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The ECIEL Study of Household Income and Consumption in Urban Latin America

An Analytical History

Philip Musgrove

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The Living Standards Measurement Study (LSMS) was established by the World Bank in 1980 to explore ways of improving the type and quality of household data collected by Third World statistical offices. Its goal is to foster increased use of household data as a basis for policy decision making. Specifically, the LSMS is working to develop new methods to monitor progress in raising levels of living, to identify the consequences for households of past and proposed government policies, and to improve communications between survey statisticians, analysts, and policy makers.

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The ECIEL Study of Household Income and Consumption in Urban Latin America

An Analytical History

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The ECIEL study was the result of collaboration among a large number of investigators in member institutes in ten South American countries and a co-ordinating staff at the Brookings Institution in Washington, D.C., over almost a decade. I have not been able to draw upon the recollections and evaluations of all those individuals. I have, however, had the generous and valuable help of the founder and first General Coordinator of ECIEL, Joseph Grunwald, in assembling this history. He also provided extensive comments on the first draft of the paper, and discussed them with me at length. My fellow Technical Coordinators, Howard J. Howe and Arturo C. Meyer, were similarly generous with their time and suggestions. I have also to thank Marcia Mason Cook, for several years the project's chief computer programmer, for insights and recommendations. Ms. Cook, Robert Ferber and William McGreevey commented in detail on the first draft, and Nancy Birdsall and Christiaan Grootaert of the Bank offered shorter comments.

These friends and former colleagues are of course not responsible for the opinions expressed in this report; neither are the staff and participants of the ECIEL Program, the officers, trustees or staff of the Brookings Institution, nor the World Bank. Any errors of fact or judgement are mine alone.

THE ECIEL STUDY OF HOUSEHOLD INCOME AND CONSUMPTION IN URBAN LATIN AMERICA: AN ANALYTICAL HISTORY

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I. INTRODUCTION TO THE ECIEL STUDY

The ECIEL Program of Joint Studies on Latin American Economic Integration is a non-profit, apolitical, independent organization which brings together a larger number of public and private research and statistical institutions to study economic issues in Latin America. (The acronym ECIEL stems from the Program's Spanish name, Estudios Conjuntos sobre Integración Económica Latinoamericana, which was adopted by the participants in 1966). The Program was founded early in 1963, with the participation of three Latin American institutions -- the Instituto Torcuato Di Tella of Argentina, the Fundação Getulio Vargas of Brazil, and the Universidad de Chile -- and its component studies were coordinated by the Brookings Institution. After eleven years under this arrangement, during which the Program expanded to include more than two dozen Latin American institutions and to undertake four major research projects, the coordination was transferred to a newly-created, autonomous institution in Rio de Janeiro, Brazil, in 1974. 1/

During a period of almost ten years, the ECIEL Program conducted, as one of its projects, a study of household income and consumption based on original surveys carried out during six years in eighteen cities of ten South American countries. 2/ This was the first time anywhere in the world that parallel

1/ A description of the Program's objectives, operation and organization is included in every substantial publication resulting from an ECIEL project. The earliest published explanation of ECIEL is Grunwald (1966); later a pamphlet (ECIEL, 1972) was prepared for widespread distribution, and this has been repeatedly revised and is now available in the Program's three languages, Spanish, Portuguese and English. It contains some factual errors as to ECIEL's history, but describes accurately the Program's objectives, structure and operations.

2/ Argentina (Buenos Aires), Bolivia (La Paz and Cochabamba), Brazil, (Rio de Janeiro, Recife and Porto Alegre), Colombia, (Bogota, Barranquilla, Cali and Medellin), Chile, (Santiago), Ecuador (Quito and Guayaquil), Paraguay, (Asuncion), Peru (Lima), Urugay (Montevideo) and Venezuela (Caracas and Maracaibo).

national studies of this nature were conducted through the collaboration of a number of independent statistical and research institutions, with a common purpose, organization and set of procedures. Any future effort to conduct studies with similar objectives should benefit from the experience of the ECIEL project. This paper attempts to describe and evaluate that experience, to provide an analytical history of the study. It was prepared specifically for the use of the Living Standards Measurement Study, recently launched by the Development Research Center of the World Bank, but it is hoped that its value will extend to other institutions and researchers.

The substantive results of the ECIEL consumption study, as it was usually called, have been presented in a series of books, articles, monographs and working papers. These are listed in the bibliography, and their contents are not discussed here. Many of the methodological choices and innovations adopted in the study have also been published. This paper emphasises not what was learned substantively or methodologically, but how the project was organized, what its objectives were, how well they were met, and how a similar effort could be made more successful or efficient. The remainder of Section I describes the origin, history and conduct of the project, and discussed the degree of relevance of the ECIEL experience to other research. Section II is devoted to the principal objectives of the project and the kinds of choices that had to be made in pursuing them. It also describes the political context, and its interaction with technical objectives. Specific problems arising in the study, whether political, technical or organizational in origin, are treated in Section III. An effort is made to evaluate how successful ECIEL was in responding to those problems, and to draw some conclusions regarding optimal choices or policies. Finally, Section IV summarizes the principal findings of the paper, and offers some recommendations for comparable research efforts.

