partial C_i,$$

under the interpretation that a person's welfare is related to objects of choice, embodying goods and time, C_i; λ is a Lagrangian multiplier equal to the marginal utility of wealth, and r is a rate of interest;

$$U_i f_{ti} = s_i;$$

where $U_i f_{ti}$ is a utility expression corresponding to $U_i f_{xi}$ but referring to allocations of time, not goods, and s_i is the "marginal utility" of an additional hour spent in consumption of the ith period;

$$s_i = \lambda \sum_{j=i+1}^{m} \frac{\alpha_j t_{wj}}{(1+r)^j} \frac{\partial E_j}{\partial t_{ei}}$$

where α_j is the payment per unit of human capital in period j, t_w, as before, is time worked in period j, ∂E_j is the change in stock of human capital in period j and αt_e is time spent at school in period i = j - 1. Using this last expression for s_i and dividing $U_i f_{ti}$ by $U_i f_{xi}$, we obtain the ratio

$$f_{ti}/f_{xi} = \sum_{j=i+1}^{m} \frac{\alpha_j t_{wj}}{(1+r)^{j-i}} \frac{\partial E_j}{\partial t_{ei}},$$

Thus, time should be allocated to schooling on the basis of three factors: a discount rate, the return in wages per unit of educational capital, and the marginal productivity of the student's time in increasing his stock of educational capital. Discount rate aside, the marginal utility of an additional hour of a young person's time spent in "play" is future earnings foregone. ("Play," of course, is an activity at one end of a continuum, just as "study" is at the other; many activities of children are at the same time pleasurable and capital-building.) If one wanted to make the argument more complicated, one could say that it is not just future earnings that are foregone, since one might forego future consumption skills: educational capital presumably has two ends—productivity as a worker and productivity as a consumer.

Thus, according to Becker, the time of children who are doing well in school is an economically scarce resource; such children should spend their time out of school in study and in activities that support good performance in school. For children who are not doing well in school, we see that their time has a low value and might as well be squandered on frivolous pursuits. There are strong implications for income redistribution in this analysis. If we assume that children of low SES parents have been deprived in their pre-school years either of sufficient quantities of parental time, or have suffered from significant shortfalls in quality of parental time, such that the children do poorly in school at the start; then these children find little value in investing in their own development—and, presumably, their parents would find little "profitability" in trying to help their children play "catch-up," unless the probabilities of their children's being able to overcome initial disadvantage seemed high. The general direction of the Becker argument seems to be that poverty is passed from generation more or less inevitably.

The argument calls for empirical investigation, and such investigation should pay particular attention to matters of class. An advantage at the beginning of a child's school career, assuming it exists, could raise the value of $\partial E_j/\partial t_{ei}$, insofar as early aptitude tests are taken to represent ∂E_j. One must assume that initial endowment, insofar as it is subject to enhancement by parents, would be built

up by investment of parents' time in nurturing of children. We have used Becker's arguments earlier to suggest that middle-class families are under increasing pressure for both parents to enter the labor market. Hence, it is possible that poorer children grow up with more adult care than rich, if we assume that being poor is associated with long or frequent periods of unemployment for the adult members of the household. On the other hand, there are large numbers of two parent households in which full-time work by both parents fails to provide income much above the poverty level.

These points need to be considered in the light of the fact that income-poor families may be larger, requiring the parents' time to be spread over more children. (Becker argues that high wages earned in high income families cause them to restrict their consumption of such time-intensive goods as children.) On the other hand, parents may provide time to their children as a group, so adding an extra child does not necessarily restrict the amount available to any other child. (Parents' time is a "public good.") However, any such arguements that suggest a time-favored position of low-income children must also take account of the rise of single parent families and their concentration in the low-income sector of the population.

The investment of a child in his education, according to Becker, is a function not just of the child's marginal productivity in acquiring education but also of returns from that education. For a school age child, estimation of such returns is necessarily a speculative matter. Alfred Marshall long ago pointed out that future benefits are more highly prized if they are reasonably certain than if they are of doubtful prospect.[11] Class enters here possibly in the sense that upper SES children may tend to believe that assured returns to educational success will almost certainly be forthcoming, while children in the low SES families may view the matter much more skeptically. It must be recognized also that a child's commitment to building up his stock of human capital is not established by the child alone. Parents and teachers help shape these attitudes though we still know rather little of the process.

Furthermore, the Becker analysis assumes that a child's behavior toward acquiring human capital is devoid of any aspect of playfulness. Presumably, children have their own time discount rates, under which they weight future pleasures (as represented by the gradual, sometimes painful and tiring acquisition of knowledge and skills, primarily bookish) against today's pleasures (as represented by play, meaning activity pursued for its own immediate pleasure, in an atmosphere of spontaneity). If one asked a child, or an adult for that matter, would you trade on even terms an hour's pleasure today for an hour's pleasure a year from now, we can hardly imagine the person's agreeing to do so. A given child's time discount rate would logically seem to be related to his OSE, and it should also be affected by the environment of his school. An important empirical task is to see whether there are consistent patterns of association between characteristics of OSE, characteristics of school, and the time discount rates of individual children (as measured by willingness to spend time in future-oriented activities).

Are there reasons not covered directly in Becker's analysis to explain why some children devote large amounts of energy to the future while others do not? Tentatively, we may suggest three hypotheses on this question, each of which relates to class: 1) assuming that parents are *concerned* about their children's futures, it may be the case that higher SES parents simply have greater material and time resources with which to encourage and (in a variety of ways) supplement school and schooling-related activities; 2) some children from lower SES families may devote relatively large amounts of time to such tasks as care of siblings, housework and the like, cutting in to time available to master school lessons (or diverting attention and/or interest); and 3) across SES groups, what parents expect their children to gain from schooling (and what children themselves expect) may result in significantly different determinations of what constitutes a reasonable use of out of school time. These patterns may be reinforced by the fact that families of similar SES tend to cluster together in residential environments.

Interdependence of Parents' and Children's Time Responses

In some of his latest writing, Becker considers the allocation of parental time toward the human capital development of their children.[12] Parents will seek to equalize the values of $\partial E_k/\partial t_{pk}$, i.e., the marginal increase in human capital of child k relative to an hour's input of parental time for the benefit of child k. Assuming that ratios decline smoothly for successive additions of parental time, the set of ratios in a given family can be brought to equality, and the brighter children in the family will receive larger inputs of parental time. However, Becker postulates that parents seek to provide equal treatment to all their children; hence, they compensate the less gifted with goods.

Again, these are matters of empirical study. It is entirely possible that the brighter children in a family will draw into themselves either a greater amount or a higher quality (perhaps both) of parental time. In time-poor families, this may leave the children who appear less bright at early ages in a state of time deprivation.

We thus come back to the important question of the value of parental time. One can imagine that there is a threshold amount of parental time that is required for a child to be put on the path of becoming a successful adult. It may also be true that there is a required level of quality of time. Certain standards of income and education may in the past have seemed to be necessary conditions for such quality to be forthcoming. While this may seem to be an utterly pessimistic chain of argument, it leaves open the question of whether conditions setting the quality of parental time are inherent, say, in the educational level of the parent, *per se*, or are the result of the fact that educational level is associated with certain types of parental action, e.g., reading to the young child at bedtime. If this is so, there may be power in parenting education. Also, quality of parental time may be based not so much on educational level, *per se*, as on neighborhood conditions. If one lives in a neighborhood where practically all families regard themselves as economically unsuccessful, parents may feel depressed about the future and for this reason offer their children a lower quality of time than they would be able to

give if they lived in more cheerful surroundings. Lastly, pessimistic conclusions about the possibilities of some parents not being able to reach thresholds of quantity and quality of time to offer their children should be tempered by the thought that extra-familial institutions—school, recreational programs, neighborhood groups, etc.--may find the way to make appropriate compensation.

Time Budget Analysis Research

Previous Research

The Coleman Report states: "Studies of school achievement have consistently shown that variations in family background account for far more variation in school achievement than do variations in school characteristics."[13] Bowles in extending the analysis of the Coleman data, indicated that such variables as reading material in the home and family stability have a strong positive correlation with school achievement of children.[14] Given these well supported findings, it is somewhat ironical that the main efforts in the U.S. toward overcoming school failure are still thought to be concentrated in schools and in programs, such as Headstart, that are based primarily on the school model. Right at the start, then, one may ask two related questions: (1) How does family background affect school achievement, and (2) on the basis of such information, are there socially acceptable policy interventions to alter family background toward the objective of raising school achievement?

As I see it, the seminal time budget study from which one might begin to draw some insights on the first question is James Morgan *et al., Productive Americans* (1966).[15] The study was based on interviews with 2,214 adults in January, 1965. Morgan indicates that heads of households work more hours the larger the size of the family and the lower the rates of hourly pay. He also considers the propensity of wives to work in relation to husband's income (it is in inverse proportion). Putting these two things together, we might conclude that children in large families in which the father receives low pay are likely to see less of their parents, i.e., to consume less of their parents' time, on the average. However, the conclusion is much too simple a one: adults spend time in activities other than market work and care of children; they also spend time in housework, home production, volunteer work, self improvement, and leisure. (These categories are by no means exclusive; moreover, many people seem to be able to do two or more things more or less simultaneously.) Morgan attempts to assess time spent in all these various ways. He also investigates the "time horizons" of different families, in the sense, for example, of whether the family is putting money away for the college education of its children. Presumably, families with longer time horizons are more likely to provide motivational reinforcement to their children for doing

their schoolwork. For the present purposes, however, we must note that the study concentrated almost exclusively on activities of adults, not even raising questions about what adults do with children, much less about what children themselves do with their time. And it is certainly true that *vis-a-vis* children, parents have different ways of revealing the lengths of their time horizons than in putting money aside for college.

Continuing with data from the same basic source, we find results from the Morgan, *et al.*, study, *Five Thousand Families: Patterns of Economic Progress*, 1974, to indicate that the increment of time put on housework falls as the number of siblings in families increases.[16] Further, this reduction in average time in housework per child in family that occurs as family size grows is relatively small in high SES families and relatively large in low SES families.

Returning to the 1965 data of the Morgan, *et al.*, *Productive Americans* study, Hill and Stafford (1974) repeat the essence of the Morgan analysis to show that "the total housework hours spent by the wife on a child prior to grade school is about 1,120 for SES I, 2,060 for SES II, and 4,100 for SES III."[17] However, Hill and Stafford recognize that lower SES families have more children, on the average, than do higher SES families. Hence, the total time spent in housework (and presumably in child care) may not vary by as much as these figures indicate if housework (and child care) have the characteristics of a public good or if there are economies of scale in raising children. Without indicating sources of data, Alice Rossi has suggested, however, that housework is not a very good proxy for time spent with children. " . . . women devote to household chores four times as many hours as they do in interactions with their husbands and three times as many hours as they do in interaction with their children. It is housecare that keeps women at home more than child care."[18] She goes on to suggest that the Hill-Stafford type of result may reflect mainly the fact that middle-class families burden themselves with a lot of household goods that they must then take care of. Under that argument, household work might be positively related to social class at the same time that hours spent with children could be inversely related.

In a 1976 study of household production, Kathryn E. Walker and Margaret E. Woods draw a distinction, in discussing "care of family members," between "physical care" and "non-physical care."[19] The former refers to "bathing, feeding, dressing any family member," etc., and the latter to "all activities related to the (family members') social and educational development," e.g., helping children with school lessons. Data on time use patterns were collected from a stratified random sample of 1,296 households. Pertinent to our discussion here are the findings of no statistically significant relation between mother's education and time spent on non-physical care and of no statistically significant relation between employment of wives outside the home and the amount of time spent by family members in non-physical care.

Lindert (1974), using data from the 1967-68 Cornell Time Use Survey, reached the conclusion that a primary determinant of the success of a child is family size: as family size rises, the child's outlook falls.[20] Also, prospects for success are diminished for the middle children in a chain of siblings. His data were insufficient to confirm of deny the "plausible argument" that a more highly

educated mother would be more productive per hour spent in developing the potential of her child. "That issue is hard to test, given the multicollinearity between her observed productivity and such influences as the amounts of purchased inputs given to the child."

Siblings studies have suffered from the problem of omitted variables. That is, when one says that children from smaller families fare better than children from large, one may be measuring the results of parental attitudes toward achievement, not the fact that increase in size of family reduces the amount of care, on the average, given later children. The latter phenomenon is a more basic feature of large families than the latter. If, for example, parents of high achievement orientation restrict the number of their children because they believe concentrating their time on a fewer number is important to their children's success, and if the true operative condition is not parental time inputs but a contagion from parents to children of an attitude toward achievement, then possibly parents of large families could be persuaded to inculcate in their children positive feelings toward accomplishment and the plight of later siblings alleviated. In a recent article, Lindert attacks this problem by making the intra-family comparisons in achievement, using data from a sample of 312 senior male employees of a New Jersey utility company.[21] The respondents in 1963 were asked questions about, *inter alia*, the age, sex, educational attainment and occupational status of their siblings, living or dead. The familiar pattern of sibling achievement with regard to family size and birth order held up in this examination of intra-family achievement differentials. Lindert concludes: "There is a considerable body of evidence that reductions in fertility should raise the average level, and reduce the dispersion in schooling . . . the main reason for the effect of fertility reduction on schooling is simply that it gives each child more inputs from his family."[22] What is not clear from the above studies is how closely the author was able to control this data for the effects of SES.

Daniel J. Davis, *et al.*, have suggested another line of argument to explain the relative disadvantage of middle siblings.[23] Youngest siblings enjoy most whatever intellectual enrichment the parents provide to the family, but the influence of parental stimulation wanes as the number of children increases. However, as the older siblings acquire schooling, the intellectual level of the family would be raised by their contributions. Last born siblings would likely benefit from this older sibling contribution more than middle siblings. The effect should be most pronounced in families wherein the parents had rather little schooling themselves. Davis, *et al.*, tested their hypothesis using data of 191,993 Israeli eighth graders (1966 and 1970), and received statistically significant results. In addition, for a smaller sample, they reported: " . . . since school attendance was compulsory until the eighth grade, older children in the large families may soon have surpassed their parents in their ability to educate and help the younger children in the family. This hypothesis is verified by data from questionnaires obtained from a representative sample of 4,321 Israeli sixth grade children in 1973. With the number of older children held constant and with decreasing formal education for the parents, chidren are more likely to report that an older sibling other than a parent helps with homework and takes an interest in school activities."[24] These

findings are consistent with the proposition that educational level of the older persons in a child's life, even when those persons are no older than his own siblings, determines in some important way the quality of time inputs offered to him by the family.

Arleen Leibowitz, on the basis of analysis of the "Terman data," 1,528 high I.Q. children selected for study in the early 1920s, concluded that "home investments" serve to raise I.Q., even in the group of gifted, in significant measure. She took, then, I.Q. as a proxy for human capital and thus drew a relation between activities of parents in conducting instructional activities in the home and the size of the stock of human capital possessed by their children. Furthermore, educational level of the mother is positively and significantly related to schooling attainments of her children.[25]

In a later article, Liebowitz explored further the question of the importance of the mother's level of education.[26] Using data generated by Educational Testing Service for the Children's Television Workshop on the responses of a group of 3 – 5 year olds on various learning tests, Liebowitz found first that children's language development was significantly (negatively) related to size of family. Secondly, she reported a significant positive relationship between mother's schooling and her child's test score. However, and this is the interesting point, the significance of mother's education dropped substantially when the mother-child activities were introduced into the regression analysis. Liebowitz concluded: "The effect of maternal education on verbal development may be attributed at least partly to the allocative affect. That is, more educated mothers are more likely to engage in those specific activities that promote verbal abilities. Thus, when measures of those specific activities are entered into the regression, the coefficient of maternal schooling becomes no longer significantly different from zero. Reading with children appears to be the activity that most promotes verbal skills, and TV viewing appears to be the activity most detrimental to those skills."[27] There are, of course, non-verbal skills of communication. It would seem unfortunate if school success is closely linked to verbal skills and if school success is so highly regarded that processes by which non-verbal skills are developed were to be ignored.

Gronau (1976) has studied the effects of husband's and wife's market wage rates on allocation of time to market production, home production, and leisure.[28] "Children have a negative effect on their mother's leisure, the time withdrawn from the market falling short of the increased housework. As the child grows older and enters school, housework diminishes, but the change hardly results in any gain in leisure—the time saved in work at home is diverted back to the market." Finally, Aaron (1976) reported he could find little justification in social science research for advances in public expenditures on education nor on "complementary programs," such as nutrition, health, housing, or income maintenance, insofar as such programs were aimed at enhancing the child's capacity to learn in school.[29] However, he urged educators to plunk for more money for children as an article of faith, a faith sustained by the fact that research on children has so far been so poorly done as to represent no final contradiction to the point that if we spend money on children, something good might happen.

Let us make some summary observations. Most of the studies cited above, while dealing with problems of young people, are based on data obtained from adults. In the case of the Michigan (Morgan, *et al.*,) data, central to several of the studies, time spent in housework is assumed to be a proxy for time spent by parents in child development; that is, a question was not asked in a fashion to divide parents' time as between mopping the floor and reading to children. (This is not true in the Walker-Woods study, however.)

Mainly, these studies appear to rest (at least) on one major assumption and one kind of implicit assumption. The main assumption is that whatever influence the family has on the child is largely completed by the time the child enters school.[30] Since it is difficult to do survey research on 5 and 6 year olds, it follows that asking questions of children is either (a) too hard or (b) not productive of useful knowledge because the child old enough to answer questions is too old to be helped by the findings. The implicit assumption seems to be the following: children are passive recipients of parental care. If parents decide to enhance the endowment of high ability children and not to enhance the endowment of low ability children, the results on the children are as the parent ordains. That is, the present behavior of parents determines the future behavior of children, as children have not the power to selectively accept or reject what the parents offer.

The first assumption should be tested, it would seem. Children may well be subject to formative influences from family and neighborhood up to age 12 or older. If so, the child himself becomes able to describe his life and to speak to some of the influences upon it.

The implicit assumption, on the other hand, appears to fly in the face of common sense. It is hard to imagine that children do not selectively accept or reject what their parents offer. Broadly speaking, our own data, to which we shall refer in more detail below, suggests that parents can be divided along a continuum in their relations with their children. At one extreme are activist parents. Activist parents we see as those who not only read to their children but help with homework, engage in common enterprises on weekends and during summers, seek out opportunities for their children to participate in lessons and games, and involve themselves in school programs. Non-activist parents, at the other extreme of the continuum, might, almost literally, do none of these things. Let us assume that all relatively activist parents are concerned to some extent with the future development of their children. Some activist parents may exert themselves from defensive motivations: "If I don't give my child guidance, he will waste his time and do bad things." Some activist parents, on the other hand, may be less compulsive and may act on response to cues their children give them as to what interests the child would like to explore next. In our data, degree of parental activism is not strictly related to social class. We hope this will allow us to study children's persistence of participation in various activities relative to degree of parental activism, by social class.

With respect to parental investments in children, and setting aside for simplicity the role of siblings, we have indicated here three sets of variables: extent of parental involvement, types of parental involvement, and parental motivation for involvement. The first two have been examined in relation to children's mental

development and educational performance but not very thoroughly. The last has hardly been touched upon. Furthermore, as noted above, neighborhood and school set a context in which children's responses to parental investments will be forthcoming. It is hard to imagine that neighborhood and school do not have their own independent influences on the patterns of children's responses.

Lastly, we would note that parents are not the only older persons in the lives of children. We have some tentative evidence to indicate that children of low SES neighborhoods spend a lot of time with adults outside of school hours who are not their parents nor are closely related. This could be a kind of offsetting adult contribution to low SES children, making up at least to some degree for the alleged propensity of middle class parents to give their children more of their own time than low SES parents manage to do.

Time Budget Studies of Young People

Let us review the chain or argument up to this point. (1) Finance of education is a process of providing resources to schools. (2) Schools exist to help students learn and to become adults. (3) Policies that strengthen the capacity of students to learn are complementary to policies that are intended to provide financial resources for schools. (4) Economic theory suggests that households will allocate their own resources, i.e., parental time and money, to further the school careers of their children. (5) Empirical studies suggest that out-of-school life of students has significant effects on academic performance. (6) However, existing studies have not dealt thoroughly (a) with the time allocations of individual members of households and with their joint activities, (b) with the effects of SES, family attitudes, and family structure on activities, and (c) with the interactions among household activities, neighborhood characteristics and school characteristics. The question to be addressed here is whether more thorough studies are feasible.

Plainly, it is possible through survey research techniques to obtain information from adults about what they do with their time, what educational aspirations and expectations they have for their children, etc. It is also possible to describe neighborhoods and schools and even to get some feel of how neighborhood and school appear to the eyes of children. The operative question, then, is whether it is possible to obtain reliable information from children. Two recent investigations suggest that it is.

The Foundation for Child Development has sponsored a study conducted by the Institute for Survey Research, Temple University, in which 2,200 children aged 7 to 11 were interviewed, along with 1,700 of their parents. The interviews lasted one half hour. "The interviews . . . were structured to determine the general environment in which children live—their family lives, their friends, schools, health, and neighborhood activities—and to learn their perceptions, feelings, attitudes, and values." Preliminary results have been published, and a final report is expected shortly.[31]

Secondly, in the Childhood and Government Project, University of California, Berkeley, a survey on time budget format has been conducted of 764 children aged 11-13 and their parents under the direction of Elliott Medrich. Questions to

children covered activities with friends, activities alone, where activities take place, likes and dislikes about one's neighborhood, attitudes towards school and one's progress therein, organized school activities, organized out-of-school activities (lessons, etc.), mobility, school homework, household duties, TV listening, eating and sleeping arrangements, time spent with parents and siblings, activities with parents and siblings, all this (and more) along with strictly time-budget reporting—e.g., "let's talk about what you did yesterday, starting with when you got up . . ."

Questions to parents concerned neighborhood facilities, e.g., availability of recreational areas, quietness, traffic safety, police coverage, etc., educational aspirations for the given child, educational expectations for the given child, TV watching, activities with the child, efforts made to find new activities for the child, parent's own activities, including work for money, efforts made by the parent to control the child's use of his time, etc.

The sample is representative of the city of Oakland by SES and race. It includes substantial numbers of minorities in all SES groups. The completion rate of interviews (successful interviews/eligible participants) was very high—86 percent. The data are filed on computer in such a manner that we can call up tables on children controlling for parental characteristics and on parents controlling for children's characteristics. The data are supplemented with information on the child's performance in school, detailed descriptions of the physical characteristics of neighborhoods, and a limited amount of direct observation of the daily out-of-school activities of a sub-sample of children.

We would not minimize the difficulties of obtaining information from children. Interviewers must be carefully trained. Questions must be phrased delicately on many topics to avoid giving offense; questions must also be asked in such a fashion that a child's response is not given because it is what the child thinks the interviewer expects to hear; yet, the questions must take account of the specificity of children's language. The ground must be carefully laid to obtain permission, as a stranger, to talk to a child for, say, two hours without the parent being present in the room. Because of the expense of each interview, and the consequent restriction on sample size, sample design becomes extremely important. But the conclusion I would draw is that obtaining information directly from children in the 11-13 age bracket is eminently feasible.

One thing seems reasonably clear: children and parents systematically will give different answers to certain questions of the same type. When asked if they were afraid to go to certain places, 12 year olds tended to answer in the affirmative. However, a majority of parents indicated that they felt little or no concern for the physical safety of their children. In this case, I suggest the child's answer is more useful in explaining the child's behavior than the parent's answer is.

Policy Applications

If it is feasible to study activities and attitudes of children and the families of which they are a part, is it appropriate to do so? Are there important policy appli-

cations of the results? We venture to say that, as is generally the case with research, no one can be sure. However, we would like to discuss possible policy applications under three headings.

As Regarding the Family

An early task in household time budget studies is to see to what extent children's activity and child-parent interaction are related to social class. Do upper-class children engage themselves in a lot more human capital building things at home than do lower-class children? Let us assume so. We may then see that patterns of certain families depart from that expected by the income-educational status of their parents. Do such differences in family behavior reflect themselves in school performance or do they not? Recall Liebowitz's finding that a child's being read to by a parent wipes out, statistically speaking, the effect of mother's own education on the child's development. This is one activity only. Are there mutually reinforcing patterns of child's activity that appear to eradicate effects of class on the child's school performance?