A. Origin

The ECIEL Program undertook as its first project a study of optimum locations of several industries on the assumptions of cost minimization and free trade among the LAFTA (Latin American Free Trade Association) countries. The substantive results are available in Carnoy (1972 and 1970); the project partly followed a previous study on trade patterns (Baerresen, Carnoy and Grunwald, 1965). Work on this project required estimates and projections of demand for those industries' products, and incidentally revealed how little was known about the structure of consumption in Latin America. As a result of discussion begun in 1965, the ECIEL Program decided to include a detailed study of consumption in its next round of research rather than continuing to stress investment. Since it quickly became evident that data of the sort required were available in few if any countries, and consequently, that new household surveys would have to be conducted, the project dropped from consideration all non-household components of final demand. Initially, it had been hoped that governments would provide information on those components, but while some agreements on principle were reached, no data were actually collected. A further restriction, to major urban areas, was imposed by the anticipated costs and difficulties of budget surveys in rural areas. 1/

The earliest stages of the project -- characterized partially by the collection of data presumed needed for other types of studies -- had an unrealistic, 'textbook' air. There was very little perception of how large and how difficult the project would eventually prove to be. This naivete may, of course, have been an essential condition for launching the study. Nonetheless, within a year, the

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1/ Some years later, rural income and consumption surveys were conducted in several countries of the region, but none had been executed at the time of the ECIEL Surveys.

project took on a life and justification of its own: it detached itself from the initial industrial-integration orientation of ECIEL as well as from a study of price comparisons to which it was originally linked, although the consumption data continued to supply the weights for price indexes. 1/ As the project evolved, the structure of consumption itself became less important as a topic of study, and attention shifted steadily to the determinants of consumption levels and living standards, and particularly to the level and distribution of household incomes.

B. Organization of the Study

The household budget study was organized in essentially the same way as other ECIEL projects. In each country represented, one or more institutions dedicated to statistics and/or economic research were members of the Program. Sometimes an institute which was already an ECIEL member took responsibility for the project but, because of resources too limited for large-scale fieldwork, depended for the actual collection of data on a public statistical agency which was not a member of the Program. 2/ In other cases, a primarily statistical agency was expected to also provide economic analysis (as in Ecuador and Caracas), or an academic agency had to conduct the fieldwork (as in Peru, Bolivia and Bogota) or contract it to other institutions (in smaller Colombian cities and Maracaibo, Venezuela). Very few institutions initiated the project with both

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1/ Apart from this companion ECIEL study, the consumption study was linked to price indexes since, in several countries (Chile, Ecuador, Venezuela), the surveys were undertaken by public institutions precisely in order to revise the existing indexes.

2/ This division of labor between data-gathering and analytical institutions was peculiar to the consumption study. While, in principle, it combined the strengths of both kinds of institutions, it left the project vulnerable to decisions or problems arising in the statistical institutes, over which ECIEL had little influence. This problem is discussed further in Section III A.

kinds of capacity: the Fundação Getulio Vargas in Brazil was probably the best equiped for all phases of the study. The difficulty either of finding a single agency with all the desired skills, or of organizing collaboration between two institutes which might have very different objectives and strengths, affected several of the principal dimensions of the project. Two of these are discussed extensively in the following section: the degree of decentralization in Section II B, and the balance between research and training in Section II C.

The Program employed a staff, housed at the Brookings Institution in Washington, D.C., for the life of the project, to be responsible for all countries and to coordinate the work of participating institutes, each responsible for one country. 1/ At first, the project was organized by the Coordinator General with consultant help; later, the staff grew to include a Technical Coordinator, research assistants, computer programmers and secretaries. The institutes could hire consultants for their own purposes, but the Coordination accounted for most of their use. The lack of prior experience and expertise on behalf of the Coordination was compensated for by retaining as consultant to the project Robert Ferber, Director of the Survey Research Laboratory at the University of Illinois.

In principle, the Coordination had four separate types of responsibility. One, exercised at the level of the Program's General Coordinator, was to acquire funds necessary to finance the Coordination's work as well as the institutes when their own resources were inadequate. This was particularly important for the large expense of fieldwork when undertaken by private academic institutions. The job of persuading public agencies to collaborate in and contribute resources to the project was part of this task. The second responsibility was to help the

1/ When the ECIEL Program Coordination was transferred to Rio de Janeiro, Brazil, in 1974, the coordination of the consumption study remained at Brookings.

institutes carry out their data collection and research, supplying technical expertise or particular resources as needed. A third and very important task, given ECIEL's orientation toward comparable, methodologically uniform parallel studies in different countries, was to assure that the institutes' work followed a common plan and procedures. This task especially justified the name "Coordination" and distinguished it from being simply a financial and consulting service for the institutes. Finally, the Coordination was responsible for international analyses or comparisons involving all the countries represented in the project. These last three responsibilities define major dimensions of the project, and are discussed more thoroughly in Section II.

The Coordination was more nearly a creation of the institutes than the reverse. It did not control any of the institutes nor was it responsible for the establishment of offices or operations in any country for purposes of collecting data itself, etc. As the project evolved, the Coordination became a custodian for most of the data collected by the institutes, who continued to be the owners of their information and the final judges of its uses or dissemination.

Contact between the Coordination and the institutes took three forms in addition to steady correspondence. Throughout the project, seminars were held approximately every six months: these typically lasted one week, were held at one of the participating institutes and were attended by one or more researchers from each institute in the project. During most of the consumption study's life, all ECIEL project seminars were held together in order to help unify the Program as a whole, discuss matters of interest to all member institutes, and allow the planning of new projects. This constituted the only formal mechanism

among the institutes for agreement on common procedures. 1/ The discussions at the seminars and working papers prepared before or during the meeting were published regularly in documents called Resumen del Seminario, which were available to participants as well as interested outsiders. With the exception of some discussions of ECIEL's own organization or operation, seminar meetings, in particular, were always open to interested observers. 2/

The second form of contact was in the form of visits by the Technical Coordinator (or a consultant, or in some cases a computer programmer) to one or more institutes. These visits were usually brief, from a few days to two weeks per institute, and were devoted almost entirely to helping solve local problems in the study. Such visits were most important in the early stages of data collection and preparation, but were also valuable when data or analyses were returned to the institute and needed to be discussed with more people than could assemble at the seminars. In retrospect, it would probably have been wise to assure that each country was visited at least once every three months (between seminars). This would have reduced the frequent problem of seminar participants appearing to understand a technical decision or procedure but then misapplying it or being unable to apply it one or two months later, and subsequently, letting the problem wait until the next seminar. Later in the project two features tended to diminish the importance of these visits: it became common practice for the data processing to be transferred to the Coordination, and - largely in consequence - it was sometimes advisable for the direction of the visit to be reversed.

1/ Issues not settled by discussion at a seminar were usually referred to the Coordination for a proposed solution which would be distributed to the institutes for acceptance, rejection or modification at the next seminar meeting. This mechanism is discussed further in Section IIB.

2/ The reasons why this was politically as well as academically advisable are treated in Section IID.

Visits by institute researchers to the Coordinating Center were the third means of contact. As above, such visits were designed to deal with problems in one country's study only; on the other hand, however, they typically lasted several weeks or months. This was possible when the data were brought to Washington, because, in effect, all work was transferred to the Coordinating Center; no researcher was prevented from working on the project at home. The process was further improved by the specialization of the Coordination's research assistants on particular countries so that when institute participants arrived, there was someone ready to work with them full time. This arrangement, characterized by greater specialization and longer intervals, helped accomplish more than visits by the Coordinator to the institute. Of the six countries for which the project was completed essentially as planned, three sent researchers to the Coordination (as did a fourth institute which finished most of the project on its own). In all these cases the visits were extremely helpful, resolving questions that would have taken much longer to answer through correspondence, if they could be settled at all.

C. Chronological History

The Program's initial objective was for the institutes to conduct the surveys simultaneously, and then proceed more or less in step through the remainder of the project. The actual schedule, however, was much more varied, 1) because the surveys were ultimately spread out over several years, and 2) because the institutes had problems of differing severity and very different capacities for solving them. The length of time from the beginning of fieldwork to the completion of a set of standard analyses was as short as three years (in Colombia; longer if allowance is made for subsequent recalculation of some of the

analyses), and as long as seven years (in Caracas, Venezuela). The project as defined by those standard results was completed in mid-1974, about eight years after it was launched by ECIEL; however, the data continued to be used for a variety of related analyses over the next few years, and many of these results can also be regarded as part of the project. In that more inclusive sense, the study was completed with the publication of a comparative five-country analysis (Musgrove, 1978b). By that time the ECIEL Program had proceeded to a study of income distribution. Although initially conceived as a sequel to the consumption study to be based on the same data, it had evolved into a very different project by 1978.

Logically, the project can be separated into distinct stages, each required to end before the next may begin. The first stage entails the choice of the two key elements in the fieldwork, the sample design and questionnaire, i.e., the structure of observations and the structure of variables. Second is the actual data collection, including field-checking of the data and any return visits to households. The third stage involves the organization of a data file with transcription from the original questionnaires, followed by a fourth phase of "cleaning" the data or detecting and removing mechanical, logical and statistical errors. A fifth stage of testing the sample as a whole and weighting it as necessary to represent the population precedes the sixth and final stage of economic analysis, the production of final results.

Basically, this sequence was followed for every country in which the project was carried through to final results. Upon studying the history of the project as recorded in the seminar Resumenes, however, one is struck by two respects in which the discussion and decision-making among project participants followed a much less regular sequence. First, different stages received very different levels of discussion and planning; and second, they were often

considered out of sequence, some of them being "settled" well before actual work on them had begun, while others were discussed for the first time only after unexpected problems arose. Both these discrepancies between the logical sequence and actual conduct of the study had significant consequences for the costs incurred and problems faced in the project; yet this does not mean that it would have been possible to follow exactly the planned sequence under any realistic circumstances.

Regarding the first discrepancy, the stages of fieldwork and of data cleaning were relatively under-discussed by the group of participants as a whole. The former stage was left largely to the institutes, with some essential help from the Coordination - particularly through consultant visits to all the institutes in 1966-67 - but with little comparative review; the latter, on the other hand, was left largely to the Coordination to design, the actual implementation being carried out between the Coordination and each institute separately. 1/ In contrast, the stages of defining variables and specifying analyses were perhaps over-emphasized; the latter point is considered at more length in Section IIID.

With regard to the second discrepancy mentioned above the most notable aspect is the very early - perhaps premature - attention given to specifying hypotheses and defining calculations to be performed. This was largely in response to pressure from the Brookings Institution and an outside review committee to specify what the project was intended to learn or establish. The committee assumed that data should be collected in pursuit of particular hypotheses rather than for the general purpose of acquiring knowledge of income and consumption. Much discussion was given over to this issue before any systematic attention was paid to problems of data cleaning and adjustment. The

1/ Data-cleaning here specifically excludes the stage, discussed at some length, of analyzing and adjusting the sample.

latter issues, and those involved in harmonizing the different data sources, were forced on the participants by unexpected difficulties that only became apparent once the data were in hand.