We do not suggest we shall ever have truth on this kind of point, but approximate truth might have its public uses. Suppose serious effort in studying OSE establishes that activities $x_i, \ldots x_n$, as engaged in by children and parents, are associated with school success, without regard to class. This information might be the basis for quasi-experiments in parenting education, under which parents (and possibly children) would be given special incentives to adopt pattern $x_i, \ldots x_n$. Presumably, a number of these activities would be those now being stressed in parenting education, but the point is that parents could be informed more convincingly that "these things really seem to work."

Such experiments would run the risk more strongly than existing programs of establishing blame on certain parents and certain children when "these things don't work." There should certainly be continuing research to try to discover why changes in patterns of behavior are unproductive in certain instances and what barriers certain children and parents face in trying to become more effective by middle-class standards.

We recognize we assume that parents generally want their children to do well in school, i.e., that they seek a standard of middle-class performance for their children. This may not be so, but it is what our data tell us so far. That is, educational aspirations of parents for their children are uniformly high, but educational expectations are strongly associated (positively) with social class.

We also recognize that the arguments I have just been making may seem to be naive and excessively idealistic. Possibly, life in slum neighborhoods, with all its despair, frustrations, humiliations, and uncertainties is not something that can be overcome, in shaping the lives of children, by a change in child-parent activities. Bowles and Gintis have argued that our educational system supports capitalism by creating a hierarchy in the work force and by inculcating widespread acceptance of the rules of the hierarchy.[32] This may assume too great power in the educational system, and the argument generally fails to explain why teachers, parents, and children so passively accept such a process of early classification. It may be

that capitalism requires low-income ghettoes even more than it requires an educational system, because the slum may be the social condition of sufficient power to persuade low-income children that school success is unattainable and, if attainable, would not forestall a life of low-skill employment, along with frequent unemployment and poverty. Studies of OSE might give us additional information as to how far down in the social structure social mobility can be influenced by policy manipulation.

As Regards Social Agencies Other Than Schools

Lastly, studies of OSE should be useful in providing information to other social agencies than schools which serve youth. If the school performance of young people appears to be related in various ways to the participation of young people in organized activities—the nature of the relationship could, of course, be quite complex—then one would have a basis for examining the types of clientele served by different agencies, the degree to which those agencies display entrepreneurial spirit, i.e., the incentives they have to reach out for additional clients, and the general adequacy of their services, both in terms of quality and geography dispersion.[33] One might also try to discover why certain children are not inclined to participate (not necessarily assuming that they should) and why certain other children sign up and then quickly drop out. In agencies serving both children and adults, it should be interesting to discover if children are offered such quanity of services as represents a fair share of the budget—or do agencies obtain funds on the basis of case count of children served and then divert the funds to adult uses.

As Regarding the School

Certain schools have a better record with low-income youth than other schools. The same can be said of teachers. Up to now, we have little information on the extent to which these special school contributions are related to circumstance in the family life of the affected low-income youth and in their neighborhood lives, or stand in strict independence from the out-of-school lives of students. To put it another way, does the successful school, say, stimulate changes in the home and neighborhood activities of children? If the answer is yes, then possibly we could use the information about these particular kinds of changes to guide Title I programs. That is, we might come better to know what compensatory education programs should be seeking to compensate for.

Further, knowing that certain low-income families make strong efforts to improve the school prospects of their children (we are assuming we will know this), we could try to see if we can identify *school practices* that nullify or abort the parental efforts. Instead of assuming that parental efforts are cancelled out, if they are, by conditions in slum neighborhoods, we would seek to find out the extent to which schools themselves are the responsible agencies (as in Bowles-Gintis, perhaps schools do have such power). We realize we are speaking of research of a high level of sophistication, but we think we are all more aware today than formerly of the bad consequences of unsophisticated research in such topics as those we are discussing.

References

1. I am indebted to Elliott Medrich, Mary Berg, Mary Milos, and Victor Rubin, all of Childhood and Government Project, Boalt Hall School of Law, University of California, Berkeley, a project financed mainly by the Ford Foundation and Carnegie Corporation, and to James Guthrie and Kristin Anton, Department of Education, Berkeley, for help in this paper.

2. One could just as well imagine using time budget studies to improve the *present lives* of children. These two objectives presumably are related, but they are not really the same thing.

3. See James S. Coleman, *et al.*, *Equality of Educational Opportunity*, Washington, Government Printing Office, 1966; Frederick Mosteller and Daniel P. Moynihan eds., On *Equality of Educational Opportunity*, New York, Random House, 1972.

4. Since parents presumably affect their children's activities and attitudes by the example of their behavior in pursuing their own interests, we must admit that "quality" of parental time inputs to children has two main aspects: the effects on the child of direct parent-child interaction and the effects on the child of the "demonstration effect" of parental behavior.

5. See U.S. National Center for Health Statistics, *Vital Statistics of the United States*, 1976 and U.S. Bureau of the Census *Current Population Reports*, series P-23, No. 39, and P-6U, No. 97.

6. Claire Vickery, "The Time-Poor: A New Look at Poverty," *Journal of Human Resources*, Vol. XII, No. 1, Winter, 1977, pp. 27-48.

6a. Marian Radke Yarrow, *et al.*, "Child-Rearing in Families of Working and Non-Working Mothers," *Sociometry*, Vol. 25, No. 2, June, 1962, p. 130.

7. See Robert Coles, "The Children of Affluence," *Atlantic Monthly*, Vol. 240, No. 3, September 1977, pp. 52 ff.

7a. Burton L. White, "Reassessing Our Educational Priorities," Education Commission of the States Early Childhood Education Symposium, Boston, August, 1974; William D. Rowher, Jr., "Prime Time for Education: Early Childhood or Adolescense?" *Harvard Educational Review*, Vol. 41, No. 3, August, 1971.

8. Neighborhood effects are not the same thing as "peer effects" in the conventional educational production function, even though most of the neighborhood effects on a given youth *might* be received from other youth of approximately his own age. Consider

$$A_{it} = g\,(F_i^t, p_i^t, I_i, S_i^t),$$

where A_{it} = vector of educational achievement of the ith student at time t,

F_i^t = vector of individual and family characteristics for the ith student cumulative to time t,

P_i^t = vector of student body characteristics (peer influences), socioeconomic and background characteristics of other students in the school cumulative to time t,

I_i = vector of initial endowments of the ith individual and

S_i^t = vector of school inputs relevant to the ith student cumulative to [illegible] (This is a standard version of an educational production function as presented by Eric Hanushek and John Kain in "The Relationship between Inputs and Outputs—Some Conceptual and Methodological Issues," in F. Mosteller and D. P. Moynihan, eds., p. 123.)

It is assumed in such a function that

P_i^t takes on different values when the ith student is moved from one school to another in which student body composition is different. Hence, peer influence is affects put on a given youth by other youths he is with *during the school day*.

9. The argument presented here and on the next several pages is drawn from two articles by Gary Becker: "A Theory of the Allocation of Time" (*Economic Journal*, Vol. 75, 229, September, 1965, pp. 493-517) and "The Allocation of Time and Goods Over Time," (*Human Capital: A Theoretical and Empirical Analysis*, 2nd Ed., New York, Columbia University Press, 1975). Both are reprinted in Gary Becker, *The Economic Approach to Human Behavior*, Chicago, the University of Chicago Press, 1976.

10. Becker, *The Economic Approach to Human Behavior*, p. 7.

11. Alfred Marshall, *Principles of Economics*, 8th Ed., London, Macmillan and Co., 1920, pp. 122-123.

12. Gary Becker and Nigel Tomes, "Child Endowments, and the Quantity and Quality of Children," Stanford, National Bureau of Economic Research, Working Paper No. 123, 1976.

13. James S. Coleman, *op. cit.*, p. 218.

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22. *Ibid.*, p. 217.

23. Daniel Davis, Sorel Cahan, and Joseph Bashi, "Birth Order and Intellectual Development: The Confluence Model in the Light of Cross-Cultural Evidence," *Science*, Vol. 196, No. 4297, June 24, 1977, pp. 1470-72.

24. *Ibid.*, p. 1472.

25. Arlene Liebowitz, "Home Investments in Children," *Journal of Political Economy*, Vol. 81, No. 2, Part II, March-April 1973, pp. S111-S131.

26. Arlene Liebowitz, "Parental Inputs and Children's Achievement," *Journal of Human Resources*, Vol. XII, No. 2, Spring, 1977, pp. 242-250.

27. *Ibid.*, p. 249.

28. Reuben Gronau, "Leisure, Home Production and Work—The Theory of the Allocation of Time Revisited," Stanford National Bureau of Economic Research, Working Paper #137, 1976, p. 26.

29. Henry Aaron, "Healthy, Wealthy, and Wise: Backdoor Approaches to Education," Aspen Education Series, 1976.

30. There is a very large child development literature that indicates the importance of early nutrition, including gestational, and the importance of social interaction (play, stimulation) between mother (or other adults) and the infant. See the writings of M. D. Ainsworth, J. Kagan, N. Rodin, and H. R. Schaffer.

31. Foundation for Child Development, *National Survey of Children, Preliminary Results*, New York, The Foundation, 1977.

32. Samuel Bowles and Herbert Gintis, *Schooling in Capitalist America,* New York, Basic Books, Inc., 1976.

33. See F. Stuart Chapin, Jr., *Human Activity Patterns in the City: Things People Do in Time and Space,* New York, John Wiley and Sons, 1974, and Gerald A. Gutenschwager, "The Time-Budget-Activity Systems Perspective in Urban Research and Planning," *Journal of the American Institute of Planners,* November 1973.

Chapter 8

Schooling and Work: The Changing Context of Education

W. Norton Grubb

Introduction

For much of the post-World War II period schooling enjoyed a period of sustained growth. Enrollments were high, both because of the baby boom and because of longer average periods of time in school (including increased rates of college attendance). Higher education expanded, to the point where it looked as if it would become the same kind of mass institution that the high school had earlier become. Furthermore, economic growth and the development of sectors requiring high levels of schooling meant that there was no problem absorbing the higher numbers of college graduates. Teacher salaries increased, relative to those of other professionals; their status improved, and with unionization they found themselves with considerably more political power than they had ever had. Expenditure levels soared, on both the elementary and secondary and the college levels, and the sheer size of the educational sector increased dramatically.

Against this background, the contractions of the 1970's have sometimes been more surprising and seemed more severe than they otherwise would have. Declining enrollments at the elementary and the college levels, taxpayer revolts, local fiscal crises forcing cutbacks in spending, and serious problems in absorbing all college graduates in jobs thought suitable to their level of training have all reversed expectations based on experiences of the 50s and 60s. In part, these shifts have been demographic in origin: the historically low birth rates dating from 1968 and have caused declining elementary enrollments, which

could not have been foreseen, while the aging of the baby boom cohort has been responsible for a part of the decline in college-going. But the more important factor for education has been the economic decline of the seventies, with historically higher rates of unemployment, higher rates of inflation, and lower growth rates all coinciding. These indices of economic stagnation could be purely cyclical phenomena, presenting problems for schooling only in the short run. But there is also strong opinion, from across a wide ideological spectrum, that the economic problems of the seventies are symptomatic of fundamental problems facing capitalist economies, and are likely to remain with us in the intermediate or long run. In this vein, businessmen have complained about imminent capital shortages, caused in part by the expansion of the government sector. Some have warned about limits to growth because of limited raw materials,[1] while others have postulated social limits to continued growth.[2] There is some renewed interest among economists in longer-term business cycle models, like the Kondratief cycle,[3] and neo-Marxists have posed models which forecast the long-term stagnation of capitalism.[4] The thrust of all of these views is that the current economic stagnation is unlikely to be simply a short-run cyclical phenomena, but is evidence either of a downturn in a long cycle or of secular stagnation.

The ready implication for education is that, to the extent that educational institutions depend on the health of the economy for their own prosperity, the school problems of the 70s are unlikely to end with a cyclical upturn in the economy. There are a variety of ways in which longer-run economic problems manifest themselves in schools. One of the most concrete is the cutback in tax resources available for schools, as governments of all levels find themselves with fewer tax resources and as the size of the government sector seems too large to an increasing number of taxpayers. Similarly, the higher rates of unemployment in the seventies have made the absorption of graduates—particularly college graduates—somewhat more problematic, raising the issues of what level of schooling (or what distribution of schooling) is "optimal."

But the issue subsuming many of the problems which educational institutions currently face is that of productivity. Anxiety over inflation and low rates of economic growth have led to a concern about productivity in many areas, and schools have not been exempt. Some of the productivity issues are entirely internal to schools, as expressed in the fears that teachers have become less "productive" and in the attempts to make them more so through various accountability schemes, or in the "cost-quality" debate on the efficacy of school resources in promoting academic achievement.[5] Others—the ones which will be discussed in this paper—focus on the relationship between schooling and the labor market, and express in various ways the fear that schools have become unproductive in the sense of failing to prepare workers "optimally". There are various conceptions of optimality and productivity, not surprisingly. One emphasizes the rate of return to schooling, and argues that a world in which the rate of return has fallen is one where the supply of well-educated individuals is too high relative to demand, and where there is relative "over-education." An-

other view notes the expansion of levels of schooling without a corresponding increase in the skills required in different jobs, and worries that schooling has ceased to become directly productive of skills, but is instead merely a sorting device of one kind or another, a "credential". A third conception of optimality focuses on expressions of student and worker discontent and on the apparent irrelevance of schools to adult life (especially in the labor market). This has led to a search for an alternative curriculum in the schools, more appropriate to developing productive workers. It has been labeled career education to indicate that schooling should be more closely linked to adult careers.

The history of the post-war period illustrates the dependence of schooling on economic conditions. In the earlier period of growth schools shared in that expansion. But now that growth seems to have slowed, schools have suffered directly, as well as coming under scrutiny as possible causes of lower productivity and as examples of social resource misallocation. Research agendas have changed correspondingly. During the growth of the sixties, human capital models lauded the productivity of schooling, and explained growth during the twentieth century as due partially to increases in educational levels.[6] (Not incidentally, human capital models supported the liberal programs of compensatory education.) But with slower growth rates despite ever-increasing levels of educational attainment, this model appears to contradict the facts in important ways, and some alternatives to the human capital model have been proposed, reflecting precisely the fears that schooling for one reason or another is socially unproductive.

In this paper I will review three particular topics which have come to national attention in recent years, all focusing on the relationships between schooling and work and involving in some way the social productivity of schooling.[7] The first of these is the discussion of the "over-educated American", the possibility that too many individuals are receiving levels of schooling (particularly college and post-graduates degrees) which are in some sense "too high". The second issue is that of credentialing, the charges that schools are not directly productive but are instead sorting mechanisms, for any of a variety of rational and irrational reasons. The third issue is career education, which unlike the other two is an educational reform which is well under way, rather than an academic debate about educational policy, whose reform implications have not yet been explored.

For each of these topics, there are important research agendas which remain to be explored. But in every case, the policy issues which ought to focus the research are similar. Above all, the three topics raise the problem of how educational institutions should adjust to fluctuations in economic conditions. Assuming the necessity of some changes, the next issue is that of whether certain policy initiatives are necessary, or whether schools will adjust by themselves, either through market forces or through political pressures at the state and local level which constitute another kind of adjustment mechanism. Finally, and most abstractly, these three topics can be formulated so as to shed light on the issue of the independent impact of schools in the economic system, and on progress towards social goals.

The "Over-Educated" American

With one important exception, an educational historian looking at the educational turbulence of the post-World War II era sees little that is new. Community control, the use of the schools in compensatory programs, problems of discipline and violence, conflicts over methods of finance, local control, struggles over integration—all these have played themselves out in the past as well. But one phrase and one idea is new, startlingly so: the concept of the "over-educated American." Even in the 1960s we hailed schooling as a critical component of our economic growth, and on a personal level the ethic continued that more schooling produced an individual not only better equipped to function in the labor market but also one with greater personal capacities to enjoy an active participatory life and to enrich the lives of others as citizen, parent, and neighbor. Never, until the 1970s, has anyone suggested that less education might be better than more.[8]

At least some of the facts underlying the fear of "over-education" are undisputed. These size of the college-age cohort in the 1960s was unprecedented, a product of the post-war baby boom; combined with sharply increased rates of attendance, this meant a huge increase in the group attending and graduating from college. While rates of return to college stayed high and relatively constant throughout the 1950s and 1960s when the college group was expanding so rapidly, the differential associated with attending college began to decline in about 1969, so that the rate of return to college fell as well. Reports of college graduates (even those with M.A.'s and Ph.D.'s) having difficulty finding jobs, and being forced to accept jobs usually thought inappropriate for highly educated individuals, proliferated in the 1970s. Beginning in 1969 or 1970, rates of college attendance began to fall, though absolute levels of college attendance failed to fall immediately because the size of the college-age cohort was still growing.[9] Within colleges, an increased seriousness (or desperation) about studies replaced the political activism of the 1960s. Course selection has reflected a preoccupation with the vocational value of courses; rates of application to professional school has burgeoned, while applications to programs with less obvious vocational relevance, Ph.D. programs, for example, have slackened. On the other side of the college "market", many post-secondary institutions—particularly marginal colleges without established reputations, and private colleges with high tuition—found themselves increasingly pressed to find enough students, and some have been forced to close.[10]

One of the strongest interpretations of these "facts" comes from the human capital perspective. In this model, individuals respond rationally to rates of return to schooling. The high rates of return in the 1950s prompted high attendance rates in the 1960s, with the increased numbers of college graduates absorbed by a fortuitous increase in the demand for college-educated employees by government, education, communications, and the aerospace sectors. But beginning in the late sixties, the supply of college-trained labor began to outrun the demand, and this over-supply has since caused individual rates of return to fall. This will

in turn cause attendance rates to fall until the return to college is higher, high enough that the rate of return is approximately equal to the return on alternative investments. Thus the normal operation of educational "markets" will in time restore a relatively high rate of return to college education—perhaps not as high as in the 1950s and early 1960s, though certainly high enough to make college a worthwhile investment—but for a smaller number of people than are currently attending college. In the sense that there are currently too many college graduates to earn a rate of return as high as the return on alternative investments, there is "over-education."[11] One change which this model foresees is an end to the conception that more education would benefit anyone. Rather, college will be a rational investment for some, but labor markets will be unable to absorb a large increase in college attendance. The possibility of unlimited amounts of upward mobility through schooling has, in this model, been effectively closed.

Of course, a fall in the number of college enrollments and graduates could be slowed or eliminated by an increased demand for college graduates.[12] But this would not change the conclusion that the "market" for college-educated labor eventually comes to an equilibrium where the rate of return matches the return available in alternative investments, solely by the rational calculations of potential students. In this model, there is only one problem which might require any public policy initiative. If the period of time to equilibrium is too long, and the transition to equilibrium too painful to those college graduates who are unable to get the jobs they hoped for, then some changes may be necessary to hasten the equilibrium—as by making college enrollment more difficult or more expensive, by temporarily absorbing college graduates in the public sector, by reducing expectations of what a college education will bring, or by restructuring jobs so that they are still attractive to college graduates.[13]

The human capital model therefore encourages an attitude of benign neglect towards schools and towards school-work connections, since markets are trusted to work efficiently. But there are some other views, at variance with the human capital model, which suggest that the human capital description has misinterpreted some of the available facts and that different kinds of policy initiatives are appropriate. Testing these alternatives provides an obvious basis for a research agenda, one which is evidently policy-oriented. I will discuss them under four different categories: imperfections in the human capital model; demographic causes; credential models; and a model describing the degradation of labor. None of these alternatives has been as well-developed as the human capital model; indeed, some of them are wholly my own invention. I describe them here in order to suggest that there are some alternative explanations, at least plausible given what we know about schooling and labor markets, which merit serious consideration before accepting the human capital model as a complete explanation.

Imperfections in the Human Capital Model

One alternative might be described as the *persistent habit model.* At issue is the magnitude of responses to rates of return to schooling. In this version, rates of return to college become depressed by the over-supply of college graduates, but

attendance rates do not decline appreciably in response. Instead individuals of all classes tend to continue to college since public and parental subsidies reduce the direct costs (as distinct from opportunity costs),[14] because the social moratorium provided by college is pleasant and psychologically valuable; and because attitudes (including parental attitudes) towards the educational system and college attendance are too deeply rooted to reverse in a short space of time.[15] This model can in theory be distinguished from the human capital model with data on attendance available since 1970, although it would be impossible to distinguish the two with data available prior to 1970 because there is no conflict between the two models during a period of expansion and high rates of return.[16]

With continued high attendance, the private return to college would remain permanently depressed. This might imply that the social meaning of college would change: rather than a mechanism of upward mobility and a rational investment, non-pecuniary aspects—the consumption value and its value in preparing an individual for creative leisure—would be stressed instead. This has implications for the educational system as a whole, to the extent that it is currently considered a pyramid: one of the benefits to completing high school—the ability to continue in college—would consist of non-pecuniary benefits only, for example.

An alternative *denouement* for this model may be more consistent with American educational history. When falling rates of return fail to reduce attendance rates, then non-market mechanisms designed to reduce attendance while maintaining the image of schooling as a mechanism of upward mobility—"cooling out" mechanisms—are implemented; examples include stiffer entrance requirements and the current development of two-year colleges holding out the illusory promise of continuing to a four-year institution. For a variety of structural and political reasons, such policies would not be class-neutral, but would tend to foster greater attendance reduction among working- and lower-class youth than among middle-class youth.[17] The result would be a class-stratified system of higher education with some of the precuniary returns to college restored, and with the legitimacy of schooling maintained because of the subtlety of cooling-out mechanisms.

Another wrinkle on the human capital model might be called the *defensive strategy* hypothesis. This stresses the non-pecuniary benefits of schooling, especially higher class status and the chance to choose more jobs which are intrinsically more meaningful. Even when returns to college fall, schooling still remains the only means of access to higher class status and better work, except for the miniscule population with great wealth. In particular, most individuals use schooling to maintain their class status, to ensure that they do not suffer a fall in status. With lower rates of return, this implies that working-class and lower-class students would be unlikely to continue to college, since they would have little to gain from it; but middle-class students would continue to attend college because it represents the only possible way of avoiding a fall in class status. This version could be distinguished from the human capital model by examining class-specific responses to rates of return: lower-class individuals should display higher responses than middle-class students.

The defensive strategy hypothesis implies that higher education will in time become increasingly class segregated, reversing the class integration that took place after World War II. The legitimacy of the entire schooling system, based on offering equal opportunity and a route of upward mobility to all, would be undermined, as would its efficacy in legitimizing an unequal distribution of income. These tensions could be dealt with in a variety of ways, as by increasing the rate of return for lower-class youth or by erecting further barriers to middle-class attendance. But evidently a policy of benign neglect would in this case be insufficient.

Demographic Causes

Another kind of explanation of current problems in absorbing college graduates would emphasize demographic effects.[18] One possibility is that the current over-supply of college graduates is a problem associated only with the baby boom: the large cohorts of the post-war years had to be stashed somewhere, so that attendance rates went up because they couldn't be absorbed by the job market.[19] As the smaller cohorts born in the sixties come of college age, job prospects for their age group will be better since there will be relatively few young workers as a fraction of the labor force; they will not be forced into college attendance as the baby boom cohort was. This model depends critically on an assumption that workers of different ages cannot readily be substituted for one another—that young workers would not experience competition from the older workers of the baby boom.[20] This implies that the crises of over-education will be confined only to the baby boom cohort, which will be in a state of permanent depression. The important policy question is then whether and how to absorb this cohort.

In the models developed to forecast college attendance, the age structure of the population has been assumed exogenous. A step in the direction of endogenous treatment of demographic patterns is that of Easterlin, who has analyzed long swings in birth rates as a function of labor market prospects; birth rates in turn affect the labor market prospects of future generations. Thus the cohorts of childbearing age during the Great Depression had low birth rates because of poor job conditions and extreme uncertainty about the future. These small cohorts, coming into the labor market after World War II, faced rather good prospects partly because of the small cohort size, and therefore married young and increased the birth rate. This baby boom cohort, facing poor job conditions now, is postponing marriage and childbearing and will have low completed family sizes. By extrapolation, this cohort is likely to face good conditions in the late 1990s and have higher birth rates. This kind of Malthusian model must of course be integrated with other kinds of cycles and trends, and its projections would tend to be long-run. But this is an indication of how another element in forecasting the relation between school and work might be made analytically endogenous. In terms of policy, it suggests that population and educational policy must be aware of rather long swings in demographic behavior, and act accordingly.