As a result of both variations in the schedules of the different institutes and of the need to consider topics in sequential order throughout the project, discussion at a seminar could never be limited to a single stage of the work. There was, however, a definite progression of emphasis over time. The fieldwork was mostly completed between mid-1967 and late 1969; one survey was taken late in 1966, and one institute finished in 1970 and another in 1971. The last fieldwork ended in 1972, but that was due to the unique circumstance of adding another city and institute to the country study long after the project had begun. During the seminars of 1969-71, the participants focused attention on problems of matching data definitions among countries and on starting to define the final tabular and econometric results. The sample structure - a complex design which could be simplified in several different ways by eliminating interview waves or subsamples, or changing the composition of the sample by economic stratum - was defined by 1968; but the closely related question of how to test such a sample for representativity, differential mortality, respondent conditioning and similar problems was not taken up until 1971, and not fully resolved until 1973, 1/ after most of the choices had been made concerning which final results to produce. The sample design and interpretation were the subjects of several discussions among the Coordination, outside consultants, and the U.S. Bureau of Labor Statistics, with which ECIEL exchanged much information. One early methodological paper (Musgrove, 1972) resulted from this interest. From 1972 through 1974, the emphasis in seminar discussions and

1/ The sample design (and country-specific variants) and the criteria and techniques of sample analysis are discussed in detail in Musgrove (1978b), Apendix A.

working papers shifted away from the consumption study, as such, to other possible projects to be based on the data, such as studies of income distribution or of saving behavior. After 1974, the only discussion related to the consumption study concerned the possibility of completing and publishing studies for those countries (Argentina, Bolivia, Brazil and Paraguay) not included in the comparative study published in 1978.

D. Publications and Uses of the Data

The published output of the study was originally intended to consist of a series of chapters organized by nation and consolidated in one or more volumes through a comparative analysis prepared by the Coordination. Eventually, however, a single comparative volume was published (Musgrove 1978b), with no national contributions, each institute being left to publish the results for its country. A version of one chapter of the comparative study and an extension of part of another chapter were also published separately (Musgrove 1977 and 1978a). The Colombian national study was the first to be published (Prieto, 1971a), with two subsequent derivatives also prepared (Prieto, 1971b and 1974). The Peruvian results were the next to be completed (Figueroa, 1974), followed by those for Venezuela (Fernandez and de Khan, 1974). The Chilean data and some preliminary results were described in an early publication (Dirección de Estadística y Censos, 1970), while the substantive results were disseminated later in three papers (Chaigneau, 1975; Chaigneau and Szalachman, 1977 and 1978). All of the above papers were completed before the publication of the Coordination's Comparative Study. No national studies were published for Argentina, Bolivia, Ecuador and Paraguay; a number of publications have been based on the Brazilian data. 1/

1/ The reasons for failure to produce a published national study differ among countries and are discussed in Sections IID and IIIA. The bibliography is incomplete for Brazilian studies.

The detailed procedures used in the study are mostly reported in the Resumenes, and not published separately, with the exception of Musgrove (1974a) which covers one of the data-cleaning techniques. Three methodological papers evaluating the study's procedures and choices have also been published: One (Howe and Villaveces, 1974) discusses the data-preparation sequence, a second evaluates the sample design (Musgrove, 1972), and the third offers recommendations for the kinds of statistical results which should be sought early in a budget study (Musgrove 1976).

Several analyses have used the ECIEL consumption study data to study either expenditure on particular categories such as food (Arellano, 1975 and 1977), beef (de Rubinstein and Nores, 1979) or education (Musgrove, 1978c). Others have included all or nearly all expenditure categories, but used different regression models than those of the standard ECIEL analysis, either for one country (Roldán, 1974; Rodríguez and Gómez, 1977) or for several (Howe and Musgrove, 1977).

ECIEL organized a conference, held at the Institut fur Ibero-Amerika Kunde in Hamburg, West Germany, in October 1973, for which several papers based on the consumption study were prepared by participants in the Program; some of these were published separately afterwards (Figueroa and Weisskoff, 1976; Meyer, 1980; Musgrove, 1979). Other papers were written by outside investigators invited to use the ECIEL household budget data (Cline, 1980; Crockett and Friend, 1980). The complete conference proceedings were edited by Ferber (1980), and all the papers based on this study were also published in Spanish in the Program's journal, Ensayos ECIEL.

As indicated above, the completion of the consumption study was followed by the preparation of a study of income distribution. The study, eventually

defined and conducted in 1978-80, was organized very differently from the consumption study, involving four institutes with distinct projects, methods and data sources. Two of the national studies, however, were based partly (Carrizosa, 1980) or entirely (Diéguez and Petrocolla, 1979) on the ECIEL household budget data. Prior to these studies, a number of publications used the data to examine the distribution of income (Calvo and Fields, 1978; Ferber, 1976; Fernández and de Khan, 1975; Figueroa and Suárez, 1976; Peñaranda, 1976; Uthoff, 1978) or the closely related question of the definition, extent and correlates of poverty (Ferber and Musgrove, 1978; Musgrove, 1980a, 1980b and 1980d; Musgrove and Ferber 1979).

Four other kinds of uses have been made of the data to-date, in addition to the papers based (almost) entirely on them and written or commissioned by the project coordinators and participants. First, the data have been employed in two other ECIEL projects as weights for indexes in the study of prices and purchasing power, and as a source of information on the determinants of schooling in a study of education. Second are studies by participants in the project combining the ECIEL consumption data with sources of information about other sectors or different points in time (Figueroa, 1974b and 1980; Musgrove, 1980c). Third, the data have provided material for doctoral theses, at least six of which have already been completed (Birdsall, 1979; Hill, 1978; Howe, 1974; Musgrove 1974; Lamas, 1981; and Sangumetty, 1980). 1/ Where the authors were not associated with the Program, they were provided with copies of the data for the country studied (Colombia, Peru or Brazil). Finally, on several occasions outside researchers have contracted for specific calculations based on the data; in these cases ECIEL has simply sold results at cost and not prepared analyses

1/ One other (Teixeira, 1978) may have been completed by now.

of them. The users have been the World Bank (Altimir, 1978; Berry, 1977; Bowman and Schiefelbein, 1980; Selowsky, 1978), the United Nations Fund for Population Activities (Todaro, 1978) and the Center for Research in Tropical Agriculture (de Rubinstein and Nores, 1979; Lynam, 1980).