Schooling and Credentialing

In the human capital model, the productivity of a marginal year of schooling is determined by a production function linking output to inputs of capital and labor of various education levels. The amount of highly educated labor utilized will vary as the price varies with demand and supply, and so the marginal productivity of education will vary, but the role of schooling in this process is fixed by the underlying production function. A very different model is one which this role is not fixed, but itself varies according to the supply of educated workers. In Thurow's (1972) job competition model, for example, particular jobs are fixed, and employers hire individuals into jobs on the basis of their position in a labor queue, with schooling being an important determinant of one's position in the queue. Thus when there is a large supply of college-educated labor, employers will find themselves able to hire college graduates into jobs which were formerly occupied by non-graduates. In the terms used by the Carnegie Commission (1973), some jobs are "educationally upgraded", or conversely some individuals find that they have been "occupationally downgraded", compared to their expectations of the kinds of jobs obtainable with a college degree.[21] However, productivity is a characteristic of jobs, not of education itself; education is used to sort individuals among jobs, and therefore has a private return, but does not increase overall productivity and therefore has no social return. (I will describe different models of credentialing in Section II below.)

In this version the current increased supply of college graduates can be absorbed by increased demand for college graduates, but this demand will be due to increases in the educational requirements for jobs which themselves remain the same. This change could then restore the wage differential between college-educated and high school graduates, and thus restore the previous high rates of return to college. Because the inflation of educational requirements takes some time to adjust to the increased supply of graduates, we may now be in a state of temporary disequilibrium, and will in the near future see the old rates of return re-established but with different kinds of jobs typical of college graduates. This model explains the educational inflation of the past, and when extrapolated into the future foresees a continuing educational inflation beyond the college level, with high rates of return stimulating high attendance and educational requirements continuously increasing.

Aside from temporary disequilibria, there are two serious problems if the credential model describes the relation between schooling and work. First, the marginal productivity of increased education may be quite close to zero, despite high private returns. This means that spending on increased levels of education—whether by parents, pupils, or the state—is socially irrational even though it may remain individually rational. If this true, then direct government subsidy (as in state financing of higher education) is inappropriate; at most governments should operate loan programs to correct imperfections in capital markets. A second problem is that the class status of jobs associated with any particular level of schooling is continually degraded, even when monetary returns stay high, so that the class expectations of college students are "excessive". This may simply be

a form of disequilibrium which is eliminated as pupils revise their expectations of the class status which can be attained with different levels of schooling. Alternatively, if the discrepancies between expected class status and the occupations actually available seem too great and generate dissatisfied or unproductive workers,[22] the state may hurry up the process of revising class expectations by educational practices like counseling, tracking, career education and the like.

The Degradation of Labor

By and large the models described so far have concentrated on the supply of educated workers, and have not generally investigated the sources of the demand for educated labor. The puzzle of why rates of return to college remain high while enrollments were increasing in the fifties and sixties was answered by the nature of demand: not only did those sectors which were intensive in their use of college-trained labor expand relative to others, but also the bias of technical changes happened to be such as to utilize more college-educated labor. But there is no reason for technology to continue to be biased in this way.[23] Earlier beliefs that technological changes inevitably increase skill levels have been revised; if anything, technical changes decrease the demand for skilled labor.[24]

When technology—or more broadly, the organization of jobs—is considered endogenous to the economic system, there are strong reasons for employers to develop the technology and reorganize work so as to minimize the number of skilled workers necessary.[25] Most obviously, as long as there are wage differentials between skilled and unskilled workers, replacing a skilled worker with one of lesser skills will, other factors equal, reduce the wage bill and increase profits. Second, the centralization of control and the separation of execution from all aspects of design of the work to be done, at the heart of Taylorism and the "management revolution", are facilitated by concentrating all skills in the hands of a few managers and foremen, again replacing skilled workers with unskilled workers and a much smaller number of skilled supervisors. Third, to the extent that the upper levels of the educational system instill patterns of initiative and independence—which is certainly more true of higher than of lower levels of schooling—then the centralization of control will again be more easily maintained by using workers with low schooling levels. Fourth, the specialization of tasks which accompanies the development of fragmented, unskilled jobs may imply some efficiencies, but more importantly they facilitate centralization of control since each specialized task is incomplete without the other parts of the production process, and only the foreman or the manager encompasses all aspects of production. Thus there are a variety of economic and managerial incentives to degrade the nature of jobs, in the sense of redesigning work (or developing technologies) so that most jobs require less skill.

When we look closely at the nature of work, we see that in specific occupations this has indeed happened. In factory production during the nineteenth century, relatively skilled craftsmen were replaced by semi-skilled operatives, with a smaller number of foremen and highly-trained workers like repairmen. Clerical work has been continuously degraded, from the nineteenth century

clerk who was more like an employer's assistant to the army of while-collar office workers who operate pretty much like an assembly line. Even such skilled occupations as engineers have been subject to degradation, as they become specialized and have some of their tasks taken over by the computers and lower-skilled draftsmen. The development of secondary labor markets is similarly a process whereby stable jobs with relatively high skill levels are transformed into low-skills, unstable jobs on a mass scale.

Since the degradation of jobs is motivated by the basic relations of capitalist production—cost minimization and consolidation of control—there is every reason to think that such degradation will persist in the absence of any efforts (such as those the state might undertake)[26] to reverse this process. The continuous degradation of jobs themselves does not preclude a general expansion of schooling levels, through the process of credentialing or through changes in the sectoral composition of the economy. This model implies more for the class status of jobs associated with college than for the returns to college in purely pecuniary terms: "over-educated" college graduates will find themselves in positions with reduced levels of responsibility, creativity, and control, contrary to their expectations and inconsistent with the personal abilities which colleges are now designed to foster. As mentioned in connection with credentialing models, it may be that this imbalance between expectations and the reality of occupations is quickly eliminated. But if this not true, then there remains the potential for widespread dissatisfaction which may require policity initiatives—either those designed to reverse job degradation, or those intended to change the expectations of students in higher education.

Above all, these four alternatives to the human capital model differ in the need for policy initiatives, as well as in the kinds of policies they imply. Where the human capital model views markets as appropriate adjustment mechanisms and suggests that little meddling is necessary, the other four forecast various kinds of tensions—among them increased class and racial stratification of schooling, radically different conceptions of the value of college, continued dissatisfaction in the labor force (especially among well-educated workers)—if the appropriate policies are not carried out. In addition, several of these approaches—particularly the credentialing view and the model of degraded labor focusing on the structure of jobs—indicate that educational policies by themselves may not be sufficient, but that more thorough changes in the structure of jobs and in their skill requirements is necessary.

Schools as Sorters: Signaling and Credentialing

Ever since the early sixties, the relationship between schooling and earnings has focused a great deal of research energy. This kind of investigation, begun in order to analyze the extent to which investment in schooling pays off, has since then been elaborated in a variety of ways: the quality as well as the quantity of schooling has been examined;[27] differences in returns by race, sex, and class

have been described;[28] differences by the class status of jobs[29] and between the primary and secondary labor markets have been documented;[30] regional variations have been observed;[31] and the influence of family background and ability on the return to schooling have been the subject of a large and hotly debated literature.[32]

From the vantage of the conventional human capital model, there is no problem with the interpretation of most of these results: the relationship between schooling and earnings describes the increased productivity of a person as a result of having attended school. Observed differences in rates of return may be due to differences in the quality of schooling or in the ability of individuals, but the school-earnings relationship still measures a real increase in worker productivity. The nature of the transformation in an individual during the process of schooling is not always specified, but it is generally thought to include some cognitive capacities and dexterities—like literacy, knowledge of calculus, skill at running a lathe, etc.—or, in another version, the kinds of values, goals, and personality traits appropriate to production in capitalist hierarchies. The focus on non-cognitive capacities—analyzed especially by Bowles and Gintis (1976)—is a very different kind of model, particularly in its normative view of schooling and in its policy implications. It is a cousin of the human capital view only in the sense that it describes the process of schooling as one in which abilities, broadly conceived, are embodied in individuals.[33]

But during the seventies a very different view of the relation between schooling and earnings has gained considerable currency—the view that schooling is not directly productive, but is instead used as a mechanism of selecting individuals into jobs with different levels of productivity and earnings. The reasons given for using schooling as a sorting device are varied, and not all of them are precisely described. The best developed are the signaling models of Spence (1974), Arrow (1973), and Stiglitz (1975). In these, the use of schooling to sort individuals is rational from the viewpoint of the employer because, under the assumption that only the most able individuals are able to complete higher levels of schooling, education conveys information about a person's abilities, even though it may itself have no independent influence on those abilities. Some other models capture the spirit of the signaling models, in which education is important for the information it conveys: in Thurow's (1972) job competition model, employers may rank applicants on the basis of education since it is a good indicator of their "trainability". The use of schooling is efficient for the employer, and is rational in that sense. But most of the costs of this signal are borne not by individuals but by society at large through the subsidization of schooling. The policy issue, therefore remains whether there are alternative signals which are equally accurate but which are cheaper, or alternatively whether there is a distribution of the costs of schooling which would more accurately reflect the distribution of its benefits.

The various signaling models have been developed in the context of individual employers operating in perfect competition. As such, the conclusion that education is efficiently used is embedded in the assumptions, since any non-efficient use would be eliminated by competition.[34] Similarly, the assumptions of these

models preclude any description of the use of schooling arising from the group action of employers or employees, since all employers and employees act individually in atomistic markets. But a variety of other explanations for the use of schooling as sorting devices deny these assumptions and conclusions.[35] In the work of Berg (1970), which in many ways began the current interest in credentialing, employers use schooling as a sorting mechanism because of unfounded and uncorrected beliefs that more highly educated individuals are more productive. Another explanation sometimes advanced, by Berg as well as many others, is that employers prefer to hire well-educated employees, for the prestige they confer or some other similar "non-economic"—that is, non-profit-generating—motive. From the vantage of signaling models, such inefficient uses of schooling should disappear, at least in competitive sectors of the economy, as some firms find that they can operate more profitably by not utilizing excess levels of education. The persistence of credentialing in this sense would therefore be testimony to the ineffectiveness of markets in eliminating inefficient practices.

A number of explanations have focused on the practices developed in large bureaucracies. For example, Rawlins and Ulman (1974) suggest that department managers in a large corporation may invest in more education that is economically rational so as to handle emergencies; the structure of the corporation permits them to do so since there are no mechanisms for calculating profitability—that is, comparing the marginal cost and the marginal product of additional years of schooling—at the departmental level. Another possibility is that firms hire individuals with inefficiently high levels of schooling at lower job levels, and some of them are promoted; this may be, in a large corporation, a more efficient process of selection than the alternatives. A third version is that bureaucracies need to develop rules of thumb, to minimize decision costs and information flows, to "idiot-proof" lower level decisions, and to justify decisions to higher level managers. Levels of schooling are of course the basis for simple and unambiguous rules of thumb.[36] All of these explanations imply that the non-rational utilization of schooling will be most prevalent in large, highly structured bureaucratic work places.

The various organizational explanations describe one form of market failure, since large bureaucratic organizations are often sheltered from the market forces by reason of their size (or perhaps by virtue of being a government organization), and can therefore develop internal practices which would not be rational in small, competitive firms. But another set of explanations for credentialing relies explicitly on market imperfections of various kinds. Rawlins and Ulman (1974) have described conditions under which a monopsonistic employer may employ individuals with higher levels of school than would be true in a competitive situation. Monopoly power on the other side of the labor market, in the hands of employees, has been widely recognized as leading to the inflation of educational requirements, especially in the licensed occupations such as medicine, law and the like. Collective bargaining may have the same effect in non-licensed occupations. Finally, where wages cannot be lowered, then the over-utilization of schooling can occur simply as a rationing device to choose in a non-arbitrary way among excess of applicants,[37] or it may develop in the course of business cycles as employers

substitute highly-educated workers for less well-educated workers during recessions, with a ratcheting upwards of education qualifications over time.[38]

A final set of explanations for the over-utilization of schooling explicitly recognizes the operation of class interests in labor markets and the threat of class conflict. At some level, these theories recognize that the relationship between schools and the labor market is one which has developed over a relatively long period of time—in this country, perhaps over seventy or eighty years—and therefore incorporates the results of past struggles over the distribution of social goods, various forms of class consciousness as well as discriminatory attitudes (both racial and class-based), in addition to the purely "economic" functions of allocating labor to different tasks. In one version, schooling has come to be utilized and thereby over-utilized to select individuals because it legitimizes the authority relations which characterize the capitalist mode of production, and because it is an effective mechanism for perpetuating the class structure from one generation to the next.[39] Another version recognizes that employers have an interest in relegating lower-class and non-white individuals to lower positions as a way of reducing labor costs as well as a mechanism of asserting control over the labor force by pitting one part of the working class against another—but may be unable to discriminate against lower-class and non-white individuals directly. In such cases, utilizing schooling as a sorting device accomplishes the same objectives, without relying on illegitimate methods of selection.[40]

In many discussions of the credentialing "problem," there is not always a recognition that some kind of consistent explanation is necessary. Similarly, much of the empirical work on the utilization of schooling has not been explicit about what kind of explanation is being tested. The work of Berg (1970) is an example: he presents a number of case studies in which the productivity of individuals—measured directly or by the ratings of their supervisors—is unrelated to their level of education, but the reasons why this might be true are never clearly articulated. Another popular kind of demonstration has been to show that educational qualifications in different jobs have risen faster than the educational requirements, as measured by the General Educational Development (GED) scores developed by the Department of Labor.[41] Aside from the serious problem that GED scores are not readily translated into years of schooling, and may not be particularly accurate in the first place, these demonstrations indicate nothing about the reasons why school attainments might have outrun requirements. The polls which find that the skills learned in school are irrelevant to what individuals actually do in jobs, that they are relatively unimportant in hiring or are misunderstood by employers,[42] constitute at best rejections of the human capital model, but they fail to distinguish among the wide variety of alternatives just described. Taubman and Wales (1973) have developed a complex and highly inferential method of comparing actual occupational attainment with the occupation which would be expected in the absence of any screening; this allows them to conclude that high school graduates are screened out of the better occupations, but again this constitutes a rejection of the human capital model rather than a specific test of any alternative.[43] Approaching the problem from the opposite perspective, Wise (1975) has defended the human capital view by demonstrating that increases in salaries among a homo-

geneous group of workers at the Ford Motor Company are related to some measures of educational attainment. Aside from the problem of whether salary increases do in fact reflect real productivity, as he assumes, this constitutes only a demonstration that the human capital model is not entirely wrong, and fails to indicate which other models might be valid and to what extent.

There have been a few efforts to deduce testable hypotheses from models distinctly different from the human capital view. One of these is Albrecht (1977). He tests the signaling model by looking at the utilization of schooling in two different groups of applicants for positions with the Internal Revenue Service: one coming from outside the IRS, for whom there is presumably relatively little information, and one group of IRS employees for whom there is relatively more information. He finds no evidence that schooling is used differently in evaluating these two groups, and therefore rejects the signaling hypothesis. In devising a test of "credentialism," which he defines as the utilization of education in an economically irrational fashion, he reasons that credentialism could survive only in organizations isolated from competitive pressures. In comparing the utilization attitudes towards minimum educational requirements for a sample of employers, differentiated according to various measures of competition, he finds some support for this model. Similarly, I have specified some tests of the signaling model and of the hypothesis that employers utilize schooling to screen non-white and lower-class applicants, looking at the returns to schooling across different labor markets. But efforts to construct testable hypotheses indicate what a difficult task this is. For example, does the greater utilization of education in large workplaces found by Albrecht indicate non-pecuniary ("consumption") motives, the hoarding of education by managers, or the need to legitimate complex authority relations? To complicate the situation, the signaling hypothesis implies quite the reverse: large firms can accumulate information on productivity from the observation of an employee, and may not need to rely on schooling as much as small firms without internal labor markets; I have confirmed that large firms do in fact rely less on schooling than small firms.[44]

In sum, both the theoretical and the empirical work on the use of schooling to sort individuals in the labor market are as yet underdeveloped. The various explanations ought to be more precisely formulated, and empirical tests need to be clear about the rationale for the phenomena they are describing. In many cases, the data necessary to test theories of signaling and credentialing must come from small individual work settings, with correspondingly high costs of data collection. The possibilities for utilizing readily-accessible national data such as Census data appear to be rather limited,[45] though additional research effort should go towards utilization of existing sources.

In fact, the development of policies to cope with the criticism that schools are inappropriately used as sorting mechanisms must await more precisely specified and tested models, since the policy implications of different explanations are not always the same. If, for example, schools act to signal those individuals of greater ability, then the appropriate policy would be to develop alternative signals which are more efficient and more equitable, or to finance schools in different ways. But it has also been suggested that, since the information conveyed in

schooling is information which is a public good, it is entirely appropriate for the state to subsidize schooling as is presently done. If market imperfections are responsible for the persistence of credentialing, the appropriate policy initiatives would be quite different than if signaling explains the over-utilization of education. If, on the other hand, credentialing, is a manifestation of class power and reflects a particular resolution of class struggle, then it is hard to know what kinds of policies, in the conventional sense, would be appropriate; nothing less than dismantling of the division of labor which creates class divisions would be appropriate. The only policy-relevant conclusion which has generated some consensus is that, if educational requirements are "too high" relative to some standard, then those groups who have been denied access to schooling—non-whites, lower-class whites, and (in some particular fields) women—have been inappropriately denied access to occupations with higher earnings, status, and other social rewards. The obvious implication is to restructure education so that such groups are not denied equal access. This conclusion is not essentially different from that implied by human capital models, but the various signaling and credentialing hypotheses add a greater urgency to efforts to implement equal access.

But the various theories of signaling and credentialing also suggest—contrary to the conclusions of the human capital models developed in the sixties—that the continued expansion of American education may be inefficient and socially unproductive, as well as inequitable. By itself this may be insufficient to halt the expansion of higher education and of public subsidies to higher education, if private returns continue to stimulate privately-financed college enrollment and political pressure for public support. But at the very least it represents a discouraging view of the productivity of schooling, one which demands a revision of earlier conceptions of schooling as well as a realignment of school and work.

Whether any of the signaling or credentialing models is correct is a debate still in its infancy among academics. But in the population at large, the phenomenon of credentialing—loosely used to describe any irrational use of schooling requirements—is more generally accepted as being true. Popular feeling against credentialing has begun to manifest itself in litigation[46] and in demands (often from minority communities) that educational requirements not be inflated—at the same time that credentialing is clearly expanding through the licensing of real estate brokers, insurance salesmen, paraprofessionals, and others. Above all, these developments indicate that the persistence and expansion of credentialing can cause serious tensions. Indeed, the legitimacy of both educational institutions and labor markets is threatened. This makes the need for policy initiatives more urgent, at the same time as it clarifies the insufficiency of educational policy; changes in job requirements are needed as well.

Career Education

During the seventies, career education (CE) emerged as a powerful movement seeking to reform schooling. Unlike those who worry about the extent of

"over-education" or those who claim that schools are simply credentialing devices, proponents of career education have concentrated more on the curriculum and the content of schooling than on the issue of how much schooling is optimal, though this has concerned them too.[47] The major attention of career education has also been focused on elementary and secondary education, rather than college and post-graduate education which has dominated the other two topics. But it shares important elements with them: CE embodies the view that schools have ceased to be productive, in its own phrasing that schooling has become irrelevant to adult life (including work); and it sometimes draws on the arguments about "over-education" and credentialing in its criticisms.

At its grandest, career education incorporates a wide variety of criticisms of schooling: that schools have become uninteresting; that they fail to provide students with useful and marketable skills; that they fail to reach a large proportion of students—especially minority and lower class individuals—who are "under-educated," while others continue mindlessly to college and find themselves "over-educated" for the jobs that are available; that schools reinforce sex stereotyping and thereby limit the choices which women are able to make. Career education sets as its goals the solution of these various problems, ostensibly by reorienting the curriculum around the careers and work which individuals can expect to experience. Careers and work are defined in the broadest terms: work includes all productive activity, whether paid or not, and careers include leisure and family life as well as a succession of jobs.

But the very scope of career education has made it difficult to describe its essence. At times CE appears to be a general thrust towards improvement in the schools, correcting every imperfection and eliminating every gap between goal and reality in educational institutions.[48] To understand the real core of career education, it is necessary to ignore many of the more general criticisms and goals and concentrate on those elements which are genuinely different from the rest of the elementary and secondary curriculum. Thus I understand the central thrust of career education to be the complete reorientation of the curriculum around occupations—paid employment. This in turn takes many forms, including "career awareness" courses beginning in the elementary grades to make students more knowledgeable about work and job opportunities; "experience-based career education," still in the stage of demonstration projects, where students work in apprentice-like situations and combine direct experience of the world of work with complementary academic learning; the creation of "career clusters" at the secondary and post-secondary level, curriculum tracks within schools which focus on particular groups of occupations; and specific skill training such as that which has gone on in vocational programs. These are the mechanisms, then, through which the sometimes diffuse goals of career education are to be realized.

Many of the criticisms raised by career educators have merit, and they have clearly found receptive audiences as the burgeoning of CE indicates. But at the same time the career education has spawned a great following, it has also been severely criticized.[49] As yet career education has not been in existence long enough for programs to be evaluated on the basis of what they actually do for

students, so the debate over career education has by necessity focused on the philosophical and empirical assumptions, the implied values, and the historical roots of career education.[50] The various critics of career education have challenged the validity of the assumptions underlying career education, particularly the assertion that infusing schools with information about work and with work-like activities will in fact make much difference, either to the kinds of decisions students make about continued schooling and employment, to the affection students feel for schools and their perceptions of schooling as "relevant," to future labor market success or to the solution of social problems like unemployment. A more pragmatic criticism has been that career education will simply not be supported by either business or labor, so that it will end up being another reform internal to educational institutions with no real impact on the ways in which schools interact with labor markets. A final kind of criticism, explicitly normative rather than empirical, is that to the extent that career education is successful, it represents a repressive curriculum, one designed to integrate individuals into the exploitive and degrading system of wage labor and to perpetuate class-based, racial, and sexual patterns of inequality.

For many of the assertions in favor of and against career education, there can be no real resolution since they are based on values, on readings of history, or on forecasts of the future of career education. But, whether fortunately or unfortunately, career education has survived to the point where programs have been implemented in a wide variety of schools. Thus it is now becoming possible to formulate some research which would either validate some of the empirical assumptions and expectations of career education or would deny their validity. In particular, the fact that career education at this point manifests itself in specific programs and curriculum tracks which generally coexist with the more "traditional" programs means that there is often some basis of comparison—between career education and other programs—which can be used to test the efficacy of CE.

In developing a research agenda, the scope and vagueness of CE and the refusal of some career educators to be precise about what it is designed to achieve present serious problems. Obviously a program of evaluation requires a series of objectives to be evaluated. In fact, one of the valuable outcomes of a serious program of evaluation would be an increase specificity of career education and a refinement of its objectives. On the basis of what I have identified as the core of career education, there seem to be four areas of research which would shed light on the value of career education.

First and most obviously, career education programs can be scrutinized to see if they make any discernable differences to students in them, compared with non-CE programs. Initially the basis of comparison may have to be those skills and attitudes learned in schools. Since, for example, career education has claimed that it would help hold the attention of students, a logical inference is that CE programs should (other factors held constant) improve traditional academic skills, such as reading and mathematical ability; at the very least, CE programs should do at least as well as others. Investigating the impact on academic skills would address the fears of some—especially educators—that career education may

undermine these areas of competence. Another kind of objective involves information about labor markets. One of the arguments most strongly advanced in favor of career education has been that students are uninformed about the labor market, and thus make educational decisions—such as the decision to continue to college—without any real information; on the other hand, the human capital model is based on the assumption (and some empirical findings) that the information available to students is fairly accurate (though it may be available only with a lag, particularly at critical turning points). Conclusions about whether career education students are or are not better informed about the whole of labor market prospects would support one or another of these views. A third example of the skills and attitudes which ought to be investigated includes attitudes towards schooling, towards work, and towards social institutions in general. An interpretation of any observed differences would depend on one's value system, of course, but a finding that career education makes some significant difference to attitudes is necessary before its value can be judged.