II. PRINCIPAL DIMENSIONS OF CHOICE

Before undertaking a detailed discussion of specific problems and solutions, and the lessons to be drawn from them, it will be useful to consider the principal choices among competing objectives or approaches facing the ECIEL Study. Each of the topics treated in this section can be viewed as one dimension of the project on which an optimum position had to be sought. These choices would have existed even if many detailed problems could have been avoided or more easily solved; moreover, the same or similar options must be faced in any similar undertaking. The optimal choices are, however, neither very easy to define, nor entirely independent of one another, nor always stable over time.

A. International Comparability vs. National Studies

As is evident from discussion in Section I, the ECIEL project aimed from the start for internationally uniform data and results. Comparable analysis of existing, independent national studies was considered only briefly when it was hoped that enough surveys already existed that ECIEL could dispense with new data collection. This decision did not rule out the incorporation of one survey undertaken before the project began (that for Caracas, in 1966), and it was hoped to incorporate one subsequent independent study (Mexico, 1968); in general, however, great efforts were made to harmonize the data, to subject them to exactly the same sequence of tests and calculations and to emphasize cross-country analyses. This objective initially depended on the expectation that

the household budget data would be used directly in other international studies where uniformity would be essential, but the purpose quickly became that of assuring that any analytical results - budget shares, regression parameters and so on - would be automatically comparable since they were derived from strictly comparable data.

The emphasis on comparability had two important consequences for the project. First, it increased costs greatly during the data cleaning and file preparation stage; a small part of this extra cost was recovered through economies of scale in subsequent computing and analysis. Much of the increased cost, however, was due to the fact that comparability was imposed, or fabricated, after most of the questionnaires had been designed and the data collected. In principle, therefore, costs could have been much lower if the implications of uniformity had been better foreseen. This question is considered again in view of its political (Section IID) and technical (Section IIIC) ramifications.

The second notable consequence was a division of labor in which the Coordination became responsible for all the international comparisons, leaving each member institute responsible for a national study for its own country. (These were conceived as national chapters of a joint volume, to which the Coordination would contribute a methodological and introductory chapter and a relatively brief comparative chapter.) This division of labor did not, of course, require the institutes to produce their own calculations, but it did mean that once the Coordination had produced the standard set of results for a country it became the institutes' responsibility to interpret and publish them. The Coordination never produced a study limited to one country, with the exception

of two PhD. theses (Howe, 1974, and Lamas, 1981) with very different emphases from the standard project. Conversely, no institute has produced a multi-country study. 1/ This arrangement worked successfully whenever the institute was able to produce its own study, but it meant that national studies were not published in two countries (Ecuador and Paraguay) despite completion of the standard calculations. In principle, this problem could have been easily overcome with more resources, either to expand the Coordination so that it could also write national studies, or to hire as analysts competent nationals of the countries affected.

It can be argued that the very nature of the ECIEL Program left it no choice but to emphasize international comparability: this was the Program's general objective, and it was the only way both to secure the widespread participation of institutes and to justify the substantial resources required for the Coordination. 2/ It is also true that the real costs of this emphasis were larger than they need have been. Still, the right balance between good national studies and satisfactory comparability among countries must be sought in any such study; even under more favorable conditions, it is evident that an emphasis on one objective would entail sacrifices on another.

B. Center vs. Periphery

The ECIEL project was intended to operate in as decentralized a fashion as possible, with all stages from fieldwork to publication undertaken by the institutes. Initially, it was not even expected that the Coordination would be

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1/ The apparent exception (Junta del Acuerdo de Cartagena, 1975) was prepared by an ECIEL associated institute which is international in character, and did not participate in the consumption study.

2/ This point is very significant, since ECIEL obtained its funds chiefly from donors interested in wide coverage and the possibility of generalization to as much of Latin America as possible. Higher-quality national studies that could not be compared or generalized would have been much harder to finance.

heavily involved in data handling or in publication. This orientation was, to a large extent, dictated by the emphasis on training and on institution-building which constitutes one of the Program's distinguishing objectives. The project was not intended to provide only analytical results, and it was recognized that while centralization might facilitate research output, the Program's other main goal would suffer.

This disposition toward decentralization, which arose for reasons internal to ECIEL, was reinforced by two reasons of a political and external character. The first was that it seemed necessary to demonstrate, to the participating institutes, donor agencies and national governments, that ECIEL was not simply a United States-sponsored mechanism for: 1) siphoning off data, 2) "exploiting" yet another Latin American "raw material", and 3) obtaining most of the value from processing abroad.[1/] Decentralization was intended to build credit for ECIEL, even at some cost in efficiency. The second reason was the hope that the study's results would be used by Latin American governments, which were expected to accept conclusions and recommendations more readily from a local institution than from a foreign Coordination whose structure and purposes they might suspect, or at least not understand.