Ultimately, a verdict on career education must depend on whether it makes a difference to the adult lives—to the "careers"—of students enrolled in CE programs. This suggests a variety of criteria for evaluation. Thus if CE does live up to the claim that it can give students the information to make more "rational" decisions about school attendance, we should see a difference in subsequent college enrollment decisions. If CE in fact provides accurate information about job availabilities and the experiences necessary to recognize personal preferences, there should be a relatively greater congruence between an individual's field of preparation and his or her subsequent field of employment. If CE does in fact prepare individuals for "careers," then there should be relatively greater evidence of a logical sequence of jobs—e.g., those with progressively greater responsibility, complexity, or earnings—for those students from CE programs. If CE eliminates sex and race stereotyping, then female and non-white CE graduates ought to display a wider variety of careers than those who have not been in CE programs. If CE does in fact provide an individual with the flexibility necessary to adjust to changing labor market conditions, then CE graduates should display relatively fewer spells of unemployment and time out of the labor market than others. Programs should also make some difference to job satisfaction (for reasons which would require further examination), to patterns of leisure time use, and to adjustment to family life. The list of hypotheses which could be tested is much larger, of course; the important point is that they be addressed to the claims which career educators have made on behalf of their programs.

There are of course serious conceptual and methodological difficulties in comparing the results of different curricula. Such comparisons require longitudinal data, which is both difficult and expensive to collect. They require a control group, which is often non-comparable in significant ways. Above all, the problem that some selection process takes place—for example, that career education programs for some reason enroll less able or more able students than the school population at large—presents the possibility for substantial bias in the results even after controlling for such a selection problem. There has been considerable research evaluating curriculum differences, so that some experience has been developed in

handling these problems.[51] But whatever the difficulties, the point remains that assertions which are essentially empirical and which lay the basis for policy decisions merit testing. Only by carrying out evaluations of career education programs will it be possible to judge their validity.

A second general area of research starts from the claim of some career educators that CE will end the "mis-match" of workers and jobs by providing more precise information and training for those jobs which are available. Such a development would be beneficial indeed, since past efforts to match students to the jobs available have by and large failed.[52] More generally, it appears that career education has inherited the mantle of the manpower programs of the 1960s in that the demise of those programs has left career education as the only expanding program which attempts to correct certain social and economic problems—such as unemployment and underemployment—through education and training. Yet there are unresolved questions about the effectiveness of manpower programs[53] which apply with equal force to career education. In particular, the independent power of training programs in an economy with relatively slack demand for labor—as measured most obviously in historically high rates of unemployment—remain a highly debatable issue. In essence, the claim that any training program can reduce the "mis-match" between workers and jobs depends first on the relative number of jobs which go unfilled despite the existence of unemployment—either because of the lack of information, training bottlenecks, or regional patterns of demand and supply—and second on the ability of programs to forecast and then correct the sources of these imbalances. A research program which clarified the magnitude of these kinds of "mis-matches," specified the reasons for them, and identified programs which might eliminate them would help reduce some of the rhetoric in the claims for and against career education.

A third research agenda should focus on some of the criticisms of career education, particularly on the contention that CE is likely to reinforce the tendency of education to track individuals by social class, by race, and to some extent by sex. While CE claims that it will reduce such tracking, some of the early evidence is not particularly compelling: a content analysis of CE curriculum materials found that they promoted both sexual and racial stereotypes.[54] Thus it is important to monitor not only the content of career education, but also the class, race, and sexual composition of CE programs. One part of such a research agenda should focus on junior colleges, since the expansion of two year colleges has provided one of the mechanisms of social stratification through the "cooling-out" of various groups,[55] and junior colleges have been increasingly suffused with vocational and career education courses.

Finally, research on the possible signaling and credentialing functions of schooling will shed some light on the appropriateness of CE. If schools are essentially sorting devices, then the impact of career education at the margin is likely to be small. CE might result in different individuals gaining access to the best occupations—e.g., more (or less) non-whites, more (or less) whites from lower-class backgrounds—but in the aggregate it would be unlikely to make any difference to the aggregate productivity of the labor force.

So far career education has developed on its own, with relatively little federal money though with considerable leadership from the Office of Education. It is clearly a reform which has responded to some of the deepest fears which surfaced in the late sixties and early seventies, and as such has been independently proposed by a number of groups investigating education and work issues.[56] But the relatively small amount of federal funding may be greatly expanded in the next few years,[57] as there evidently is considerable political pressure behind career education. It is difficult to forecast the development of career education in the absence of policy initiatives based on evaluation. If its proponents are right, then it will restore interest in schooling, increase the productivity of workers, improve worker satisfaction, eliminate racial, sexual, and class-based discrimination in schooling, and correct various imbalances in the school-work link. If its critics are correct in their forecasts, then it will either serve as a repressive measure reinforcing docility and obedience in alienating work environments, increasing job satisfaction by lowering expectations, and furthering class, racial, and sexual stratification; or it will simply degenerate into another academic subject of no real importance. The debate is in large part political and value-based, and as such is unlikely to be decided on the basis of research. But a research program would at the very least give some empirical content to debates over career education.

Conclusion

Evidently, associated with each of the three topics I have covered is a potentially vast research agenda. There are, however, a few ways in which the three research agendas presented are similar, aside from their common origin in educational phenomena which have been widely experienced—the expansion of schooling levels without any sense of its purpose; the feeling of schoolings's increasing irrelevance and lack of efficacy, the falling returns to college and the realizations that college graduates cannot always get "college-type" jobs, and the like. First, the various hypotheses I have proposed are clearly different from the human capital models which have dominated research on the relationship between school and work. This suggests that the usual human capital agenda—the estimation of returns to schooling in particular—is insufficient, at least until such results can be more precisely understood.[58] Second, the problem of signaling/credentialing appeared in each of the three sections. When expressed in the most general terms as the issue of how selection in the labor market is related to schooling, and how skills required in jobs are related to skills in school, then signaling/credentialing in fact appears to subsume all the others. Finally, given what I take to be the dominant economic "fact" of our times—the rather sluggish economy of the 1970s and the prospects that slow growth and high levels of unemployment will remain in the intermediate or long run—all three of these topics in some way come back to the issue of what the marginal role of schooling can be in ameliorating labor market problems, in the absence of any efforts to change the structure of labor demand.

At the deepest level, the changed economic setting of the seventies allows us to perceive a variety of contradictory roles which schools have come to play. In

the period of growth after World War II, these various roles could all be simultaneously accommodated. The large cohorts of the baby boom could be stashed in school while still being promised—realistically, it appeared—that there would be skilled jobs for all of them. The educational system could afford simultaneously to increase teacher salaries, expand its plant, and provide compensatory programs. With employment and growth at high levels, schooling did not appear to be a zero-sum game. Compensatory education, some equalization of local school spending levels, a narrowing of the distribution of years of schooling, and even some racial integration could all take place without threatening the utilization of the schools by middle-class groups to maintain the class status of their children, and without generating the kinds of fierce battles which characterize such efforts now.

Now resource limits and high unemployment rates make every educational decision a zero-sum game; with clear winners and losers, explicit trade-offs, and highly political consequences. Integration efforts mean that whites lose control over schools. Resources flowing to women under Title IX mean that male sports programs are cut back. Efforts to equalize the distribution of educational resources threaten the relative position of the wealthy. Declining fiscal resources have put an end to automatic teacher salary increases, generating increasingly bitter fights and setting teachers against parents and children. If young people stay in school to avoid the depressed labor market and "youth unemployment," there is no guarantee that they will find the kinds of jobs they have been led to expect. Everywhere the contradictory roles of schooling are evident: between providing equal opportunity and social mobility and reproducing the class and racial structure; between legitimizing a particular economic and political system and providing students with a truly critical capacity; between preparing an efficient labor force for the mass of lower-level jobs and maintaining the promise of unlimited social mobility for all; between providing employment for a well-educated middle class group (teachers) and remaining a relatively efficient and inexpensive institution of social reproduction.

These contradictions were not readily apparent in the fifties and the sixties. With all institutions growing simultaneously, ascribing growth and productivity increases to schooling was plausible, and accommodating all demands on the educational system was possible. The changing economic context of the seventies, while it may force some major policy revisions, also provides us with sufficient information to re-evaluate the relationship between schooling and work. The resolution of the contradiction in education is necessarily a political and value-laden process. But our highest hope should be to utilize the information now available to perceive the regressive and atavistic solutions for what they are, and to reject them in favor of more progressive solutions.

References

1. See, e.g., Meadows *et al.* (1972).
2. Hirsch (1977).
3. Rostow (1970); Mandel (1975), Chapter 4.

4. For the beginnings of such theories, see Sklar (1969) and O'Conner (1975). The various short cycle, long cycle, and secular decline theories have been reviewed in Gold (1977).

5. For a review of this literature, see Averch *et al.* (1972).

6. Denison (1962); for a recent evaluation, see Nelson (1973).

7. I will therefore not discuss issues of productivity internal to schools, such as the efficacy of school resources in enhancing achievement and other educational goals (the "cost-quality" debate). I will also not discuss separately the use of schooling in promoting equality between men and women and between blacks and whites, but instead discuss this as it relates to the three topics discussed below. Nor will I review the topic of youth unemployment.

8. There have, however, been historically constant suggestions that more education is "unsuitable" for some groups within the population—i.e., the "manually minded." This has reemerged in the career education movement, discussed in Section III below.

9. Between 1970 and 1974 the group aged 18 to 24 grew from 24,683,000 to 26,908,000; this group will not cease growing in absolute terms until after 1980. *Population Estimates and Projections*, Current Population Reports No. 601, October 1975.

10. Most of the "undisputed facts" in this paragraph are presented in Freeman (1976). Some of the alternative scenarios presented below will indicate ways in which these facts might be challenged.

11. See especially Freeman (1976), and the technical articles underlying this: Freeman (1975) and (1977). See also Dresch (1975). These views have been widely popularized. Freeman suggests that, while the prospects of new college graduates will bottom out in 1979, they approach the golden years of the late sixties until the early 1990s; and the prospects for older college graduates will not improve until the 1990s. Dresch similarly suggests grim prospects in the intermediate run.

12. For example, The Carnegie Commission Report, *College Graduates and Jobs* (1973), looks for some increased demand, especially from the health sector.

13. A second policy issue might arise if the social benefits of higher education outweigh the private benefits. Then a policy must be devised that keeps college attendance at a high level while coping with the discontent due to relatively low private returns. Most commentators now feel that the marginal social benefits of college are fairly small; some, viewing student activism of the 60s with alarm, might even say they are negative. But Thurow (1974) has argued that—at least under certain assumptions about the nature of production functions—the marginal social benefits must be positive.

14. Most calculations of rates of return calculate returns to either total private or total public costs, not returns to the student on his or her own costs.

15. Much of the Carnegie Commission report, *College Graduates and Jobs*, assumes that college attendance won't fall very seriously with lower rates of return. Instead, the policy problem will be to find jobs (or "educationally upgrade" jobs) for the large numbers of those who continue to go to college. See the "major themes" in Chapter 1, especially the third one. Freeman (1976) includes the possibility of the persistent habit model in his definition of "over-educated" (footnote 5, p. 5). In terms of his model, the smaller the response to rates of return, the slower the adjustment to equilibrium. In the extreme case of no response, there is then no adjustment towards a new equilibrium.

16. This model may have to explain away the decline in rates of college attendance since 1969 or 1970. Two possibilities are to attribute high rates in the 60s to the Vietnam War and the threat of the draft, or to view the current decline as postponement due to uncertainty rather than a reduction.

17. The classic discussion of "cooling out" is that of Clark (1960). On the use of two-year colleges to cool out lower class youth, as a way of stratifying higher education the way the high school was stratified in the Progressive Era, see Karabel (1974).

18. Dresch (1975) has integrated demographic effects into his model, and in fact shows that these demographic factors dominate increases in attendance rates in explaining post-war college attendance increases. But in his projections, demographic factors are relatively unimportant.

19. Ryder (1974) shows how effectively the baby boom cohort was sopped up by college attendance rather than by the labor market.

20. There is no evidence that I know of on the substitutability of old and young workers. Substitution may be asymmetric. If the two groups have different skills—young workers are stronger, with more up-to-date knowledge, while older workers are experienced, wiser, and better at management—there can be relatively little substitution of young for older workers and vice versa. Some institutional rigidities—seniority systems and tenure, for example—hamper only the substitution of younger for older workers. If, on the other hand, older and younger workers are essentially substitutable for one another, then successive cohorts will live in the shadow of the baby boom, experiencing continued competition from them and suffering continual unemployment and underemployment in the absence of sharp falls in college attendance.

21. On educational upgrading, see also the *Manpower Report of the President*, 1972, Chapter 5, and Folger and Nam (1967). The term does not necessarily describe anything but a tautology: If a college graduate accepts a job formerly held by a non-graduate, that job can be said to have been "educationally upgraded." The term only describes a real change in the relation between schooling and work if employers come to require a higher level of schooling for the job, whether it is strictly necessary or not.

22. The possibility of a "new working class" of highly educated workers in jobs of rather low status has been investigated by Gintis (1975), Bowles (1974), and Levin (1976), among others.

23. For a pessimistic view, see for example Dresch (1975).

24. See Bright (1966), Horowitz and Herrnstadt (1966); and Silberman (1966) for anecdotal evidence.

25. This section is based on Braverman (1974).

26. For example, those critical of the emergence of the secondary labor market have called for the development of policies to prevent the transformation of primary sector jobs or to upgrade secondary jobs.

27. E.g., Weisbrod and Karpoff (1968); Johnson and Stafford (1973); Link (1973); Wachtel (1976).

28. E.g., Harrison (1972); Schiller (1971); Carliner (1975).

29. Wright and Perrone (1975).

30. E.g., Carnoy and Rumberger (1975); Osterman (1975).

31. Hanushek (1973); Grubb (1978).

32. E.g., Blau and Duncan (1967); Griliches and Mason (1972); Hause (1972); Morgenstern (1973); Taubman and Wales (1973); Bowles and Nelson (1976); Jencks *et al.* (1972), Appendix B.

33. For some empirical confirmation of this view, see also Edwards (1972).

34. This is not, strictly speaking, true. In Spence's (1974) model, for example, the equilibria he describes are ones in which *ex ante* expectations of individual abilities are confirmed by *ex post* experiences within the company. But there are no automatic mechanisms in his model to move from a disequilibrium situation to an equilibrium. It seems possible, therefore, for an employer to utilize schooling in some non-optimal way, and yet for him to be unaware of this and therefore not to move to an equilibrium.

35. The term "credentialism" has been used by both Albrecht (1976) and Brown and Bergmang (1977) to describe the use of schooling as a sorting device where it is not economically efficient. In practice, this refers to all models except the signaling theories.

36. See Albrecht (1976).

37. See Brown and Bergman (1977).

38. Rawlins and Ulman (1974).

39. See especially Bowles and Gintis (1976).

40. Brown and Bergman (1977); Grubb (1978).

41. Eckaus (1964); Scoville (1966); Rawlins and Ullman (1974); Ruchlin (1971).

42. See Rawlins and Ulman (1974); Golladay (1976), Table 5.3; reporting results from the National Center for Educational Statistics, National Longitudinal Study of *The High School*

Class of 1972, Management Bulletin No. 22, August 29, 1975; Diamond and Bedrosian (1970); Hamilton and Roessner (1972); Berg (1970).

43. This method has been reported by Mantell (1974) for the case of engineers, with conclusions similar to those of Taubman and Wales.

44. Grubb (1978).

45. Of all the empirical studies reviewed in this section, the only ones which use national data are those of Taubman and Wales (1973), Mantell (1974) and my own work; in addition, the comparisons of GED scores with actual educational attainments rely generally on data available from the Census and the Department of Labor.

46. A landmark case was *Griggs v. Duke Power Company*, which ruled that non-job related selection criteria which discriminated against blacks were invalid. On the use of litigation to attack different forms of credentialing, see White and Francis (1976).

47. Career educators have worried about both "over-education," which represents for them those students who continue to college without any clear goals and without knowing what the pay-offs and alternatives are; and "under-education," those individuals who drop out of school because they cannot see the relevance it has for their future lives.

48. See especially the "consensus" document on career education—*An Introduction to Career Education: A Policy Paper of the U.S. Office of Education* (1975). The goals in that paper are with a few exceptions uncontroversial, even for critics of career education like myself.

49. Grubb and Lazerson (1976); McGowan and Cohen (1977); Nash and Agne (1973); Fitzgerald (1973).

50. In discussing the controversy over career education, Acland (1977) has usefully reminded us that the distance between educational ideals and execution is a great one, and the debates based on the stated ideals of career educators are inappropriate because they have polarized the issues and positions involved and because both sides may miss the reality of career education. McGowan and Cohen (1977) describe one form the reality of CE might take: the replacement of vital concepts in which school and work are fused by a series of academic courses on the subject of jobs. Were this the result, debates over CE would be very different.

51. The most appropriate comparison literature to mention is that evaluating the effects of vocational education programs, since—as I have argued in Grubb and Lazerson (1975)—career education is essentially a renewal of the goals earlier embodied in vocational education. This literature has been reviewed in Reubens (1975), among other places; see also *Assessing Vocational Education Research and Development*, which concludes (in Appendix A) that the evaluation literature "yields little useful information for vocational educators."

52. In her literature survey, Reubens (1975) documents the high proportion of vocational education students who wind up in jobs other than those they were trained for.

53. See, for example, Mangum (1968).

54. See Kabak (1975).

55. See Karabel (1972).

56. For some of the recent sources of support for career education, see Grubb and Lazerson (1975). Among other groups, the National Commission on the Reform of Secondary Education (1973) the Panel on Youth of the President's Science Advisory Committee (1974), and the *Work in America* task force all proposed integrating work experiences into the school curriculum and affirmed vocationalism as a central goal of the educational system.

57. The vehicle is H.R. 7, the Elementary and Secondary Career Education Act of 1977, which proposes $275 million over four years.

58. For a recent review of the human capital literature, with the conclusion that this research has "degenerated"—that is, it has been increasingly forced to *ad hoc* and insubstantial explanations—see Blaug (1976). He concludes that models based on the information context of schooling—like some of the signaling and credentialing models—will come to dominate research.

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Chapter 9

Recurrent Education and Employment

David Stern

Introduction

This paper considers recurrent education as a means to help accomplish three purposes: (1) "democratization" of workplaces, (2) increasing the growth rate of labor productivity and thereby helping to reduce unemployment in the long run, and (3) mitigating disruptions due to the 1945-60 baby boom. With reference to the first two purposes, "recurrent education" will mean certain kinds of work-related education for adults, which may or may not require some interruption of full-time work.[1] The third purpose, in contrast, does imply periods of absence from work, but these could be used for avocational as well as vocational education, or even for pure recreation. The reasons for the changing definition will be explained in the discussion.

Recurrent Education and Workplace Democratization

During the 1970s it has been unfashionable in the U.S. to propose educational reform as an instrument for reforming social structure. Books like Jencks' *Inequality*[2] and Carnoy's and Levin's *The Limits of Educational Reform*[3] have discouraged belief in the effectiveness of educational policy as a means for reducing socioeconomic inequality. A National Institute of Education review admonishes us:

> "There is a limit to what education can do to cure the ills of society. And suggesting that its curative powers are stronger than we have learned they are will only add to the discrepancy between social expectations and what the educational system can actually deliver."[4]

In this time of unremitting realism, it has therefore become unusual to find expressions of hope and enthusiasm such as those contained in some recent discussions of recurrent education. These discussions are now suggesting that recurrent education can help promote far-reaching reforms in the economy and society, through democratization of workplaces.

Most, if not all, Western European countries have enacted laws extending the rights of employees to participate in managing the economic enterprises in which they work. Sweden has led the way with a series of laws, the latest of which went into effect in January 1977. It requires employers to negotiate with employees over all major decisions affecting the workplace, "such as shifts in production or organization, sale of the unit, or large-scale staff transfers."[5] "Management prerogative" is virtually eliminated. Employees will now share responsibility for decisions about investment, product design, marketing, production planning, personnel management, and so on. Sweden's Prime Minister Palme called the new law "the most radical and thorough equity reform in this country since universal suffrage."[6]

Effective use of the right to participate in managing the workplace, perhaps even more than the right to vote, requires a substantial amount of education. Anticipating this requirement, Sweden enacted several laws in the early 1970s which now enable all employees to take short periods of educational leave with pay, and with protection of job security.[7] Swedish labor unions are encouraging workers to use some of their paid leave to attend "study circles" (a traditional Swedish form of adult education) organized by the unions to teach the purposes and techniques of economic self-management.[8] Recurrent education of this kind gives people a chance to redefine their relationship to their own work, to take some additional responsibility for understanding and influencing what goes on in the organizations which employ them. Education becomes a process for workers to know more about what they are doing. By making possible some redistribution of authority in workplaces, this kind of education helps to accomplish a major transformation of the social structure. In Sweden, and in the Italian metalworkers' union program of "150 hours,"[9] education is believed to "have an important contribution to make to the further development of Western democratic societies," by enabling and motivating employees to engage in "more active and determined involvement in the problem areas of political and economic life."[10]

As a recent OECD monograph put it,

> "industrial democracy requires its participants to be well versed in certain fields; conversely, participation in the democratic process inevitably stimulates motivation for education as those involved perceive the need for this expertise."[11]

And the OECD report on recurrent education in Japan gives a similar account from the pragmatic viewpoint of employers:

> " . . . in the 1960s industrial circles made a quick response to the idea of lifelong education, from the point of view that they could not achieve higher productivity without heightening their employees' incentives to work spontaneously and effectively. In other words, the principle of

management has been transformed from the traditional type of "controlled management" to the new type of management that requires workers themselves to exercise control over their jobs. This recognizes the workers as responsible people with imagination and ingenuity. Such a change requires a new and appropriately planned training system, and in this sense a large firm becomes a school for continued learning."[12]

In the U.S., the possibility of education contributing to workplace democratization, and thus reducing "underemployment—the chronic underutilization of education, skills, and other human resources"—has been described by James O'Toole.[13] As leader of the 1972 HEW Task Force of Work in America,[14] O'Toole helped introduce the idea of job redesign and "humanization" of work into American public policy discussions. He then wrote in 1975,

"Experiments in Europe and America in which learning has become the goal of blue-collar jobs have shown remarkable success in meeting these problems of underemployment. Management experts are starting to build a body of evidence indicating that individual growth and organizational growth can occur simultaneously and compatibly. In a General Foods plant in Topeka, Kansas, for example, all workers have the opportunity to learn all the jobs in the plant and are compensated for each new job they learn. Almost all workers, including those who have only minimal levels of education, know how to repair the plant's complex, transistorized, computer-like monitor with thousands of circuits and switches. It was found that learning is the key to job satisfaction in this plant. Even more important, this desire to learn has spread to nonwork activities. General Foods offers to refund the tuition for any course any of its employees pursues in his or her spare time. Three times more workers in the Topeka plant take advantage of this offer than in all other General Foods plants. It appears that learning on the job has whetted the workers' appetite for more education. It has overcome the sense of educational inadequacy that afflicts so many blue-collar workers. Productivity is 40 percent higher than in comparable but traditionally designed plants, and there is hope that other employers will follow suit."[15]

Another example of American workers developing an appetite for education after beginning to exercise more control in the workplace is the Harman International plant in Bolivar, Tennessee. In a carefully planned demonstration involving the UAW, top management from the company, and a team of social psychologists, workers in the plant have been given opportunities to decide how they wanted to restructure their jobs. One of the results of the changes they have made is that the work is getting done more quickly, and the employees are free to go home early or stay at work and use the time as they choose. One thing they have chosen to do with this new free time is start a school in the plant, with instruction in both work-related and avocational subjects.[16]

On the basis of such evidence, O'Toole asserts that "workers want their learning to be work-based," and this:

"might be seen as an opportunity for the true integration of work and learning. Schools, now in a desperate search for 'new clientele,' might take the initiative and approach employers with programs that would grant credit for learning what occurred on the job, in class, or wherever. Using flexible performance certification or competency-based systems, the purpose would be to facilitate the continuous upgrading of workers' credentials."[17]

While it may be true that employees in general would be more satisfied and productive if they could learn more on the job, it seems to take something like the Topeka or Bolivar experiments, or the Swedish workplace democratization movement, to make large numbers of workers want to learn more about their work. Like the tuition reimbursement available to all General Foods employees, opportunities for work-related recurrent education already exist in many companies and many countries, but they are generally under-utilized. For example, in 1967 the Conference Board surveyed 200 U.S. and Canadian companies which had some kind of tuition-aid plan for employees. In 155 companies cooperating with the survey, there were more than 3.6 million employees eligible to receive some reimbursement for taking job-related courses, but the typical company's participation rate was 4.4 percent, in spite of the fact that 53 companies even allowed some courses to be taken on company time. In most companies, the majority of workers who did participate were male, salaried, non-union employees.[18]

The plans surveyed by the Conference Board were all-management-initiated plans. However, in at least two cases where major unions (the UAW and the IUE) have bargained for and won this kind of educational fringe benefit, the rate of participation has been similarly small.[19] Even in France, where paid leave for work-related education is a right guaranteed by national law since 1971, one of the top officials responsible for the program in 1973 listed first among the "most difficult problems" still remaining the fact that "Many workers are not yet inspired with the spirit of continuing education . . . ."[20] However strongly workers might desire some paid leave, many choose not to take it when it has to be spent learning things related to work. A program of paid leave for vocational education for young workers in Belgium has been similarly undersubscribed.[21]

Existing opportunities for work-related recurrent education tend to be used mainly by workers who already have relatively high status. "Study after study has shown a direct correlation between the length of initial education and the likelihood of participation in adult education."[22] In France, for example, employees in professional and managerial positions are more likely to take advantage of paid educational leave than workers in more routine subordinate jobs.[23] But does this mean that initial education causes people to want to keep learning, or that initial education simply selects people who are most willing and able to learn, or that initial education helps determine access to the kind of job about which there is something interesting to be learned? Von Moltke and Schneevoigt can only conclude that "Motivation for educational leave . . . appears to derive at least as strongly from occupational status as from prior education."[24]

But exactly what is it about "occupational status" which inclines workers who have more of it to want to keep learning? An answer is suggested by one study of continuing education among workers who all had very similar occupational status. This was a 14-year study of 110 engineers in three organizations. The study found an association between participation in continuing education and the degree to which an individual's job was professionally challenging.[25]

This finding is consistent with a general view that motivation for learning at work is both intrinsic and extrinsic. Higher-status workers are more likely to participate in work-related recurrent education both because they like the feeling of knowing about their field of work and because they receive material rewards for applying their knowledge. In a stratified system of work roles, jobs at higher levels tend to be more interesting and challenging (which does not always mean more "satisfying"), as well as better paid. More initial education helps to provide (but does not guarantee) access to these good jobs. Individuals who do find challenging and well-paid work early in their careers apparently are more likely to want recurrent education later on, both to keep growing intellectually and to keep advancing up the job ladder.