In one sense, decentralization of the project was effective. Institutes did not simply gather data but, at least in some cases, shared in every subsequent stage of the study. Throughout the eight-year period (1966-1974), the

1/ One component was the fear that the data would be useful for marketing decisions by United States firms, so that Latin Americans would be exploited commercially with the help of their own information. Even purely academic uses, however, were sometimes suspect; North American researchers were also occasionally accused of "data piracy" and of leaving behind nothing of value. ECIEL was largely successful in avoiding such accusations, at least from persons and institutions familiar with the Program.

major decisions in the project were taken jointly, with every participating institute having a say in the Program seminars and remaining in close communication with the Coordination between seminars. Even when an institute lacked the technical capacity to help solve some problems, the Coordination and several other institutes would make a joint decision instead of the former arriving at a unilateral decision. The political doubts and suspicions that could have possibily destroyed the project may have receded for other reasons as well, but the open and democratic nature of the study undoubtedly helped dispel them.

In another sense, however, the history of the project is one of steadily increasing centralization. The centralization of data handling became necessary to compensate for the excessive decentralization of the previous phase of questionnaire design and data collection. Harmonization of the data in order to preserve comparability had to be undertaken by the Coordination, because at this stage no institute received any other institutes' data, and only the Coordination was familiar with all the different data formats, definitions of variables, and survey designs. Some decentralization of effort was preserved at this stage, as one institute (in Brazil) prepared its own data-conversion algorithm; 1/ overall, however, decentralization was sacrificed in the interest of comparability.

This stage, when substantial central intervention was required, can be characterized as a crisis situation that was resolved by temporarily shifting the balance between the institutes and the Coordination. Once all the data were

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1/ Conversion programs prepared by the Coordination were of course verified by the institutes, but the initiative was centralized in those cases.

converted to the common ECIEL format, and the national-level (or questionnaire) information was clearly distinguished from the international or uniform data, responsibility could once again have been decentralized, and to some extent it was. 1/ However, throughout the rest of the project another factor worked consistently in favor of increased centralization, namely the Coordination's position as the supplier of last resort, whether for computer facilities, programs, statistical methods or analyses. As the project moved from data-gathering toward economic analyses of statistical results, its requirements increasingly exceeded the capacities of the weaker institutes. If the project was not to come to a halt in the country affected, the Coordination would have to take on more responsibility. Eventually, to be sure, the Coordination's requirements outran its resources, in both time and money, and some work was abandoned. This did not occur, however, until quite late in the project.

In one or perhaps two cases (Ecuador, and to a lesser degree Paraguay), the shift of costs and responsibilities to the Coordination reflected, among other causes, lack of commitment of human and financial resources on the part of an institute. The ECIEL mechanism did not provide the Coordination with any means of compelling an institute to finish a project or of punishing it for noncompliance; moreover, the Coordination raised funds for the institutes mostly in the early, labor-intensive stages and had little money with which to motivate them later on. Thus, if an institute lost interest in the project the only alternatives for the Coordination were to abandon it or to take over its work.

1/ In particular, the Coordination did not base any analyses on the national-level data, and, in fact, made no use of them until after the standard part of the project was finished.

These cases were the exception; generally, centralization increased because the institutes ran into difficulties despite a strong commitment to completing their tasks, and needed and obtained help from the Coordination. Two examples are pertinent here. First is the centralization of computer processing, which came about because the institutes had no facilities (Paraguay), lacked the staff and skill to use a machine when it was available (Ecuador), or found that local processing simply took so long as to jeopardize the entire project. Even those institutes which published national studies (Colombia, Chile, Peru) relied on the Coordination for the calculations analyzed. By the end of the project, the majority of the institutes were self-reliant, or nearly so, in computer processing, so that a more decentralized project would have been feasible. However, even then, the effort to standardize the data-cleaning and statistical programs would have been made diffiuclt by the heterogeneity of machinery, languages and available computer packages, which gave a further impetus to centralization. The Coordination tried to decentralize the computing work, investing substantial resources in the translation of programs and their installation in those institutes with adequate facilities, but in no case could the difficulties be overcome in time for local processing to contribute much to the final results. Centralized computing, of course, required that the data be transferred to the Coordination; this was the crucial step in increasing the Coordination's work. 1/

1/ Political obstacles intervened in the one case (Argentina) where an institute did acquire the capacity to carry out all the standard processing through programs transferred by the Coordination.

The second example is more general and less dramatic: It is the tendency to centralize methodological decisions whenever they could not be settled during a seminar. This was not an invariable rule, since some questions - the estimation of price-elasticities, to take the clearest example - were investigated by one or more institutes, in addition to the suggestions made by the Coordination. In general, however, unsettled methodological issues were referred to the Coordination for a decision, and while the Coordination's proposed solution or procedure was discussed by the participants, it, nonetheless, tended to become the final decision. Using the Coordination as a last resort arose largely from the institutes' own choice: decentralization and democracy at the political level led increasingly to centralized decisions at the technical level. The process developed partly because of the increasing technical sophistication of the study, and partly because the political motive for decentralization withered once trust was established and, particularly, once the data were physically transferred to the Coordination. Further, as the institutes gained more experience with the ECIEL mechanism, and as the project's time frame extended much longer than expected, considerations of speed and efficiency became somewhat more important - and these often favored centralization.

It is evident that the issues concerning the degree to which a project should be centralized and the degree to which it should possess a comparative, international nature could not be uncoupled. It is also clear that these issues could not be decided once and for all, since the balance between the Coordination and institutes continued to shift throughout the project, for a variety of reasons. Finally, the extent of decentralization could not be and increasingly was not, the same for all participants. This point, that the

Coordination's role differed among institutes because the latter demonstrated such different interests and capacities, is given further consideration in Section IID below.