The implication is that work-based recurrent education will not appeal to *large numbers* of workers unless and until they can see either more personal satisfaction or more material benefits—or both—coming to them as a result of learning more about their jobs. Schools seeking new clients will not have much success in marketing work-based programs until and unless more workplaces are restructured to create either intrinsic or extrinsic incentives—or both—for employees to keep learning. Schools can create *opportunities* for workers to learn, but they cannot bring about the changes in workplaces necessary to *motivate* learning. As Von Moltke and Schneevoigt remind us,

> "Education alone is a weak agent for social change, and an impotent one in the face of countervailing tendencies in the labor market and in social policy. As part of a far-reaching, comprehensive policy of change in working life and social conditions, education may provide an instrument to mitigate certain side effects of a technological-industrial society and to assist in moving toward desired change; but the impetus for change comes not from education but from the overall complex process of developing a public consciousness of the pressing issues that need to be tackled. This is essentially the way in which educational leave is to be used in Sweden. Educational leave in this sense should be viewed . . . as part of a number of interlocking social and economic policies, the crux of which must be to change the conditions of work—the leading variable in adult life.
>
> "Educational leave in this sense is rare in Europe . . . ."[26] It is non-existent in America.

The question is: Where will the impetus for workplace democratization come from, and will it be strong and pervasive enough to warrant a whole new system of recurrent education to accommodate it?

Prospects for Workplace Democratization

In Europe, workplace democratization and paid educational leave have both been pushed mainly by labor unions and the political parties with which they are affiliated. In the U.S., although some unions have won tuition reimbursement and similar educational fringe benefits in collective bargaining, the AFL-CIO has not yet begun to lobby for a national system of paid educational leave. Clark Kerr notes that a national system of health insurance is much higher on American labor's political priority list.[27]

The low priority of paid educational leave for American labor unions is consistent with the low priority they give to workplace democratization. Unlike their European counterparts, American labor unions have not been pushing either for national laws to increase workers' participation in management or for contracts with individual employers which would do the same.[28]

There is real difficulty in achieving workplace democratization through collective bargaining, because it is hard to know exactly how much additional material output or intangible psychic benefit for employees would result from a given change in the organization of the workplace. Therefore, although both sides at the bargaining table could expect to gain something, it is not in the interest of either side to press a formal demand, because the rules of collective bargaining dictate that if one side makes a demand, the other side can expect to ask for some concession—but neither side knows how big a concession would be warranted. The process of collective bargaining therefore inhibits the search for reforms from which the gains for each side are probably positive, but uncertain.[29]

Some individual unions, most notably the UAW, have begun to develop approaches to workplace democratization through discussions with employers *outside* collective bargaining.[30] These efforts may produce important changes within certain industries. But the American labor movement as a whole is far from ready to make any coordinated effort to bring about workplace democratization through national law, as in Europe. In the near future, as Henry Levin says, American unions are "likely to be a rather conservative force. . . . But, over the long run they may have to yield to other forces such as grass-root pressures for change."[31]

What is the likelihood of grass-root pressure sufficient to overcome the inertia of American labor unions? Kenneth Walker, in a recent paper on "The Implications of Industrial Democracy for Educational Planning," observes that the first reason for the trend toward industrial democracy has been

> "pressure (which) has built up in many countries for the granting of equal rights and maximum opportunities to all categories of workers. The principal categories affected have been: women, ethnic minorities, handicapped workers, less educated workers, and older workers."[32]

Walker claims,

> "The basic political trend which has contributed to increasing interest in industrial democracy is simply the gradual working out of the impli-

cations of the *democratic ideals* that took root in the 18th and 19th Centuries."[33]

However, as Henry Levin argues in his paper on "Workplace Democracy and Educational Planning," democratic and egalitarian ideals do not prevent

> "the continuing functioning of the societies to produce unspeakable poverty and squalor on the one hand and unimaginable wealth on the other; to sanction a ruling class and a disenfranchised one; and to sponsor political, economic, and physical repression . . . "[34]

Although Levin and Walker may disagree on the effectiveness of egalitarian, democratic ideals in bringing about democratization of workplaces, they do agree that for purely economic reasons more and more employers are likely to institute reforms of the kind Levin calls "micro-political." These

> "represent alterations in the internal decision making of the work enterprise that increase the participation of workers in matters which affect the nature and organization of their work. . . . In these cases some traditional managerial prerogatives are relinquished or shared with workers. . . . Thus, the typical micro-political modifications of the work enterprise will increase the participation of workers in determining such matters as production schedules, training regimen, work assignments, and work methods. However, such changes will not affect the overall control of the organization as reflected in decisions on the choice of products or services to be produced, pricing policies, investment plans, distribution of profits, or overall organizational structure . . . . "[35]

As examples of micro-technical reforms which more employers can be expected to emulate, Levin cites the General Foods plant in Topeka, the Harman International plant in Bolivar, Tennessee, the Scanlon plant, and the Volvo plant in Kalmar, Sweden.

Levin and Walker also agree on two main reasons why such reforms are likely to proliferate. First of all, Levin asserts, "The record for improving productivity through these forms of participation is rather substantial."[36] Similarly, but more cautiously, Walker writes,

> "Although participation may not provide an invariably satisfactory solution to the practical problems of production . . . sufficient success has been achieved so far to indicate that managements will continue to seek to solve these problems by various forms of workers' participation in management."[37]

The second reason on which Walker and Levin agree is that the growing numbers of young, highly educated workers will *demand* more participation. Walker cites studies showing more educated people are more likely to believe in the efficacy of the political process, are more interested in "political affairs and processes," and are more likely to participate.[38]

Levin asserts that,

> "the frustrations of the young and overly-educated worker are due in large measure to the lack of autonomy and low skill requirements of the jobs that will be available. . . . The implication is that at the lower levels of the firm productivity will be maintained by placing an increasing emphasis on participation in the decisions that affect the work situation."[39]

Both of these reasons are debatable, however. In 1974 I carried out a survey among a sample of 900 unionized municipal employees, specifically to test whether age and educational attainment were associated with expressed desire for different kinds of changes in the job. From conversations with accountants, nurse's aides, secretaries, and social service supervisors, I developed a list of specific hypothetical changes in their jobs. These changes included a two-hour reduction in the working week with no change in pay, two different proposals for more flexible work hours, a proposal for two hours of paid educational leave each week, a proposal for tuition reimbursement, and three proposals which would have given employees more opportunity to participate in managing their own workplaces. I expected younger and more highly educated workers in each job category to express greater desire for more participation. They did not.[40] Demands for the various hypothesized changes were, as expected, associated with expressed dissatisfaction over various aspects of the job, but these dissatisfactions appeared to be idiosyncratic, not consistently associated with age, education, sex, race, or family status. Perhaps in other samples some clear associations would emerge, but such associations are not ubiquitous in the population. Other studies have found that expressed satisfaction with the job as a whole is more strongly associated with autonomy and "challenge" among more highly educated workers,[41] and that people also express more dissatisfaction with jobs where they feel their education is not useful[42] (this is also true in the municipal employees sample). But my survey indicates that these attitudes and preferences of highly educated employees do not necessarily result in stronger demands for participation in managing the workplace. Somehow, as Richard Hackman says, people adapt.

Hackman is a researcher and practicing management consultant who has done a great deal of theoretical and applied work on the design of jobs. His recent paper on "The Design of Work in the 1980s"[43] begins with two conclusions from research and practice in the field. The first conclusion, compatible with Levin and Walker, is that "Many individuals are presently under-utilized and under-challenged at work." The second is, "People are much more adaptable than we often assume." This is somewhat at odds with the views of Levin and Walker. In a discussion perhaps modelled on Amory Lovins' famous essay on energy policy, Hackman delineates

> "two quite different routes that can be taken as choices are made about how to design and manage work in the next decade and beyond. One route, which derives from the conclusion that many people are underutilized by the work they do, leads to increases in the level of challenge that is built into jobs, and in the degree of self-control job-holders have

in managing their own work. In effect, we would attempt to change jobs to make them better fits for the people who do them.

The other route derives from the second conclusion: namely, that people gradually adapt and adjust to almost any work situation, even one that initially seems to greatly under-utilize their talents. This route leads to greater control of work procedures and closer monitoring of work outcomes by management to obtain increases in the productive efficiency of the workplace. Technological and motivational devices would be used to attempt to change the behavior of people to fit the demands of well-engineered jobs. The expectation is that in a carefully designed work environment employees gradually will adjust to having little personal control of their work, and the efficiencies gained by using sophisticated managerial controls of work and workers will more than compensate for any temporary dissatisfactions the people experience."

This is a stark choice. The first route leads to more learning and participation in workplaces; Hackman cites the General Foods plant in Topeka as a well-known example. The second route leads to "behavior modification" (already widely practiced in schools and starting to be used in business), with employees' behavior minutely monitored by on-line information microprocessors. With the growing sophistication of biofeedback machines which can control brainwave patterns, the possibilities are mind-boggling. Hackman thinks route two will lead to more "craziness" among employees because they will be rewarded externally for activities about which they have negative feelings.

Hackman prefers route one, but he thinks we are already proceeding down route two. Unlike Levin and Walker, he believes we will continue on the route of fitting people to jobs, because this is what managers and workers are accustomed to, and the resistance to change is very strong. He notes the reaction against the Topeka experiment on the part of managers in other parts of the General Foods organization.[44] Even if we had more systematic knowledge about how to fit jobs to people, Hackman believes, implementation of this knowledge will be strongly opposed by many experienced workers who have adapted to their present jobs, and by managers who believe "organizations are supposed to be run from the top down, not from the bottom up."

In contemplating the possible future of work, it is important to keep in mind that national surveys of employees in the U.S. consistently find that "interesting" work and opportunities to develop one's "own special abilities" are among the two or three most important things people say they look for in a job.[45] The demand for workplace democratization is there, but, like the demand for work-based recurrent education, it is a latent demand. Whether this demand becomes effective depends in part on the bargaining power of employees relative to employers. Fitting jobs to people makes more sense economically when people are in short supply and employers are required to work with the people they've got, rather than firing people who don't quite fit in. The prospects for workplace democratization, and the need to plan for corresponding programs of recurrent education, will therefore depend, at least in the U.S., on the state of the labor market, as measured by the rate of unemployment.

Recurrent Education, Full Employment, and Productivity

Full employment would increase the likelihood of workplace democratization; it is also a separate and important objective in itself. Some countries, notably France and Germany, have used recurrent education as a means to help achieve and maintain full employment. In France, the government has joined forces with employers and labor unions to provide various programs to retrain workers. The enabling legislation began in 1959, was modified during the 1960s, and culminated in the 1971 Law for the Organization of Further Vocational Training in the Framework of Permanent Education.[46] In Germany, the 1969 Employment Promotion Act empowered the agency which administers unemployment insurance to operate training programs as well.[47] In both countries these retraining programs are open to the unemployed—like the various manpower training programs in the U.S.—and also to employed workers. Employed workers who have to miss work for some period in order to participate in training or retraining programs receive stipends to offset foregone earnings, and their job security is protected.

These programs are intended to help maintain full employment mainly by hastening the movement of people into productive sectors where there are shortages of trained workers. As such, these programs complement efforts to predict where jobs vacancies will occur and to inform workers about them.[48] Apparently there have not yet been any attempts to measure the actual impact of these training and retraining programs on the overall unemployment rate. Indeed, it would be very difficult to separate these effects empirically from the effects of other labor markets programs and other macroeconomic policies generally.

However, a recent paper by Baily and Tobin provides a framework for analyzing the effect of recurrent education programs on the rate of unemployment in theory.[49] The problem, in the long run, is to reduce the rate of unemployment without causing inflation to accelerate. Accelerating inflation in the labor market means that wages keep increasing faster than expected. The expected rate of wage inflation depends on historical experience and is embodied in institutional arrangements such as cost-of-living clauses in labor contracts. Given the expected rate of wage inflation, Baily and Tobin postulate (on the basis of some evidence) that wage inflation will tend to accelerate if the unemployment rate decreases and/or if the job vacancy rate increases. Given the number of jobs which exist at a particular time, Baily and Tobin show that a program of recurrent education or any other program will permit a reduction in unemployment without causing wage inflation to accelerate if all of the following three conditions are met: (1) The number of job vacancies decreases. (2) The number of people looking for jobs (i.e., formally in the labor force) decreases. (3) The program does not cause employers to raise wages for other reasons.

Thus the Baily-Tobin analysis implies that recurrent education can enhance the effectiveness of direct job-creation or other policies designed to reduce unemployment in the long run, if recurrent education is focused mainly on the unemployed or soon-to-be-employed, if it does not attract people into the labor force who otherwise would not seek paid employment, and if the stipends are not so

large as to make anyone choose to participate in recurrent education rather than take (or keep) a paid job. A recurrent education program meeting these criteria would look more like an American-style training program for the unemployed than a European-style program of paid leave for employed workers. (All this assumes that people are not counted as employed while they are participating in recurrent education.)

Baily and Tobin also extend their model to analyze the problem of segmented labor markets. They conclude that it is possible to reduce structural unemployment—for example, in depressed regions—but not without causing some additional unemployment among other groups in the labor force. They believe such redistribution of unemployment may be justified in some instances. Recurrent education programs have in fact been used to help implement such policies—for example, in the coal-mining Ruhr Valley of Germany.[50]

The long-run effect of recurrent education for *employed* workers on the rate of unemployment depends on how it affects the growth of average labor productivity. Increasing labor productivity results in higher real wages and more employment in the economy as a whole.[51]

The empirical question, then, is whether increased investment in recurrent education for employed workers can in fact lead to a sustained increase in the growth rate of labor productivity. At present this question cannot be answered. There are reports of a great deal of in-service training going on in industry already. For example, in 1975 the Conference Board, with assistance from the Carnegie Corporation, surveyed 600 of the largest companies in the U.S. and found that they spent roughly $2 billion on formal education for employees in the preceding year. About one out of every eight employees participated in such training.[52]

But does this massive expenditure on training by industry pay off? Is productivity higher than it would be without such formal training efforts? Do employees really learn what they are supposed to learn, and do they put it to use? The Conference Board found "Most companies, like most schools and colleges, have found that evaluation is difficult. . . ."[53] Similarly, a Business Week report on training programs for managers observes,

> "When companies begin training so many managers, either in-house or outside, evaluating the alternative programs—and the results—become increasingly difficult."[54]

A thorough investigation of the effectiveness of on-the-job training programs would have to consider not only whatever formal instruction is provided, but also the informal processes of teaching and learning that go on continuously in work organizations. The informal processes may enhance or completely undermine the benefits of formal instruction. The formal and informal processes of learning on the job could be considered together under the rubric of "job-learning." The question of how to design effective on-the-job training then becomes one in a larger set of questions about how characteristics of organizations affect job-learning. Other questions in the set might include:

1. How to measure the amount of job-learning that takes place in actual work settings? To what extent could the written and practical tests presently administered in schools and training programs be adapted to measure learning in particular jobs?

2. Among work organizations of the same kind, what is the relationship between rate of job-learning and productivity over time? For this comparison, work organizations could be classified by the following kinds of characteristics:

   Type of product or service.
   Ownership: is the enterprise owned by workers, managers, the government, or absentee stockholders; i.e., how are profits divided?
   Size.
   Rate of expansion.
   Existence of union(s).
   Average number of subordinates per supervisor.
   Average ratio of immediate supervisor's to subordinate's salary.
   Number of supervising levels.
   Proportion of positions at each level filled from inside the organization.
   Degree of participation by employees in decisions regarding product design, marketing, investment of profits, pay structure, pace of production, etc.

3. Which characteristics of organizations are most strongly associated with job-learning?

4. How is job-learning distributed among employees by age, sex, and level in the hierarchy?

5. What are the mechanisms for job-learning? What is the relative importance of formal on-the-job training compared to informal exchanges of information and guidance?

6. How much does job-learning really interfere with job-doing? Economists have tended to assume that any increase in "general human capital formation" during work time must mean an equal reduction of output and wages. Is this true, or are learning and doing sometimes both results of the same activity?[55]

7. To the extent that learning applies only to a particular job or organization, how is the payoff divided between employer and employee? This kind of specific training creates a bilateral monopoly situation, where both employer and employee stand to gain more by continuing the contract, because the employee would have to take less pay elsewhere and the employer would have to train someone else. But if either party exploits his position, the other party would be better

off terminating the contract. What determines the actual outcome?[56]

8. Why should one employee spend time teaching another employee? Do any organizations provide formal or informal incentives for job-*teaching*? Seniority rules may remove some *dis*incentives to teaching other workers what one has learned from experience, but are there any actual positive incentives?[57]

9. What are the attitudes of employees toward job-learning? Surveys have found that some workers say they value the opportunity to learn and develop skills at work, but it is not clear whether job-learning is always considered entirely instrumental—a means toward promotion—or whether it is an end in itself. What characteristics of workers and or work organizations influence these attitudes?[58]

10. In light of all the above, what is the most productive way to organize incentives and opportunities for job-learning in different kinds of enterprises?

Answers to questions such as these would give greater empirical content to the economic concepts of depreciation and obsolescence of knowledge. In theory, depreciation is the decrease in knowledge due to individual aging, i.e., forgetting. Obsolescence is the decline in value of old knowledge as new knowledge is created. Sherwin Rosen has estimated that the combined rate of depreciation and obsolescence of human capital is at least 10 percent per year for college graduates and 15 percent for high school graduates.[59] Similarly, Hanushek and Quigley estimate that net investment in human capital becomes negative within six to eight years after workers leave school.[60] Such estimates, however, have had to be based on highly refined analysis of observed earnings, because direct measures of job-learning have been lacking. Reliance on observed earnings to measure job-learning (or unlearning) requires two very strong assumptions: that earnings are an accurate measure of productivity, and that employers know how to put employees' knowledge to productive use. If we accepted these assumptions, there would be no need to study job-learning in actual work organizations, because there would be no way to improve on present practice. However, the uncertainty expressed by employers about how to evaluate training programs indicates that the assumptions are not warranted in reality. There is indeed room for improving the transmission and utilization of knowledge in work organizations.

Even if work enterprises were optimally organized for job-learning, the scope of job-learning would still be too narrow, according to standard economic arguments. No rational employer will deliberately pay for employees to acquire any "general" training, which might have value if the employees went to work for other employers. Instead, the rational employer only pays for "specific" training, which is useful only if the employee stays employed in that firm. To the extent that this is true, it means there is little or no investment in general training of employed workers—unless they somehow pay for it themselves. Employers are

even less likely to pay for employees to acquire what Von Moltke and Schneewoigt call "nonmarketable qualifications"–

> "increased awareness and understanding of the present conditioning socioeconomic factors and the capacity to act on them. . . "[61]

These qualifications will be relevant to productivity only if the organization of workplaces becomes more democratic. With or without workplace democratization, the potential contribution of recurrent education to long-run increases in the growth of labor productivity, and to long-run reduction of unemployment, will be fully realized only when employed workers have the opportunity to make continuing investment in their own general vocational skills.

Recurrent Education and the Baby Boom

Two purposes for recurrent education have been described so far in this paper: (1) prepare people to participate in democratized workplaces, and (2) help reduce the long-run unemployment rate by increasing the growth rate of labor productivity. These two purposes are related, since reducing unemployment would increase the likelihood of workplace democratization. Reducing unemployment is also, of course, desirable for many other reasons.

Discussions of recurrent education in the U.S. in the early 1970s, however, were concerned with quite a different problem: the sudden appearance of "overeducation." Virginia Smith observed at a 1973 NIE conference that " . . . by the 1970s . . . (policy makers) were questioning whether traditionally organized collegiate programs were necessarily the best vehicles for educating the nation."[62] Frank Newman, principal author of the 1971 HEW *Report on Higher Education*, bluntly asserted:

> Since the educational system can no longer insure jobs, it should return to its rational function of instilling a desire to learn rather than offering a ritualistic entry into the job market.[63]

What troubled the policy-makers and analysts was new evidence of an unprecedented reduction in the economic returns to higher education, as documented, for example, by Richard Freeman:

> In 1968, (white) college men ages 25- to 29-years old earned 17% more than high school men of the same age; in 1973, just 6% more. By contrast, relative incomes at other ages show only slight changes.[64]

Freeman referred to this as a twisting of the cross-sectional age-earnings profile. It meant that men born after World War II, who attended college in higher proportion than any cohort before them, were receiving a smaller monetary payoff from college than previous cohorts had obtained, at least early in their careers. Since the earnings differential associated with a college degree had been stable for about three decades prior to 1970, recent college graduates were earning less money than they presumably expected. In this sense, there appeared to be a surplus of young college graduates.

Along with journalistic reports of unemployment among college graduates and Ph.D.s, this kind of evidence prompted some people to suggest recurrent education as a solution: if there are too many college graduates now, more young people should be encouraged to go to work instead of college now, and then maybe return to college later if they want. Henry Levin has pointed out the irrationality of such proposals, from the viewpoint of a young person: Even if the economic payoff from college is less than it used to be, it is still positive, and for the usual economic reasons it still makes more sense to invest in education at a young age rather than wait until later.[65] Reducing "overeducation" is therefore not a logical rationale for recurrent education. While there may well be systematic tendencies for "overeducation" to occur, recurrent education is not the remedy.

However, recurrent education could be a remedy for the problem of *too many people of the same age* all looking for jobs at the same time. Recall that what Freeman found was a reduction in the economic payoff to college mainly among *young* men in 1973. These were the vanguard of the baby boom born between 1945 and 1960 (birth rates peaked in the U.S. in 1957). Children of the baby boom attended college in larger proportions than any previous cohort, and it is therefore not surprising that the young college graduates did not command such relatively high starting salaries as in previous years.

But economic superfluity among members of the baby-boom cohort has not been confined to the top of the educational scale. Relative to male high school graduates between the ages of 16 and 24 who were not enrolled in college, male dropouts from high school in this same age group have had an increasingly hard time finding work since 1960. In that year the unemployment rate among the young male high school graduates was 15.2 percent; among the dropouts it was 18.2 percent. In 1974, when the graduates were again experiencing about the same rate of unemployment, 15.3 percent, the dropouts had 29.2 percent.[66] Although unemployment rates are not conclusive in the absence of data on wages, they do suggest that the less educated members of the baby-boom cohort were also having a relatively harder time in the labor market.

For the cohort as a whole, some idea of how cohort size has been affecting unemployment rates can be obtained by comparing the years 1956, 1965, and 1974. In all three years, the unemployment rate among men of age 35 to 44 was 2.6 percent. Therefore, comparing these three years keeps overall labor market conditions constant, at least as conditions are reflected in the experience of prime-age males. (However, the 35 to 44 year old cohort was unusually small in 1974.) Among males of age 16 to 19, the rate of unemployment in 1956, before any boom babies were seeking regular jobs, was 11.05 percent. In 1965, when the category of males aged 16 to 19 consisted of boys born between 1946 and 1949, their unemployment rate was 14.10 percent. And in 1974, the unemployment rate for males born between 1955 and 1958 was 15.53 percent.[67] The rise in relative unemployment rates of teenage males was accompanied by a *decline* in their earnings relative to prime-age males.[68] Because of their large numbers, teenage males born in the baby boom have been reporting relatively less success in finding jobs, even at lower relative wages.

In the future, unemployment and underemployment will continue to be problems for people born between 1945 and 1960. At any given rate of economic

growth, members of this cohort will find fewer opportunities for employment and advancement at any age than members of earlier or later cohorts. Between 1980 and 2010, as the baby boom reaches mid-career, a relatively large number of them will be underemployed. *Business Week* predicts the following developments in the job market for managers:

> "In the 1980s an extraordinary number of ambitious young men and women will crowd onto the first step of the management ladder, but the number of seasoned executives to supervise them will not grow correspondingly. In the 1990s the situation will reverse. There will be more experienced, fully qualified junior managers than there are spots in senior management to absorb them."[69]

For the baby-boom cohort as a whole, *Business Week* foresees "heightened competition as well as relatively depressed incomes and advancement opportunities during most of their working lives."[70]

After the year 2010, however, the problem will not be too many people of the same age all looking for jobs, but just the opposite. As the baby boom cohort reaches age 65, between the year 2010 and 2025, there will be a dramatic increase in the number of people *out* of the labor force. Most of them will expect to collect pensions, and the strain on both public and private pension plans will become enormous. Through the year 2005, the projected ratio of workers to Social Security pensioners will exceed three to one. But then it will decline sharply and by 2030 will barely exceed two to one.[71] If all these potential retirees actually do try to collect the benefits to which they will be entitled, there will be no way to avoid a rather sudden and substantial rise in payroll taxes or other levies on workers. But will those future workers be willing to support the baby boom cohort in retirement? Should those children and grandchidren of the baby boom cohort have to pay that cost?

The struggle over Social Security in 1977 and 1978 is just a taste of things to come. In 1977 Congress increased the payroll tax, and scheduled further increases to cover the projected cost of Social Security through the year 2000—before the baby boom begins to retire. Yet even these tax increases, which will *not* cover the deficits after the year 2010,[72] are being resisted, and Congress is now (April, 1978) considering a move to roll them back. These recent events indicate how bitter the struggle to cover the long-term cost of Social Security is likely to be, when Congress can finally postpone it no longer.

Many private pension plans are also facing the same kind of long-term problem as Social Security. The present value of vested pension benefits in many major companies already exceeds the value of assets held by the pension plans, often by staggering amounts. In the General Motors pension plan, for example, the unfunded liability for vested benefits was $3 billion, or 21 percent of General Motors' net worth! At Chrysler the unfunded liability was 39 percent of the company's net worth. At Bethlehem Steel it was 48 percent, and at American Motors 59 percent![73] This means the value of pension benefits American Motors would have owed to its employees if they had all retired at the end of 1976 would have exceeded the assets of the pension fund by more than half the value of the company itself. When these unfunded pension liabilities actually become due, they

will have to be paid out of company revenues. And as the number of retirees begins to grow faster than the number of currently employed workers, the same cruel questions of intergenerational responsibility will arise in the private pension plans as in Social Security.

The continuing strains and disruptions caused by the baby boom suggest another purpose for recurrent education: enable individuals to leave the labor force when conditions there are unfavorable to them, and to remain in the labor force when there is a shortage of active workers. This purpose could actually be accomplished by a very general kind of "sabbatical," which in effect allowed individuals to take some of their "retirement" before rather than after the normal retirement age. Unlike the two other purposes of recurrent education suggested here—preparing for workplace democratization and raising labor productivity—alleviating demographic disruptions implies more about the way recurrent education should be financed than about what the educational content of the program should be. The essential thing is to allow individuals to have some "retirement" during their normal working lives, and then make up the time by working after the normal retirement age. For this purpose it would not matter if the mid-career "retirement" were spent on vocational education, or on education at all.

One way to enable individuals to redistribute some amount of working time over their life-cycles would be to permit them to borrow against their own Social Security or private pensions. More specifically, every individual might be allowed to receive a stipend for some period of X months at any age, where the amount of the stipend would equal the present value, at that age, of the first X months of pension to which the individual would have been entitled. In taking the stipend before the normal retirement age, the individual would be required to relinquish his rights to the first X months of pension. Alternatively, the worker might relinquish some portion of her pension over the entire period of retirement, but retain the right to retire at the normal age. This kind of option could be created for Social Security members by simply amending the Social Security Act. For members of private pension plans, there would probably need to be an amendment to the federal Employee Retirement and Income Security Act.

A numerical example might help to illustrate the idea and to suggest modifications. The average monthly benefit for an individual retiring in the period from 2010 to 2030 would be, conservatively, $500 in 1975 dollars. If an average worker wanted to use this money at age 45 instead of age 65, he or she could collect the discounted present value, which would be about $225 a month in 1975 dollars. This uses a discount rate of 4 percent, which includes the 3¼ percent real interest rate used in projecting Social Security trust funds,[74] plus ¾ percent to allow for annual mortality.[75] This computation is intended to make it actuarially equivalent whether the stipend is taken earlier or later.

The amount of the future retirement benefit to which a worker would be entitled could be imputed for a given year in mid-career in many ways. One simple way would be to base the estimate on the number of years a worker has paid into Social Security already, and the average annual covered earnings *to date*. These two factors would both tend to award higher mid-career stipends to less educated workers, who usually start working full-time at a younger age, and whose annual

earnings also usually reach a peak earlier in their careers. Favoring less educated workers in this way would to some extent offset the usual tendency for more highly educated workers to take greater advantage of recurrent education opportunities.

Mid-career stipends computed in this manner in a given year would be comparable to actual retirement benefits being paid to workers who retire in that year. To support a family, a worker who took a mid-career stipend would probably have to supplement the stipend from savings, spouse's earnings, or other sources. Nevertheless, the stipends would help. And the figure of $225 a month given above is intended to be a conservative estimate, representing what would be available to workers in mid-career from Social Security alone. Adding private pension benefits could make a substantial difference. But even the amounts based on Social Security alone would enable more workers to survive a period of zero earnings without going that much deeper into debt—assuming banks and other private lenders would even consider making loans at reasonable interest rates to pay expenses during a voluntary leave from work, which may be unlikely.

The proposal described here is akin to some other recent suggestions for financing paid educational leave or more "flexible life scheduling" generally.[76] These proposals have not yet attracted prime-time public attention, however. One reason may be that the mainstream educational organizations, which dominate the national politics of education, would prefer to talk about money for institutions instead of entitlements for students. Apart from such tactical considerations, existing proposals for continuing education entitlements would create major new public fiscal liabilities, although in some proposals these would be offset by student repayments. In addition, current proposals would often require a whole new administrative structure to manage the money. In contrast, the distinguishing feature and chief political advantage of the pension-financed sabbatical proposal here is that it would require no new taxes, and no additional contributions by employers or employees.

Summary and Conclusions

Three purposes for an expanded system of recurrent education in the U.S. have been considered here: (1) Help bring about democratization of workplaces. (2) Help increase the rate of growth of labor productivity, and thereby help reduce the overall rate of unemployment in the long run. (3) Mitigate disruptions caused by the 1945-1960 baby boom, specifically by enabling some individuals to spend some time working after instead of before the usual retirement age. These three purposes are related but distinct, and different systems of recurrent education could be designed to achieve any one, two, or all three of them. The decision where to invest research and development resources will depend on the perceived importance of the three purposes, and the perceived feasibility of means. To spend resources on recurrent education for workplace democracy might seem politically risky in the cautious atmosphere of the 1970s, but the payoff would be substantial if the movement for workplace democratization gathers momentum here as it has in Europe. There would also be some risk in supporting research on

recurrent education for higher productivity, although here the risk is not that [redacted] purpose would be considered somehow illegitimate—rather, as with any program of basic research, there is simply no guarantee that investigating the processes of "job-learning" would achieve the desired practical payoff. In contrast, some careful analysis, and possibly an actual field test, of pension-funded sabbaticals is likely to be politically popular, but might also require some cooperation among several federal departments, since sabbaticals have important implications beyond recurrent education.

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Chapter 10

Research Priorities in Education Finance and Organization: A Concluding Note

Charles Benson and Michael Kirst

America's system of financing elementary/secondary education has become an area of intense governmental concern and activity. The issues have changed dramatically since the post-World War II concern over how to construct enough schools and hire teachers. In the decade of the 1960s, spending on elementary and secondary education increased at an annual rate of 10 percent and enrollment grew by 30 percent. By 1970 this prodigious expansion had begun to slow and concern shifted to the equity and adequacy of the fiscal base. The school system in the United States relies overwhelmingly on state and local education revenues. As enrollments peaked and in many cases turned down, local property taxpayers began to resist tax increases. Inflation in the early 1970s accelerated at the very time public support for the schools was slipping. There was increasing talk of a "fiscal crunch." But most important for this volume, the late 1960s was a period of reflection and action concerning the equity of this gigantic public finance enterprise.

Public education is by far the largest activity of state and local government. Expenditures for current operations consume over $75 billion which is nearly a third of all state and local expenditures. There are over 16,000 local school systems with two and a half million teachers working with 45 million pupils in 87,000 local schools.

A major fiscal problem results from the nature of school finance support—about 50 percent from local property taxes, 44 percent from state aid, and six percent from federal funds. The local share was even larger in 1969 when school finance equity surfaced as a major public policy issue after years of being in the shadows. Essentially, court rulings galvanized public action in several states. The concern spread to other states that were not facing court challenges. At the core of the problem is the grossly unequal distribution of local property wealth. School districts with high property valuations levy low tax rates but are still able to

finance an expensive educational program. The result is high taxes and under-financed schools for people living in poor areas and the reverse for those areas with high assessed value per pupil.

Arrangements for financing public education have been under intense scrutiny since the mid 1960s. Several overviews of the research on the economics and politics of public school finance have been prepared.[1] Under the impetus of legal and political challenges and often with the advice of the new breed of school finance expert, about 15 states have already enacted significant tax and school finance reforms. By the time all the states under court orders fall into line, over 50 percent of the public school students (and taxpayers) in the United States will have been significantly affected. An alliance of lawyers, researchers, political activists, interest groups, and foundations has brought about a non-incremental change in American public policy.[2] This change has been accompanied by an impressive expansion of research. School finance problems are inherently complex, involving intergovernmental transfers of funds, intricate aid formulas, and multiple systems of taxation. Economists, political scientists, lawyers, and practitioners have all been funded to probe various aspects of school finance. State legislators and school officials need to have these complex issues translated into concrete policy choices. The National Institute of Education has been a crucial source of research and dissemination funds.[3]

As the paper by Friedman and Wiseman in this collection indicates, we are now in a period of reconsidering some of the basic premises and measuring the impact of this era of public policy innovation. This type of work needs to be supported. But in our view the substantial number of scholars in the field and the interests of state and local governments will insure a large research output. The role for NIE is more in filling gaps and exploring the more basic discipline oriented areas. For example, organizations such as the Education Commission of the States (ECS) and the National Conference of State Legislatures (NCSL) can handle applied issues such as formula revisions and dissemination of developments in various states. We will list some specific priorities for NIE given the activities underway by other research oriented organizations.

Short-Term Research Priorities

In our view the research directions emerging from this symposium are primarily in the uncharted and novel areas as compared to the "mature" issue of school finance. We have no objective scale for ranking the extensive research menu listed in the appendix. Our view of the priorities is in part dictated by our "world view" and involvement in policymaking, as well as knowledge of the status and likely evolution of the existing research base.

Given this perspective of priority for relatively unexplored areas, we would recommend the following as research endeavors for *immediate NIE attention.*

Out-of-School Influences on School Performance

How do family structure variables interact with those of family SES and neighborhood (physical environment, social environment,

types and availability of public resources) to influence the child's educational development? To what extent are children's behavior and time allocation patterns related to societal class? Moreover, are children's time budget patterns related to school performance? Can an examination of these out-of-school variables provide insight into the forces influencing school performance?

Do parent training programs have a positive effect on the children's school performance? Are the time allocation and behavior patterns of parents and children different before and after training? In short, we need a multiple-faceted investigation of the impact of parent education which has been featured in recent federal policy pronouncements.

The Use of Courts to Establish and Implement Education Policy

Have the courts the ability to deal with complex economics and behavioral sciences data, utilized in equal educational opportunity cases? Is it possible to simplify the data for judges but still present the issues adequately? Is the adversary system an appropriate approach for these issues? Are there alternatives to the adversary system which may be feasible and should be examined?

What are the specific linkages from the court decisions to actual impact on children? How have these linkages actually functioned? Where have they broken down? What do these linkages suggest about changes in legal structures or procedures?

What is the role of courts in monitoring and assuring that the guarantees of the law are being implemented? What techniques are particularly useful in implementing court orders? To what extent do state and local education agencies comply with court mandates?

Testing Alternative Explanations of the Problems With Transition From School to Work and "Over-Education"

Grubb's paper raises four major alternative conceptual frameworks which deserve detailed examination and verification:

- imperfections in the human capital model
- demographic causes
- credentialing
- degradation of labor

Moreover, the following questions he raises need to be addressed:

Are the models of schools as sorters validated in actual practice? How and on what do employers base their employment selection? Much has been written about the sorting operations of schools but there has been little attempt to partial out the distinctive and unique factors caused by schools.

Our highest priority areas focus on broader societal forces that impact fundamentally on our various educational institutions. Researchers have talked about socioeconomic factors that affect school performance and student choices but have rarely explored them systematically (other than statistical regressions of widely available data). Now is the time to probe out-of-school influences in depth.

We know it is possible to explore rigorously the conditions under which children grow up, e.g., the resources (broadly defined) made available to children by parents and other adults, the characteristics of the neighborhood in which children live, conditions of access to non-school community services, etc. Further, we can begin to see how the conditions of childhood affect the experiences of individual children. The next, and very important step has not yet been taken: to investigate how the experience of individual children in their out-of-school lives are related to the progress of those children in school. In the analysis of educational production functions, we suggest it is time to open up the "black box" of SES.[4]

Moreover, we believe the current research on federal/state children's services has progressed to a point roughly equivalent to where research on the economics and governance of public education was in the mid-1960s. Exploratory analyses of variations in service levels within states, and the economic and political causes for such variations are now needed. One conceptual approach that might be borrowed from school finance is the adequate foundation concept, e.g., no child should fall below a minimum service level regardless of where he or she lives. This analogy would only hold for services that have a widely agreed minimum level, such as health screening and children's protective services. A goal could be to discover the extent to which there is variation in children's services, where "minimums" were not met, and the extent to which such service levels were caused by federal, state, or local policies.

Longer Range Research Priorities: School Organization

In view of the limits on NIE funds, the areas discussed below fall in our second tier of priorities—they can be delayed but deserve earmarks in long range NIE plans.

NIE has supported a considerable amount of work in organizational theory and change. The Symposium considered this area and we recommend several additional dimensions to prior efforts as discussed in the Deal/Derr and Guthrie papers.

- What efforts for educational change have worked and why? Using the Deal-Derr three-dimensional framework, such studies would identify and analyze examples of successful change or reform endeavors in education. Specifically, what are the strategies and techniques used to alter and maintain an internal organizational equilibrium in the face of external and environmental pressures? What types of changes, reforms or pressures are most likely to be successful in an educational organization? What are the common characteristics of reforms which have left an im-

print on schools compared to those that were rhetorical? We are aware that NIE has some effort ongoing in this area. However, we feel it needs to be deepened and extended.

We need to extend existing studies that investigate the incentive structure in education, perhaps using the Deal-Derr three-dimensional framework. Specifically, what are the incentives for administrators, teachers, and students? Do differences in incentive structures result in changes in behaviors and along which dimensions?

Does the size of the school attended affect the educational outcomes of a student? Is there a relationship between size and pupil performance? Is this relationship general or is it itself a function of characteristics of structure (SES, etc.)? What is the nature of the relationship? Is it different at elementary and secondary levels? Moreover, does the scale of the school, e.g., the size and number of pupils, affect teacher morale and productivity? Perhaps this research should start with theoretical models of the potential relationships mentioned above.

What has been and are likely to be the effects of participatory mechanisms for education, such as parent advisory councils, state advisory committees, in such areas as handicapped and disadvantaged programs? Specifically, who serves on these bodies and how are they elected? What activities and areas do they influence? How do they interact with professional staff?

The first two research recommendations depend on the usefulness of the Deal-Derr three-dimensional theory framework to guide inquiry. We believe the Deal-Derr approach has sufficient analytical strength to warrant scrutiny by organization theorists.

New Directions in Education Finance Research

We have purposely left a detailed discussion of research initiatives in education finance until last. Reform of public school finance has passed through several stages in the last twenty years. Stage one from about 1957 to 1965 consisted of policy paralysis and a gradual increase in inequity. The assessed value of local property became a greater and greater determinant of a child's educational opportunity. State legislatures did not perceive school finance as an equity issue but were concerned with rapidly increasing enrollments. School districts with a high property value per pupil were able to meet these expanding enrollment pressures with a much more extensive and intensive education program than their less favored neighbors.

The stage from 1965 to 1970 laid the conceptual basis for large scale reform and moved the issue to the top of state and national agendas. Legal doctrines were formulated that provided the impetus for judicial intervention and court orders. The first significant decision (*Serrano*) was handed down in 1971 and shortly after that other state courts also acted.

The third stage from 1972-1976 was an era of intense and rapid legislative and political activity. Political momentum developed at an astounding pace. In the 1973 legislative session, over 30 governors asked for significant state tax reform and 21 requested school finance reform. Important finance measures were passed between 1973 and 1975 in Florida, Colorado, Minnesota, Illinois, Maine, Montana, Kansas, Utah, California, New Mexico, Michigan, and Connecticut. But during this third stage the Supreme Court ruled in *Rodriguez* that it would not intervene, leaving the issue to the states.

A fourth stage began around 1976 and was spurred by reviews of the impact of this legislative activity, and new court interpretations which clouded and raised questions about court edicts in the second stage.

The Friedman/Wiseman paper questions the underlying theory and concept of equal educational opportunity as it has been employed in school finance. The most popular overall concept is "fiscal neutrality" which simply states that the amount of money spent on a pupil should not depend upon the wealth of the school district in which he or she lives. But this seemingly simple concept has many possible meanings (e.g., ex ante vs. ex post) and even more possible ways of statistically measuring it.[5] Recent court suits in Ohio and Washington give several states an alternative to the fiscal neutrality standard. Indeed, parts of the Ohio case appear to declare one of the leading remedies for reaching fiscal neutrality (district power equalization) unconstitutional! In sum, we recommend research to clarify the various finance concepts and measurements of equal educational opportunity and to explore the relationships and inconsistencies among them. Under certain conditions, expenditure inequalities represent inequality of opportunity; under other conditions, expenditure equality is a departure from equality of opportunity. The real issues thus raised in the Ohio and Washington cases are two: (1) who should determine the degree of inequality, the state or the set of local authorities? and (2) by what standards should such departures from expenditure equality be judged?

The other priorities in school finance focus on the impact of the substantial amount of reform now underway:

> What are the impacts of school finance reform on choices for residential/industrial locations and property values? Specifically, what is the impact of school finance reform on the capitalized value of land? This might entail development and testing of a productive model for changes in household location in response to post-reform educational costs and opportunities.
>
> Although some preliminary work has been completed, we need an improved use and development of demand functions for simulation studies of school finance reform. Such functions would specify the effect on district expenditure levels of voter incomes, size and composition of the local property tax base, the amount and character of educational grants, and the price of education to the district. This would tie in with studies of tax and expenditure referenda before and after the implementation of school finance reform.

How are changes in demography and voter profiles interacting with school finance reform? Specifically, is enrollment decline and fewer voters with school age children reinforcing some of the problems caused in wealthy districts by school finance reform? Are specific types of districts more likely to be confronted with this "double vise"?

We have summarized all the research suggestions from the papers in the appendix. The rationale for these various suggestions is contained in each of the papers. Some of these suggested areas of research may take on higher priority in the future, though we have not included them in our list of three areas for immediate NIE attention. For example, David Stern's paper indicates that recurrent education will become an increasingly important area of concern as the baby-boom cohort grows older, and to the extent that employers and labor unions in the United States begin to institute "workplace democratization" along the lines of recent reforms in Western Europe.

The appendix permits readers to rank research areas according to their own sense of priorities.

References

1. See Walter Garms, *et al., School Finance* (Englewood Cliffs: Prentice Hall, 1978).
2. See Ford Foundation, *Paying for Schools and Colleges* (New York, 1976).
3. See various work plans of NIE, School Finance and Organization Division.
4. One simple problem illustrates the significance of this venture. Much concern in education has been directed to the question of whether Title I expenditures are additive to state and local expenditures in low-income schools or simply displace state-local resources. An equally interesting question is whether Title I resources add to or displace parental support for school progress.
5. See Robert Berne, "Equity and Public Education: Conceptual Issues of Measurement" (New York University Graduate School of Public Administration, Working Paper No. 4, 1977).

Appendix

Education Finance and Organization: A Research Agenda for the Future

Michael Kirst and William Hartman

Recap

The Appendix presents a detailed agenda of research topics in the areas of educational finance and organization. The emphasis of this agenda is purposefully long range. The research studies recommended are designed primarily to investigate broad, underlying issues about which there is insufficient basic information at the present. The purpose of this approach is to identify critical areas needing further research, in order that the results from the necessary theoretical and empirical studies will be available to assist policy makers in dealing with educational problems over the next decade.

The research suggestions are grouped according to topic. They center around each of the papers presented previously. The specific suggestions made by each author have been summarized and are included. Additionally, the suggestions reflect comments and questions made by the seminar participants during the discussion of each topic, as well as suggestions from outside reviewers and those developed by the editors of this collection.

Role of the Judiciary in Equal Educational Opportunity

The paper; "Law, Politics, and Equal Educational Opportunity: The Limits of Judicial Reform," by David Kirp posits two hypotheses concerning the present and future involvement of the courts in educational equity cases. First, that judicial involvement has declined due to the political and non-judicial nature of many equal educational opportunity questions. Second, that the involvement that the courts do have is most appropriately interpreted as contributing to an essentially political resolution of such questions. Out of these premises arise a number of use-

ful and interesting topics for further research. They are presented below and have been grouped according to four major themes: 1) interrelationship between the judicial and political branches of government; 2) impact of judicial and legislative actions; 3) judicial process; and 4) significant legal questions in doubt.

The first set of research questions centers on the area of intergovernmental relations and the interaction among the courts, legislature, and federal, state, and local agencies.