C. Research vs. Training

The objective of the household budget study was always two-fold: to produce quality research, and to train people and institutions so they could undertake similar research thereafter with much less outside assistance. As was indicated above, this second objective justified a higher degree of decentralization and greater cost than would have been necessary for the immediate research alone. It is not easy to evaluate the success of the training effort; participants in the project disagree among themselves over how much training really took place, and over whether it was worth the cost. 1/ In order to approach this question, it seems useful to differentiate between certain key concepts.

The first distinction to be made is that between the training of individuals and "institution-building" in the larger sense of creating capacities involving many different skills and people. The ECIEL experience strongly suggests that the former was necessary, but insufficient, for the latter: It was necessary because the link between one institute and the others (including the Coordination) typically resided in just one or a few individuals. During the interval 1965-72, for example, an institute might have sent as many as seven different persons to ECIEL seminars, but generally one or two individuals attended the majority of meetings. Technical coordinators frequently visited the institutes, expanding personal contacts; however, these visits were brief, and the Coordination could do relatively little to influence an institute directly and still less to train its personnel. Moreover, seldom if ever did

1/ Even those who directed the project disagree on this point, and the experiences of the institute participants were still more varied. In large measure this is because benefits are hard to measure, and the gradual increase in knowledge and experience cannot easily be attributed to specific inputs.

did the individual(s) attending the seminars lack skills or knowledge relevant to the project otherwise possessed by colleagues in their institutes; thus, anything a participant failed to learn or understand was not easily transmitted to the institute since no one there could compensate for the deficiency.

Individual training could be insufficient, even when it was relatively successful, for two reasons: 1) The individual could simply leave the institute and take his training with him; 2) there were few systematic efforts to ensure that what a project participant learned was passed on to his colleagues or affected the operation of his institute. Often, in addition to his work on the study, the participant had other duties, such as teaching or overseeing an office which handled many routine statistical tasks. Or his superiors might want to minimize the resources committed to the project, and so be reluctant to involve other staff members. "Institution-building" is at least as much a question of organization and dedication as of technical competence. In a few cases, however, (in Bolivia and perhaps in Paraguay), the project did hold an institution together for a time; its effect was total rather than marginal.

The second distinction to be made is between incremental training and learning from zero. A person who already knew something of sampling theory could extend his knowledge to the complex, non-proportional designs used by ECIEL, and if he had some acquaintance with econometrics he could learn a great deal about dummy variables and different functional specifications; but he could not learn either sampling theory or econometrics from the beginning, in the ECIEL project alone. The seminars were too brief and too infrequent to be used as basic courses in such materials. Moreover, the possibility of shifting the effort to the Coordination implied that it was seldom essential for an institute participant to understand fully a particular theory or technique. Finally,

if the Coordination had formally attempted to teach the various subjects required for the project, it would not only have needed much more time and resources, it would also have complicated still further the task of finding suitable participants in those countries where few people already had the necessary minimum training.

Together, these two considerations explain why the training component of the project was most successful in teaching additional skills to individuals who already had substantial academic backgrounds. 1/ This leads to the third distinction of importance, that between formal or classroom training and apprenticeship or practical experience. The ECIEL Program could offer very little of the former, for which it was simply not equipped, but it offered the latter to many participants who, otherwise, would have had little chance of duplicating the experience elsewhere. The study can be judged very successful in its role as an apprenticehsip which offered the opportunity to apply theoretical knowledge under supervision over a long period. In this respect, ECIEL helped fill a serious gap in most Latin American university education, which emphasizes theory but gives students little practice in confronting problems outside a textbook. 2/ Moreover, while previous academic preparation determined to what degree a participant could exploit this opportunity, all those involved could learn something from the experience.

1/ By the end of the project, participants in three institutes had doctorates in economics from United States universities. During most of the study, however, neither the participants nor the Technical Coordinators had that much training, nor was it necessary for most purposes, particularly since expert help could be and was obtained from consultants. The line between adequate and inadequate formal training corresponded more nearly to a bachelor's degree, and, in this respect, large differences existed among institutes.

2/ This conclusion is reinforced by the experience of two of the project's former Technical Coordinators, and the Coordinator of another ECIEL project, in teaching economics and quantitative methods to Latin American university graduates over several recent years.

The centralization of the project, therefore, did not always undermine the training function. To some extent training was protected, since it was possible for relatively unprepared investigators to participate without significantly harming ongoing research. In addition, the opportunity, reduced by centralization, to learn by doing in one's own institute was replaced by the chance to spend an apprenticeship of some weeks or months in the Coordination. Bringing participants to the Brookings Institution to help work on their own data greatly simplified the Coordination's task of harmonizing and cleaning the data, and allowed the institute researchers to learn from cooperating in the effort. This collaboration was extremely valuable for Colombia and Peru, and almost equally helpful for Brazil and Chile.

In principle, the intention of the ECIEL experience was to train the project investigators and their colleagues; this did not include the training of people in the peripheral skills required for the project, such as computer programming, management, accounting, etc., either in the institutes or the Coordination. In practice, however, the efforts to decentralize and hold down costs meant that resources were devoted to training programmers, in several of the institutes as well as in the Coordination. Had decentralization been more successful, these resources might have been better spent; as it happened, relatively little was accomplished in several of the institutes, and the apprenticeship of programmers probably delayed the project and eventually raised costs in the Coordination. Two institutes (in Colombia and in Brazil) did, however, benefit substantially from the apprenticeship of programmers who spent several months at the Coordination while working on their own countries' data.

The ECIEL experience suggests a general principle for any collaborative project in which training is treated as an important independent objective. A line should be clearly drawn between the skills and knowledge to be developed within the project, and those skills, which should be treated as prerequisites, to be available outside the project. Failure to make this distinction can extend the teaching or apprenticehsip beyond the project's capacities, diluting or slanting research efforts without a corresponding gain in training.