1. What has been, is, and will be the interrelationship between the courts and the legislature and the various educational agencies in the development of equal opportunity policy?

a. Institutional analyses of the role of the courts vis-a-vis education are needed. Current understanding of the role of the courts is either incomplete or severely restricted.

b. What are the specific linkages from the court decisions to actual impact on children? How have these linkages actually functioned? Where have they broken down?

c. An examination of similar situations in areas other than education should provide useful insights and analyses, particularly when anticipating the future actions of the courts in education.

d. An improved theoretical and practical framework for determining responsibilities between the courts and the other branches of government would be helpful. Which matters are appropriate for the courts to decide, which for the legislature and executive branches, and which require interplay and interaction?

e. If, as Kirp suggests, the courts are not to be as active in equal educational opportunity policy making in the future, what are the likely alternative mechanisms that will function? What groups will be involved? How and to what extent?

f. What are the probable judicial responses to legislative vagueness in education statutes?

g. How will the courts view interpretive regulations promulgated by educational agencies which provide more specification (and possibly restriction) than does the enabling legislation?

h. To what extent do the state and local education agencies comply with legislative mandates? What is the role of the courts in monitoring and assuring that the guarantees of the laws are being implemented?

2. What is the authority of the legislature, under the Fourteenth Amendment, to define rights and remedies in narrower terms than the courts have ordered under the equal protection clause?

In the last decade, there have been a number of important judicial, legislative, and agency actions in the area of equal educational opportunity. These next research questions call for an examination of the impact of these actions.

1. What has been the response to the judicial activity and decisions in each of the following areas: intradistrict resource distributional equity; education rights of handicapped children and of non-English speaking students; and sex discrimination?

a. What has been the response of the defendant, i.e., educational agency, involved? A careful description of the specific details and an evaluation of the results is needed.

b. What has been the role of the federal agencies involved, e.g., HEW, Office of Civil Rights?

c. Have there been additional and supplemental law suits to the primary cases? What have been their outcomes?

d. What voluntary actions by educational agencies have been taken? What have been the courts' responses to these actions?

e. What is the state of educational research on effective programs in each of the areas? Since many of the suits involve claims of harm with or without certain programs, it is increasingly necessary to know what works and what doesn't.

2. Have the outcomes for children been altered by the recent judicial and legislative guarantees? Have the reforms involving equal educational opportunity concerning educational finance, or handicapped, non-English speaking, or disadvantaged students had an effect on the students involved? To what degree and how?

3. What has been the impact of legislation imposing new requirements on states and districts or tying money to particular requirements or reforms?

a. For example, have the federal (and state) regulations requiring careful monitoring, e.g., Title I, been effective? Have the costs been worth the results? How well has the mandated "comparability" been enforced?

b. Which of the new judicial requirements and judicial reforms have been successful in accomplishing their objectives (e.g., Serrano, PARC, Mills, Lau)? What are the characteristics of successful efforts? Why have others not succeeded?

c. What changes have occurred in the cultures of the affected institutions as a result of the new mandates and monies?

d. How are violations dealt with, if at all? Is there a different system of sanctions that would be more effective and appropriate for education?

The use of the courts as a forum for equal educational claims has involved the judiciary in a most complex area. The following set of research suggestions involve an investigation of some of the specific means by which the judicial process has dealt with educational issues.

1. Have the courts the ability to deal with complex economics and behavioral science data utilized in equal educational opportunity cases?

a. Is it possible to simplify the data for judges, but still present the issues adequately?

b. Is the adversary system an appropriate approach for these issues? Are there alternatives to the adversary system which may be feasible and should be examined?

2. Have the use of court appointed masters to aid in the fact-finding and data assessment process and to supervise remedial activity proven effective?

3. How effective has been the creation and utilization of non-judicial institutions to resolve problems of equity in education?

a. Review panels.
b. Due process hearings.
c. Ombudsmen.

Finally, there are several important legal questions concerning equal educational opportunity which remain unresolved. These problems will require additional research and analysis if appropriate solutions are to be reached.

1. How is equal educational opportunity to be defined? Better, more useable, and operational definitions of equal educational opportunity are needed to aid policy makers.

a. What are appropriate measures or standards to be used?

b. Should there be different definitions and measures for different student populations, e.g., handicapped? What would the differences be and why?

c. Should different definitions and measures be used by different governmental bodies?

d. How should one distinguish between short run and long run effects of educational reforms?

e. How should one distinguish the effects of a judicially caused reform separately from the effects of declining enrollments and collective bargaining?

2. Can there be determined a judicially workable definition of "educational need" and the appropriate remedies?

a. How can the "conflicting solution" problem be resolved, i.e., competing theories about what is an appropriate education for certain students? The problems of conflicting learning theories and educational philosophies are particularly acute for the handicapped and the non-English speaking students.

3. What are the educational rights of children who live in communities that disapprove of tax elections and bond issues which result in shortened school years?

a. Should or must the state provide additional aid to prevent or compensate for harm received from such actions?

b. Should or must the state step in to keep open schools that are threatened by closing due to lack of community financial support?

4. What is the constitutional permissibility of ignoring "municipal overburden" in calculating state school aid?

School Finance Reform and Educational Equity

The paper by Lee Friedman and Michael Wiseman, "Toward Understanding the Equity Consequences of School Finance Reform," addresses the significant question of how past and potential reforms in the states' school finance system affect the equality of educational opportunities available to children. A research agenda for this area is proposed in three parts: 1) concepts of equity; 2) actual impact of reforms on equity; and 3) behavioral responses to school finance reform.

First, the authors call for more research and theoretical development to clarify the various concepts of equal educational opportunity and to recognize the relationships and inconsistencies among them.

1. Are there additional issues concerning equal educational opportunity (in addition to those selected by the authors—divisional and distributional equality,

simple and conditional neutrality)? Are they consistent with each other or do they conflict when applied simultaneously?

a. What is the extent of intradistrict per pupil expenditure disparities among school districts generally? Are these a result of race or socioeconomic status of the pupils involved?

b. Should the resources be measured in real or nominal terms?

1) Municipal overburden

2) Cost of education indices

c. Should inputs (resources) or outputs (educational outcomes) be used as the appropriate measures? What are the consequences of selecting each one?

d. Should pupil "need" be considered in equal opportunity issues? How can this be defined for various types of students? What has been the impact of pupil weighting systems on interdistrict spending disparities, on the magnitude of specialized funding, and on pupil outcomes?

2. What is an appropriate definition of categorical assistance and expenditures?

a. How do the present federal and state categorical aid programs affect equity? Under which definitions of equity?

b. Do the funds from these programs go toward legitimate categorical needs?

1) Higher prices for inputs.

2) Requirements for additional inputs.

c. Have categorical programs encouraged excessive labeling of children (e.g., mentally retarded) by districts to obtain additional funds?

The second research area concerns an examination of how school finance reforms which have been undertaken have changed the equality of educational opportunity, under any definition. The specific topics focus on improved empirical studies on what has happened as a result of recent reforms.

1. What has happened to educational equity in states that have undertaken school finance reform? Has equality been improved to a greater extent than in states which have not enacted reforms?

a. Multiple state comparison, including both reform and non-reform states.

b. Intensive studies of individual state systems to include:

1) School district characteristics likely to influence spending behavior.

2) Construction of detailed cost indices for districts.

3) Actual allocation of expenditures.

2. What improvements can be made in measures of family income of children within a school district?

a. Can the available data from Title I eligibles count be improved?

b. Do the data from the food stamp program provide a useful alternate source for income information? How could it be used?

3. How are the funds made available through both categorical and general purpose grants utilized?

a. Is there an empirical justification for the allocation of categorical funds? Do the price differences for which they are made actually exist?

b. Do expenditures in districts with exceptional costs actually exceed those without them?

c. Have school finance reforms, particularly new monies to equalize per pupil expenditures across districts, affected the composition of district expenditures? How have such funds been used, e.g., increased teacher salaries? Such an analysis requires the development and use of a price index for deflation of outlays.

4. What effects on student outcomes can be identified as having been caused or influenced by expenditure changes that have come about through school finance reform?

a. Has academic performance been affected?

b. Have other, less quantifiable, student outcomes been affected, e.g., attitudes, social behaviors, career choices?

In the final research area, recommendations concentrate on the development of additional and improved predictive theoretical modeling and empirical testing of behavioral responses of school districts, families, and firms to school finance reform. Such an emphasis is chosen: 1) because the effectiveness of the reforms depends upon the behavioral responses; and 2) to uncover important, but unintended or unexpected consequences of the reform.

1. Additional econometric studies of the demand for education are needed.

a. Supply versus demand studies.

1) Influences of short and long run variations in demand for education on its price. For example, if costs to local taxpayers is an important factor in determining the resistance of school boards to salary increases, then average teacher salary increases should be greater in districts in which higher levels of state aid or matching grants reduce the cost to the taxpayers. Is this assertion borne out empirically?

2) Speed and pattern of adjustments to changes, particularly abrupt changes wrought by finance reforms, and the impact of such changes on post reform expenditure levels (e.g., prices—teacher salaries; quantity—student-teacher classroom ratios, number of administrators; "quality"—new types of personnel, equipment).

3) Use of improved estimated demand functions in simulation studies of school finance reform. Such functions would specify the effect on district expenditure levels of a) voter incomes, b) size and composition of the local property tax base, c) the amount and character of educational grants renewed, and d) the price of education to the district.

b. Demand function specification in an improved mathematical formulation. Such formulations could reduce the restrictions and distortions on the analyses of the effect of intergovernmental grants on district expenditures caused by the now common log-linear form.

2. Analyses of the demand for education based on tax referenda data.

a. Effects of changes in electorate composition (e.g., increasing age and reduction in proportion of school children) on demand for education.

b. Studies of tax and expenditure referenda before and after the implementation of school finance reform.

3. What is the impact of school finance reform on the location of business firms?

a. Theoretical models of tax effects on businesses with testable implications.

b. Effect of finance reforms on valuation of commercial and industrial property; particularly, those which differentially affect the levels of property taxes across school districts.

4. What are the impacts of school finance reform on residential location and property values?

a. Effects on property values of large-scale reforms.

b. Development and testing of a predictive model for changes in household location in response to post-reform educational costs and opportunities.

5. What are the determinants of choice for families who send their children to private schools?

a. Reasons for growth and changing nature of private schools.

b. Relationship between private school utilization and public school expenditures.

6. How is the use of privately provided supplemental education by families with children enrolled in public schools determined?

a. How widespread are these activities?

b. Do they vary with public school expenditures?

c. Do they vary with the amount and intensity of school finance reform?

One way of exploring the questions raised concerning the behavioral responses of school districts, families and business firms to school finance reform is to focus on a specific subset of school districts. Much of the discussion and concern of school finance reform has centered around the reallocation of monies to improve low and medium wealth school districts. Little systematic research has been undertaken to investigate the impact of such reforms on high wealth districts, particularly as changes either restrict or reduce the funds available for their school programs. The following set of research questions could be examined using a comparative design in a national sample of wealthy suburban districts (e.g., both high assessed value and per capita income).[1]

1. What programs or types of expenditures are eliminated or reduced when expenditures in high wealth districts are restricted? Is teacher instructional time with students affected? If so, what subjects, instructional processes, or student populations are impacted?

2. Can the causes for the program changes be identified and the impact of each specified? For example, the importance of enrollment declines vs. state school finance changes.

3. Are high wealth districts locked into certain high fixed costs because of past state regulations and their own practices, e.g., most teachers at upper end of salary scale?

4. What are the decision processes and criteria utilized in making program cuts or reductions?

5. How are the changes in demography and voter profiles interacting with school finance reform? Are high wealth districts experiencing changes that aid or hinder schools' efforts?

6. Has school finance reform caused a change in the perceptions of the citizens in high wealth districts of the quality of their schools? Is school finance reform seen as damaging the quality of education available in the public schools?

7. Have wealthy school districts resorted to devices such as private foundations or shifting certain school services to the city budget in order to avoid recapture of local tax monies and state school equalization?

8. Does school finance reform cause an increase in private school enrollments? If so, what are the reasons behind such a trend?

9. Have low income tax payers in high wealth districts been heavily taxed as a result of school finance reform changes with no commensurate state circuit breaker property tax relief? What have been the changes in tax burden on this group before and after school finance reform?

10. What are the mid and longer term effects of school finance reform on:

a. Value of suburban housing?

b. Locational choice by businesses?

c. Changes in socioeconomic status patterns of families and students moving into and out of the districts?

d. Changes in local preferences for mix of local public services, i.e., schools vs. police, fire, health, recreation?

Organizational Change in Education

The paper by Terrence Deal and Brooklyn Derr, "Toward a Contingency Theory of Organizational Change in Education: Structure, Processes, and Symbolism," develops a three dimensional framework for conceptualizing and analyzing organizational change in schools. Change is defined as the internal organizational response to external pressures along three separate, but interrelated dimensions—structure, process, and symbolism. The authors suggest that unless all three aspects of an organization are considered when planning organizational change, the effort will be less than fully successful.

A research program to evaluate this view of organizational change in education will require a thorough evaluation of both the theoretical foundations of the three linked dimensions and the actual and potential application in schools. Accordingly, the research directions suggested by the authors and implied in the concept fall into two categories—theoretical development and empirical data gathering and testing.

The theoretical inquiry should be focused generally on strengthening the conceptual base proposed in the paper. This would involve the series of research studies outlined below.

1. Extend the theoretical development of the three dimensional approach. The description presented in the paper is necessarily general, but provides an out-

line for rigorous elaboration. Such an effort would include:

a. Refinement of the concepts of structure, process, and symbolism.

b. Thorough enumeration of the features of each dimension.

c. Specification of the relationships among the dimensions.

d. Identification of the conditions under which these relationships would be expected to hold.

The most underdeveloped dimension is that of the symbols and myths in education. Particular emphasis should be placed on the identification and definition of the major symbolic characteristics prevalent in school systems.

In this theoretical development some consideration should be given to the identification and specification of other important contingencies or contingency areas (beyond the three dimensions), if any. Some idea of their relative importance and their relationships with each of the three dimensions is needed.

2. Recast or reorganize the existing literature on educational change in organizations into the three dimensional framework. This effort would involve the integration, if possible, of the three dimensional concept into the major theoretical approaches.

Additionally, the reported studies of educational change and reform efforts of the 1960s and 1970s should be reevaluated and reinterpreted using this framework.

3. Incorporate new developments from the social and management sciences into the three dimensional approach. A multidisciplinary approach involving sociology, psychology, political science, anthropology, organizational theory, and economics, among others, should add to the breadth of understanding of organizational change in education.

4. Develop case studies of school situations using the three dimensional framework. The case studies would be used to clarify, analyze, and refine the three dimensional concept. Such studies would highlight:

a. Existence of three separate dimensions to consider.

b. Interactions among the dimensions.

c. Ways in which an organization maintains equilibrium among the three dimensions in the face of outside pressure for change.

5. Conduct surveys of existing organizational patterns and process in schools, particularly at the high school level. In addition to documentation of current practices, an investigation of the incentive structure in education could be carried out along the three dimensional framework.

a. What are the incentives for administrators? for teachers? for students?

b. Do differences in the incentive structures result in changes in behaviors? along which dimensions? in the expected and desired directions?

6. Conduct comparative organizational studies across educational and non-educational organizations in different environments. Such studies will provide an opportunity to evaluate the theoretical concepts and their expected outcomes in organizations.

7. Develop new and improved measures of educational effectiveness. Better measures of learning outcomes than are presently available from standardized tests are called for. In addition, entirely new types of measures which capture currently unavailable aspects of schools are necessary, e.g., participant morale,

community support, quality of the educational experience, student creativity and independence.

8. Develop measures for each of the three dimensions—structure, process, and myths. If this approach is to be useful, adequate descriptions and measures of the features of each dimension are necessary. Such measures of organizational characteristics will provide a more appropriate means of analyzing and comparing organizations and their efforts at change, than the highly subjective assessments relied on primarily today.

9. Investigate the pressures for change and reform on an educational organization. The paper concerns itself with outside pressures, but does not address the possibility of internally generated pressure for change. This raises the question of whether all pressures for change come from outside the school organization. If not, is there a difference in character and/or outcomes for internally initiated change?

Additionally, the types of possible outside pressures are not differentiated. It would be useful to know if the schools respond differently to different kinds or sources of pressure. What are the important characteristics of outside (or inside) pressures that make schools more or less responsive to change?

The empirical portion of this research program should be designed to gather and analyze evidence that will both aid in the evaluation of the efficacy of the theoretical constructs and assist in planning successful efforts for organizational change. Three approaches to the empirical studies are suggested; they are natural experiments, laboratory experiments, and planned interventions in field settings. The use of each will depend on the availability of appropriate organizations willing to participate and the nature of the specific study. A series of suggested research studies is presented below.

1. Conduct empirical studies to test the validity of the three dimensional approach. As the theory, concepts, and relationships of this approach are further developed, a companion effort to evaluating their effectiveness in explaining and predicting organizational change in schools should be carried out.

a. Does the three dimensional approach explain adequately the observed organizational responses to outside pressures?

b. Are there changes along each of the dimensions in accordance with those predicted by the theoretical relationships among the dimensions?

c. Is it possible to implement change affecting one dimension only? In two dimensions only? Under what conditions?

2. Investigate what efforts at educational change have worked out and why. Using the three dimensional framework, such studies would identify and analyze examples of successful change or reform endeavors in education.

a. What are the strategies and techniques used to alter and maintain an internal organizational equilibrium in the face of external and environmental pressures?

b. What types of changes, reforms, or pressures are most likely to be successful in an educational organization? Why? What are the least likely to succeed? Why?

c. What are the common characteristics of the reforms which have left an imprint on schools compared to those that were rhetorical?

d. Are there cycles of reform in education which reappear periodically? Which types of reforms have left a residue or had a lasting impact on schools and which have been passing fads which are periodically recycled? Which of the recurring reforms (e.g., competency movements) have been tried and not implemented or discredited in the past? Studies of this nature should include an investigation of the impact of reforms at the classroom level.

e. Does the involvement of school personnel—administrators, teachers, students—improve the chances for successful implementation of changes? How? Why?

f. Have there been successful efforts to change the symbols, myths, and social expectations concerning schools? What were the features changed? How was it accomplished?

g. How do the external pressures and requirements get integrated into the on-going structure, process, and symbolism in successful efforts. How do they get modified or subverted in less successful efforts?

3. Examine the relationship between the external environment and the internal organization.

a. Are certain patterns of structural, process, and symbolic characteristics of educational organizations more effective in implementing change under different environmental conditions?

b. Does the internal organization make a difference, or are the outside pressures dominant? Under what conditions?

c. It is possible to design and implement an externally induced change independent of the characteristics and desires of the internal organization? What would be the key features of such an effort?

4. Test the predictions cited for the loose-coupling theory.

a. Successful educational organizations will respond to outside pressures by adding new units, but will buffer them from the existing structure and activities.

b. Successful educational organizations will resist changes requiring evaluation of their activities.

One final research area should be mentioned. The three dimensional framework implies a hierarchy among its dimensions, with the symbolic aspects being the most difficult to change, but also the most important in obtaining successful changes. This suggests a somewhat normative, but critical research area: What are the new symbols and myths that should be created in order to enhance the chances for desired changes in education? Is it possible with this framework to match the desired change or reform with the current inhibiting myths and the needed new myths? Is it also possible to specify and synthesize the concomitant changes necessary in the structural and process dimensions to maintain the internal equilibrium of the organization?

Effects of School and District Size

The paper by James Guthrie, "Organizational Scale and School Success," describes the widespread school and school district consolidation movement and points out the meager and contradictory research base underlying it. Three major areas of research studies are suggested to provide sound data and reliable results on which to base the continuing policy decisions regarding organizational size of schools and school districts; they are: 1) the economic efficiencies (and inefficiencies) of school and district size; 2) the educational effects on students and staff of school and district size; and 3) effects on public control and participation of school and district size.

A preliminary recommendation, however, is a complete review and analysis of the extant research studies relating the effects of the organizational size of schools and districts. Such a review should cover not only studies in all of the social science areas, but in related fields such as educational administration, business administration, and industrial engineering as well. In this review, as in the following research suggestions, particular care should be taken to distinguish among the various organizational levels (district, school, classroom) and the functions (financial, political, instructional outcomes) of each level.

The first area concerns the economic efficiency (or lack of it) of the size of the educational enterprise. What are required are thorough analyses of the empirical relationships between the scale of educational organizations and their economic efficiencies:

1. Do larger schools, or larger school districts, lead to economies of scale? Are there concomitant diseconomies of scale, as well?
2. What are the nature of any significant economies or diseconomies?
3. Do these economies or diseconomies vary with other characteristics than size of the school or district, e.g., urban vs. rural, population density?
4. How much money, if any, can be saved through future consolidations and school closings?

The second area for additional research concerns the educational effects of school and district size. What are the impacts on students and professional staff of the size of the organization in which they function?

In studies of this nature it is critical to control for student input characteristics, such as ability, family background, community characteristics, in order not to confound these with school or district size.

1. Does the size of the school attended affect the educational outcomes of a student? Is there a relationship between size and performance? What is the nature of the relationship? Does district size have an influence on student outcomes?
2. Does the size of the school or district affect the quality of instruction? If so, what is the nature of the relationship?
3. Does the scale of a school affect teacher morale and productivity?
4. If there are effects of school size, do they result from the actual scale of the physical plant and the size of its population, or from the size of the organization in which the students and teachers participate?

5. What would be the relative utility of "mini-schools," or schools within schools, as a strategy for reducing organizational size, while maintaining some of the scale economies associated with larger units?

6. What is the impact of school and district size on the nature and accuracy of the perceptions of both students and parents concerning school performance? Does the school or district size influence the use to which such information is put?

The final research area suggested concerns the relationship between school and district organizational scale and public control over and participation in these organizations.

1. What was the nature of political participation, both at the school and district level, prior to the influence of the consolidation movement? Studies of participation rates by social and economic strata and by geographical location should be included.

2. Is there a relationship between the size of schools and districts and public participation in school governance?

a. Are there differences between large and small schools and school districts along such dimensions as: voter turnout; votes for non-incumbents; number and length of school board meetings; characteristics of school or district decisionmakers; interest group activity; degree of partisanship; and linkage of the school political system to the wider political sphere? Such analyses should disaggregate participation measures by economic, demographic, and geographic factors.

b. Is public allegiance to schools affected by changes in district boundaries or school attendance areas and by increases in size of either?

c. Has the increase in school and district size had any effect upon political participation over school matters?

d. Does the organizational size of schools or districts have an influence upon parent participation in their own child's schooling?

e. Does the reduction of representativeness and political access (more constituents per school board member) affect public participation in school issues?

f. Do interest groups compensate at the local level for the absence of personal contact? What has been the result of this change?

3. What has been and what are likely to be the effects of newly created participatory mechanisms for education, such as ESEA parent advisory councils, state advisory committees for the education of handicapped students (required under P.L. 94-142)?

a. Who serves on these bodies? How are they selected?

b. How much do their members actually participate?

c. In what activities do they engage?

d. How do they interact with the professional staff of the schools, districts, and state educational agencies?

e. What impacts do they have?

Demography and Enrollments

In her paper, "Demography and Changing Enrollments," Harriet Fishlow presents projections of elementary, secondary, and postsecondary enrollments

through the year 2000. The major message in these projections is that all educational levels face substantial enrollment changes in the coming years--from the current enrollment declines to expected increases in the next ten to 15 years. Two main research areas are suggested: monitoring and improving projections of educational enrollments; and strategies for dealing with drastic enrollment fluctuations.

While the basic work in population projections is and will contine to be done by organizations such as the Census Bureau and the National Center for Health Statistics, it will be necessary to develop new and improved techniques and projections to satisfy specific educational needs. Several studies of this nature are suggested below.

1. Continued investigation of the critical assumptions underlying the population and enrollment projections, in order to ensure an appropriate base for estimating future school enrollments. Will they require revision in the light of new and emerging trends in society? Examples of such assumptions would be rates for high school completion, college (four year and junior) attendance, and college completion.

2. Create local and state capabilities for enrollment estimation and forecasting.

a. Use of trained state level units to assist districts in making more reliable projections of their enrollments is suggested. A pilot project to test the feasibility of this type of assistance would be an appropriate first step. California is recommended as the test site since it has an active projection unit in the State Department of Finance. For adequate planning time for long range decisions involving personnel and facilities, the district capability should be extended to five to 15 years.

b. Better methods and techniques for estimating migration patterns for school districts need to be developed. Districts need reliable enrollment projections at the local level to carry out their planning. National or state trends may provide little useful information or even be counter to actual district experiences. Can enrollment projection data be developed at a level specific enough to be of use to local planners?

3. Develop alternative and new projections of population and enrollments. Several different kinds of projections, either based on different techniques or assumptions or providing related information, would be useful.

a. Projections using a variable birthrate assumption. Such an assumption would begin with the present actual value and phase-in toward an assumed stable value in a certain number of years. This could eliminate the discontinuities which occur when the value selected for the birthrate is different from current experience.

b. Projections with birthrate as a function of the economic condition of the time. Such projections should be preceded by an analysis of the relationship between birthrate and economic condition. Future projections on this basis, however, will require long range economic forecasts which are perhaps subject to greater inaccuracies than straightforward estimation of the birthrate.

4. Develop projections of the resources and the costs necessary to meet the enrollment projections. What do the projected numbers of students mean in terms of teachers, administrators, buildings, and dollars required for their education?

5. Conduct studies of certain specialized populations which may differ substantially from the overall patterns of the total population. What are the major trends associated with these groups? How will their future enrollments be affected?

a. Handicapped. Epidemics such as rubella can cause increases in children with certain types of handicaps, while improved medical procedures and early education programs can remediate previously handicapped children or at least prepare them better for school.

b. Disadvantaged. Population shifts, particularly in urban areas, have been and may continue to cause changes in the composition of the school population. This can have important implications for school planners.

Of primary interest to local administrators will be various strategies and techniques which can be used by the school districts to respond in an effective and efficient manner to enrollment fluctuations. A number of possible responses which require further research and testing are suggested below.

1. What methods of coping with the employment problems caused by changing enrollments are appropriate for districts and under what circumstances? What effects would such methods have on personnel requirements, student learning, and district costs? The possible responses outlined below should be researched and analyzed. Since many of the following would require changes in existing legislation and regulations, the analyses should also consider the political ramifications and strategies of the possible responses.

a. Retraining teachers for new school positions, e.g., secondary teachers to elementary, regular teachers to handicapped programs.

b. Teacher renewal programs for improving skills and motivation of an increasingly older and more experienced teacher force, including a "teachers teaching teachers" strategy, and opportunities for teachers to engage themselves in applied research in education.

c. Inter-district switching or trading of teachers for both short and extended periods.

d. Teacher job sharing.

e. Alternatives to teacher layoffs, e.g., reduced class size, more specialized support personnel.

f. Inclusion of layoff provisions in collective bargaining agreements and their impact.

g. Alternatives to the tenure system and seniority layoffs.

h. Programs to retrain and find new employment for teachers and administrators.

i. One year certification programs for holders of appropriate college and graduate degrees to increase supply of teachers rapidly.

2. Will there be another teacher shortage in ten to 15 years as enrollments in elementary and secondary schools begin to climb again?

To study this possibility will require a thorough analysis of the current teaching force, e.g., age, patterns of turnover, likely retirement years, along with a

projection of these (and other factors) into the future. Then a match could be made of the expected stock of current teachers, plus additions, with the projected demand to see if the projected supply (from teacher preparation programs less attrition to other careers) is sufficient to fill the gap.

3. Are teacher salaries responsive to the forces of supply and demand in the educational labor market? Or is it a one way street—salaries rise in times of high demand, but do not decline in times of slackening demand?

a. If teacher salaries do not rise (e.g., in times of slackening demand) how does that affect education? Do teachers spend less time with students as a result?

4. What methods are appropriate for coping with problems of an excess or shortage of physical plants caused by enrollment changes?

a. How can the flexibility of the district's physical plants be increased? Methods suggested for investigation include leasing, mothballing currently unneeded facilities, central (state or district consortium) ownership of moveable classrooms to be shifted according to need.

b. How have districts successfully handled school closings and surplus plants? A survey of districts in which these problems have been resolved could provide a useful manual of case histories and suggested solutions for local administrators.

c. Are year round schools a feasible alternative to increasing the investment in physical plants? Are there actual economies? What are the major considerations and problems when planning a year round operation? What have been the experiences of schools that have tried this approach?

5. What are districts' responses when operating expenses fall more slowly than revenues? What actions have districts taken? What have been the effects on the composition of the staff? How and on what bases have the decisions been made to retain, reduce or eliminate programs? Has the quality of education been affected?

6. Do state finance formulas for schools aid or hinder districts' efforts in times of declining enrollments? Increasing enrollments? How and to what effect?

7. What have successful post-secondary institutions done in adjusting to the period of slow growth, impending (if not actual) enrollment declines, and rising costs? Once again, a survey of representative institutions could provide information for a manual for administrators containing case histories and suggested solutions.

Implications of Demography and Enrollments on the Instructional Work Force

Two important issues which are related to the predicted student demographic and enrollment changes are the changing nature of the labor force in education and the economic implications that these changes may bring. Both of these topics have important and widespread policy implications. A set of research questions

which is presented below addresses both of these subjects.[2]

1. What are the relevant demographic characteristics of the instructional work force in public schools today? How are they expected to change over time under alternative assumptions about the future?

a. What are the profiles of the teachers, other instructional staff, and administrators, in terms of such demographic characteristics as age, educational preparation, professional experience, race and ethnic characteristics, and full-time or part-time status? Have these undergone significant changes over time?

b. Do the demographic characteristics of the instructional work force vary in relationship to the demographic and socioeconomic characteristics of the local communities and students and to the locational characteristics of the communities?

c. Have changes in the level of district expenditures or the structure of school funding had an impact on the structure and composition of the instructional work force?

d. What will be the demographic characteristics of the instructional work force in the future if present trends continue? Under alternative assumptions concerning economic conditions, size of pupil population, level of educational services, collective bargaining?

2. What are the financial burdens on the districts and on the states of the demographic characteristics of the instructional work force and what changes in the financial burden will result from changes in the demographic characteristics over time?

a. Has and will the changing financial burden vary systematically with district locational, demographic, and socioeconomic characteristics?

b. What have been the impacts of differences in the characteristics of the work force and employment conditions on the costs of instruction, e.g., shifts in salary schedule, mix of types of instructional personnel employed, training and experience of personnel, level of benefits provided, ratios of pupils to various categories of professional staff, program assignments of school personnel? What will be the effects of changes in these factors over time?

c. How have changes in collective bargaining status in states and school districts affected the demography and compensation of the instructional work force?

d. Will instructional salary costs require an increasing proportion of school budgets? Will certain types of programs and other expenditure items be squeezed or eliminated to meet increased salary requirements?

e. What impact have changes in fringe benefits (e.g., pension, health, welfare contributions) for school employees had on the financial burden of school districts? On the level of educational services provided by the districts?

f. What would the impacts be of various state and federal funding strategies and early retirement programs on the demography of the instructional work force, the nature of the educational programs offered, and the financial burden of the district?

Out-of-School Influences on School Performance

In his paper, "Time and How It Is Spent," Charles Benson proposes a broad investigation of the effects that different patterns of time use by children, their parents, and other significant adults in their lives have on the process of a child's growing and becoming an adult. Such studies would deal with: 1) time allocations of individual members of household and with their joint activities; 2) effects of family structure on children; and 3) interactions of time use, family structure, neighborhood characteristics, and school characteristics.

The suggested methodology is that of child time budget (CTB) studies, which is defined as the systematic investigation of children's activities and attitudes in a context of family, neighborhood, and school. A major advance in such studies is the inclusion of children themselves as data sources. In addition to the CTB approach, are there other research methodologies which may be appropriate for addressing the same issues? Similar findings from different methodologies would tend to strengthen the results.

A key issue in this area is *how* does family background (broadly defined) affect school achievement? That it does has been documented at length, but the questions of how and why the various factors which make up family background actually interact to influence school achievement are unanswered. To deal with the how and why behind the statistical relationships calls for studies investigating the activities of children and their parents in the context of data concerning family socioeconomic status (SES), family structure, neighborhood characteristics, school characteristics, and the child's school performance. The following types of questions would be examined:

1. Are there identifiable behavior and time allocation patterns of children which are related to successful and unsuccessful school performance?
   a. To what extent are children's activities related to social class?
   b. For children who have different CTB patterns than expected for the family SES, are such differences reflected in school performance? Can an examination of these outlying cases provide insight into the forces influencing school performance?
2. Through what ages and to what extent are children subject to formative influences from family and neighborhood? Do they maintain the influence after children enter school?
   a. Do children selectively accept and reject parental influences from early ages? What are the general patterns of influence versus independence? How do they change over time? Are there important differences by social class?
3. What are the important measures of family structure and do they have a significant impact on the child's school performance? Such characteristics would be investigated as family composition and size, birth order of child, education of parents, number of parents working outside the home, discipline, parent attitudes toward time use and achievement, level of family activities.
4. How do family structure variables interact with those of the family SES and neighborhood (physical environment, social environment, types and availability of public resources) to influence the child's development?

5. How do family structure and SES affect parents' time available to children?

6. What are the impacts of parental actions with their chidren?

a. Are child-parent interactions related to social class?

b. Can specific types of interactions overcome the negative effects of SES and class on school performance? What are these interactions and the conditions under which they are likely to be effective?

c. What do parents of successful students do differently with their children than parents of unsuccessful students?

d. What are the dynamics of parental involvement with their children, i.e., extent of involvement, type of involvement, and motivation for involvement? How are these related to children's mental and social development? To school performance?

e. Are there differences in the quality of the parents' time spent with children? How can this be measured? Does the quality of time make a difference to child development and school performance? Is the quality of time related to the activity involved?

f. Is there a tradeoff made by parents between the time they spend with children and the material goods they provide them? Does this vary by child within a family? Does this tradeoff make a difference in the child's development and school performance? Are there social class differences in these patterns?

g. Does the productivity (efficient development of the child's potential) of the mother, father, or significant other adult vary with their educational level? What are the reasons for this? Do these differences vary by social class?

h. Do parent training programs have a positive effect on the children's school performance? Are the time allocation and behavior patterns of parents and children different before and after the training?

7. Are there mutually reinforcing patterns of child activity and of child-parent interactions that eliminate or reduce the effect of social class on school performance?

a. What barriers do certain children and parents face in trying to achieve more effective school performance?

b. Why are changes in behavior patterns productive or unproductive in certain situations for certain children and parents?

8. What are the influences of other adults (beyond parents and teachers) on the lives of children? Who are these other adults? Are there systematic differences in the importance of other adults across social class and neighborhood?

The separate influence of the school on children should also be investigated as it interacts with the important aspects of the family and neighborhood characteristics.

1. Are there practices, procedures, or policies of the schools which hamper or nullify parental efforts for their children's improvement? What are their nature and how do they operate? Are they different for different social classes and neighborhoods?

2. What are the relationships between successful teachers and schools and the family and neighborhood characteristics of their students? Can and do successful teachers and schools stimulate productive changes in the families and neighborhoods of their students? Are there differences across social classes?

A general research methodology suggestion which can apply across many of the specific studies and questions suggested above should be noted. There are many "experiments" in education (e.g., Early Childhood Education program in California, Title I projects) which provide opportunities to conduct longitudinal studies on the impact of different out-of-school experiences and characteristics. Such studies can utilize the CTB and other methodologies to add the dimension of out-of-school influences on school performance.

As the author acknowledges, the focus of the research is on influences which improve school performance, an essentially middle class goal. There may be other, and perhaps more appropriate, measures to certain groups of children and parents. Research along this theme would question whether the schools and their programs, as currently structured, are legitimate and appropriate, whether the purpose of out-of-school activities is to enable children to do better in school, and whether there are not different goals which are more important for certain social classes.

Relationship Between Schooling and Work

In his paper, "Schooling and Work: The Changing Context of Education," Norton Grubb questions the validity of the human capital model as an adequate explanation of the relationship between education and economic growth. Alternative interpretations are proposed and discussed to explain the social productivity of schooling. In particular, three primary issues are analyzed—the "over-educated American;" credentialing; and career education.

The concept of the "over-educated American" suggests that too many individuals are attaining levels of education, especially college and graduate degrees, which are too high for the available job market. A primary focus of research in this area would be the testing of alternative explanations of the "over-educated" situation for credibility and explanatory power, with particular emphasis on the current problems of absorbing college graduates into the labor market. Four alternatives are suggested for investigation and development.

1. Imperfections in the human capital model.

a. Persistent habit model. This alternative would investigate the magnitude of response of individuals to rates of return to college attendance. Has the social meaning of college become its consumption value, and not a rational economic investment in upward mobility? If so, will this result in either a continued depressed rate of return for college attendance or the establishment of a "cooling out" mechanism to reduce attendance (mostly among working and lower class youth)?

b. Defensive strategy hypothesis. The class specific responses to rate of return for college would be examined in this research. Have middle class youth continued to attend college, and will they continue to, even with depressed returns, while lower class youth do not?

2. Demographic causes.

Under this alternative, the baby boom is seen as the primary cause of the over-supply of college graduates. The problem of over-education is then associated with this cohort and its impact on the economy. In dealing with policy responses based on this explanation, several important questions require investigation.

a. Can workers of different ages be substituted for one another? What is the substitutability among older, more experienced workers, and younger, more educated workers? How will productivity be affected? Wages?

b. Are birth rates a function of labor market conditions? This is a longer term question which will require the integration of birth rate changes with other economic and social cycles and trends.

3. Credentialing.

Under this explanation, education is used to sort individuals among jobs and has little relationship with the actual job requirements. Has there been an inflation of the educational requirements for jobs? This will require an analysis over time of the educational requirements for representative jobs vs. the actual content of the jobs themselves. If the educational requirements for jobs have increased, has there been a corresponding increase in productivity from the more educated workers?

4. Degradation of labor.

The basic assumptions of this alternative are that: 1) with increased technological changes jobs become more specialized/fragmented; 2) relatively fewer skilled workers will be required; 3) control is centralized in the hands of a few skilled managers; and 4) most workers will have reduced responsibility versus their expectations from college attendance. All of these assumptions are testable and should be investigated.

On the other hand there are small but noticeable counter-trends toward labor-intensive methods of production (e.g., E. F. Schumacher, *Small Is Beautiful*), worker control, and new patterns of job sharing. How might these affect the nature of work and workers' education and employment expectations?

A fundamental aspect of the relationship between schooling and work is the question of how selection in the labor market is related to education. As Grubb describes, "both the theoretical and empirical work on the use of schooling to sort individuals in the labor market are as yet underdeveloped. The various explanations ought to be more precisely formulated, and empirical tests need to be clear about the rationale they are describing. In many cases the data necessary to test theories of signaling and credentialing must come from small individual work settings, with correspondingly high costs of data collection. The

possibilities for utilizing readily accessible national data, such as census data, appear to be rather limited, though additional research effort should go toward utilization of existing sources."

Several specific research suggestions in this area are identified and presented below.

1. Are the models of schools as sorters validated in actual practice? How and on what do employers base their employment selection?

2. Are there alternative signaling methods that could convey the same or better information to employers at a reduced cost?

3. What would be the distribution of the costs of education if based on the distribution of benefits inherent in the signaling models?

4. Are the inefficient uses of schooling as a sorting device less prevalent in the competitive sectors of the economy? Have some firms found that they can operate more profitably and competitively by not using excessive levels of education?

5. Conversely, are large bureaucratic organizations sheltered from market forces more likely to exhibit a non-rational utilization of schooling in their employees?

6. Does monopoly power on the employee side of the labor market, e.g., medicine, law, education, lead to the inflation of schooling requirements?

7. Does schooling perpetuate the class structure in the labor market? Such an analysis would require occupational and job status analyses by class and successive generations.

The final major topic addressed by Grubb is that of career education. It is defined as "the complete reorientation of the curriculum around occupations—paid employment." The basic focus of the research suggested for this area is to validate or deny the empirical assumptions and expectations of career education. This is to be done through a comparison of career education and the more traditional educational programs. The first step required for such an evaluation effort is to increase the specificity of what is meant by career education and to refine its objectives. Following this, four specific areas of research are suggested:

1. Do career education programs make discernable differences to students in them, compared with non-career education programs?

This research would, in effect, test the claims made by career educators on behalf of their programs. Areas of comparison would include: traditional academic skills; information about labor markets; attitudes toward schooling, work, and social institutions; college attendance decisions; careers of students, including logical job sequences; minority and handicapped students; unemployment experiences; job satisfaction; leisure time use; and adjustment to family life.

Such a research effort would require longitudinal data, an adequate control group, and an initial non-bias in the students who are in the career education and non-career education programs.

2. Does career education reduce the "mis-match" of workers and jobs by providing more precise information and training for those jobs which are available?

Research into this topic must first clarify the magnitude of these "mis-matches" and determine if, indeed, they constitute a serious and widespread problem. Next, the reasons for such "mis-matches" should be specified, e.g., lack of labor market information, training bottlenecks, regional patterns of supply and demand. Finally, this research effort would identify satisfactory programs to reduce or eliminate the "mis-matches."

3. Does career education reinforce the tendency in education to track individuals by social class, race, and sex?

The research in this area would analyze both the content of the curriculum and the composition of the student population in career education programs for evidence of stereotype tracking by class, race, or sex. Particular emphasis should be placed on junior colleges due to their large number of career education programs and the potential use of "cooling-out" mechanisms which may impact differentially across class, race, and sex.

4. Is career education an appropriate effort?

This research effort should be linked to those in signaling and credentialing. If schools function primarily as sorting devices, then career education programs are likely to have little effect. Different individuals may gain or lose, but the aggregate productivity of the labor force would be unaffected.

Recurrent Education

In his paper, "Recurrent Education and Employment," David Stern is concerned with the relationship between recurrent education and over-education. He focuses first on the potential for work-related recurrent education to reduce both underemployment and unemployment. Then, a possible mechanism for financing recurrent education is discussed. The research suggestions concerning recurrent education are grouped according to these two areas.

Prior to an investigation of the effects of implementation strategies, however, it is necessary to sharpen the basic understanding of recurrent education itself. A definition of recurrent education is provided by Stern, although the broad and sometimes vague use of the concept is also noted. Specification of an agreed upon definition of recurrent education (Stern's or another), including the programs and activities to be included, the objectives, and the means of measuring the achievement or lack of it, is a necessary initial step in a comprehensive and cohesive research program. Stern also poses a fundamental question concerning the role of recurrent education in the educational system.

> "How would recurrent education fit into the traditional life pattern of first education, then work, then leisure? Would it redistribute the same number of years of education over the average person's lifetime? Or would it add extra years of schooling throughout a person's life instead of only at the beginning?"

With the basic definitions established, an examination of the current and potential impact of recurrent education on work and productivity can be carried out systematically. The research suggestions in this area range from fundamental inquiries into the relationship between learning and work to specific questions concerning the effects of recurrent education on worker output.

1. As a base from which to proceed, Stern proposes a set of research questions to investigate the nature of work-related learning. With the answers to these questions, it will be possible to design and target recurrent education programs more effectively. The studies suggested below would concentrate on organizations where workers are already motivated to learn and to use their learning to become more productive.

a. How can the amount of job-learning that takes place in actual work settings be measured? To what extent can the written and practical tests presently administered in schools and training programs be adapted to measure learning in particular jobs?

b. Among similar work organizations, what is the relationship between rate of job-learning, rate of profit (or other output measure), and change in number of employees?

c. What characteristics of organizations are most strongly associated with job-learning?

d. How is job-learning distributed among employees by age, sex, and level in the organizational hierarchy?

e. What are the mechanisms for job learning? What is the importance of formal on-the-job training compared to informal exchanges and guidance?

f. How much does job-learning interfere with job doing? Does job-learning reduce output and wages? Are learning and doing sometimes the result of the same activity?

g. To the extent that the learning applies only to a specific job, how is the payoff divided between employer and employee? What factors determine the ou[redacted]?

h. What are the attitudes of employees toward job-learning? What characteristics of the employees and of work organizations influence these attitudes?

i. Do any organizations provide formal or informal incentives for job-teaching? What are these incentives and have they been effective?

2. Does or can recurrent education increase employee motivation and productivity? What is the magnitude of this effect, if any?

a. Can educational experiences improve worker interest in work without changing the nature of the work?

b. Can recurrent education change unfavorable attitudes toward work?

c. Will an increase in worker motivation result in an increase in productivity? Will output rise faster than real wages following recurrent education?

d. Can recurrent education offset productivity declines by compensating for the obsolescence and depreciation of knowledge in workers?

3. Is there a need or desire for recurrent education programs? If so, what types of programs and of what magnitudes?

a. Is there evidence that significant numbers of people want or need to "stop out" for education in mid-career?

b. What are the motivations or incentives for recurrent education?

c. Why is there such low utilization of the existing systems and opportunities in the United States? In other countries with established sabbatical leave programs for employees, e.g., France and Germany?

d. Is the degree of worker control of the work place related to the demand or need for recurrent education among the workers involved? Does "industrial democracy," i.e., greater exercise of authority at lower levels, lead to an increased utilization of recurrent education by workers?

e. Are schools attractive mechanisms for recurrent education programs or do they discourage people from participating?

f. Are there alternatives in adult education without having to interrupt a career?

g. Is there a significant demand for recurrent education which is not work or career related, but is for consumption benefits and personal development? What is the nature of this and how can it be met?

4. Will recurrent education affect worker substitutability? Will it make possible greater substitution of retrained older workers for younger workers?

a. Are employers willing to hire older retrained workers for entry level jobs?

b. Are older workers willing to accept entry level jobs?

c. Can the retrained workers be absorbed by the labor market if significant numbers of workers utilize recurrent education opportunities?

5. Can recurrent education prevent inflationary rises due to labor skill shortages?

a. Is it possible to use recurrent education as a tool for avoiding labor "bottlenecks?"

b. Can the training be accomplished quickly enough and in the necessary locations to relieve labor shortages? Or will the response via recurrent education be too slow and instead create more overeducation and underemployment?

6. Are there alternative work organizations which can be structured to maximize or increase the educational value of work for the people involved? Such research would probably involve experimentation in specially created, laboratory organizations.

Borrowing against their Social Security or private pensions is a proposed mechanism for individuals to finance their recurrent education programs. A number of research questions arise concerning the means and effects of financing recurrent education.

1. What financial incentives are necessary to cause a worker to utilize recurrent education?

a. Will the proposed Social Security funding amounts be sufficient incentive or support for workers to use recurrent education?

b. What level of financial support would be required to attract workers to recurrent education? How is the demand for recurrent education related to the financial support provided?

2. What are the costs of recurrent education to the individual (private costs)? To the employer? To society?

a. Do they equal or exceed the economic benefits derived from recurrent benefits?

3. What are the potential and likely magnitudes of workers who might use this option?

a. What would the impact be on the Social Security system of such utilization?

4. What parallel changes in retirement laws, pension plans, and related items would be required to utilize the Social Security system as suggested?

5. What are alternative entitlements and other financial mechanisms which might be used to achieve the same objectives as the use of the Social Security system?

a. How do they compare with the proposed system in terms of: fiscal impact; political constraints; likelihood and magnitude of potential use; types of programs utilized; characteristics of users; and learning results?

Conclusion

As is evident from the preceding research suggestions and the papers on which they are based, there exists an extensive agenda for research in educational finance and organization. While no claim is made that the research questions are exhaustive, the editors have been careful to insure that many major policy issues with which American education will have to deal over the next ten to twenty years have been covered in this effort. This began with the determination of the specific topics for the papers themselves, continued in the use of a seminar format to provide the individual authors with an opportunity to test their ideas and concepts, and concluded with the selection of the specific research suggestions to include in the agenda.

Consequently, we believe that the agenda presented is challenging, but at the same time urgent. To work successfully in dealing with the pressing issues at the national, state, and local levels, education must expand the present knowledge base along both theoretical and empirical dimensions. It is to this purpose, then, that the research agenda has been developed.

References

1. These suggestions are derived from Michael W. Kirst, "Impact of School Finance Reform on High Wealth, High Expenditure Suburban School Districts," paper prepared for the Ford Foundation, September, 1977.

2. These questions draw heavily upon a proposal submitted to the California State Department of Education by Jay Chambers and Susan Sargen, "The Demographics of the Instructional Work Force: Economic Impact and Educational Implications," July, 1977.