A final observation on the training component of the study is that while it was expected that learning would spread beyond the participants to other institutions, it is very hard to evaluate such diffusion or even to be sure it occurred. In a few instances, participants moved into important government positions and took their knowledge with them; in other cases a direct influence of the ECIEL experience cannot be distinguished from more general changes in the intellectual climate, changes which ECIEL may have helped bring about.

D. The Political Context

While it was hoped that the household budget study, and all other ECIEL projects, would produce results useful for policy decisions, the Program's goal was consistently to conduct studies uncontaminated by political pressures and considerations. This decision reflected not only the academic orientation of the Program's founder, but also a realistic appraisal that the injection of politics could not strengthen a project, but rather could easily destroy it by preventing institutes from participating, disrupting the flow of information, etc. At the time (the early 1960s) and considering the circumstances, participation by United States scholars was a potential liability. Most Latin Americans who, for whatever reason, might have been suspicious of the Program

or any of its projects, had never heard of the Brookings Institution, and commonly supposed that it was related closely to the United States government. Thus, it was not easy to point to Brookings - independent of political authority in its research, but able to command governmental attention and respect for its conclusions - as a model for ECIEL to follow. In fact, on a few occasions members of the Coordination were accused, directly or implicitly, of spying: and given the ruses sometimes employed by intelligence agencies, and the apparent uselessness of much information they gather and classify, it was not easy to dismiss such accusations by describing the nature of ECIEL and the type of data its projects generated. Such accusations, of course, came from uninformed sources, and may have been exaggerated for dramatic effect, but behind them existed some genuine anxiety.

ECIEL's cautious, apolitical, scholarly, open approach certainly appears to have been wise since, despite assorted difficulties, no politically-motivated disaster befell the Program or the Coordination in the history of the project.1/ The gradual shift toward centralization, discussed above, is clear evidence that political difficulties typically lessened rather than mounted during the project. Nonetheless, in order to form an overall judgement of how successfully ECIEL avoided or resisted politicization it is helpful to identify three different levels of political interaction.

1/ If ECIEL had not established a reputation as an honest research enterprise it could easily have been destroyed in the aftermath of Project Camelot in 1966, when it became known that many local social science investigators in Chile were financially supported - often unknown to themselves - by funds supplied by the CIA, which hoped to assemble a body of data on political and social behavior. The threat of scandal or political disruption was therefore not a fantasy but a real danger.

The innermost level comprised the participants in the project, who had to debate and resolve many issues potentially incurring substantial costs to themselves or their institutes. During the course of the seminars, struggles arose over how to settle these issues: very often the question involved which institutes would have to modify their procedures or proposals to accommodate others. In this sense, politics intruded into the conduct of the study; yet it is important to realize that the group was never paralyzed or split by disagreement. When a common solution could not be found, the problem was always either referred to the Coordination or resolved by permitting variants of a general methodological scheme. These solutions often raised the cost of the project but never threatened its existence. Equally important, the controversies never involved national political positions or antagonisms: the institutes pursued their own interests, not the supposed interests of their respective countries. 1/

One political decision of consequence was taken at this level early in the history of the Program: it was decided to treat all institutes equally, irrespective of their type (public or private, academic or bureaucratic) and capacity. Formally, even the Brookings Institution, which housed the Coordination, was just one institute among many. 2/ Probably no weighting or

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1/ This distinction lies at the core of ECIEL, although observers and even participants have often blurred it by referring to countries rather than to institutes, and by calling the researchers "representatives" or even "delegates." This confusion seems, particularly, to affect employees of national governments and international organizations, and to prevail less among academicians.

2/ Resumen I (Santiago, 1963): "The institutes mentioned will work independently and in conditions of perfect equality. Methods of work, agreements and conclusions will be reached by unanimous decision."

distinction among institutes would have been feasible, but it is arguable that this extreme democracy increased costs and the work required of the Coordination. A clearer directive role for the Coordination might have been preferable.

A second level of political interaction can be defined within each institute between the participants in the project and their superiors. While the latter controlled the local resources available for the project, they often did not fully understand the study, were more distressed than the participants over the cost and time involved, or were more concerned to protect their reputations than to assure that research was properly carried out. In particular, institute directors were more likely than the actual investigators to fear criticism for releasing the data to outsiders, to want to conceal deficiencies in the data or in the institutes' capacities, and so on. The Coordination lobbied these people so as to protect the research, but such efforts sometimes had relatively little influence on them. At this level, politics of a standard bureaucratic kind constituted a more serious obstacle to the study; but while there were delays, again there were no disasters to the research, and the political struggles that occurred were not entirely outside ECIEL's sphere of influence. 1/

A third and final level to consider consists of the entire individual country. National political considerations could and often did hamper the institutes, whether or not the reasons had anything to do with the nature of the project, the relation between the institute and those in other countries, or the connection with the Coordination. At this level, ECIEL was powerless, and some

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1/ From the institutes' viewpoint, it could be argued that one or more "disasters" did occur when this kind of political problem prevented the completion of a national study and the institute's full use of its own results. There were, however, no cases in which the research was blocked altogether.

disasters to the research did occur. The only consolation is that nothing the Coordination could have done would have been very likely to yield a different outcome. In fact, any long-term project conducted in several countries of a politically unstable region is virtually certain to be disrupted for political reasons at least once. It is remarkable that even under these conditions, individuals who wanted to see the research proceed could and sometimes did help protect the project in a country by aiding its transfer to another less politically affected institution (as occurred in Peru, from the Universidad de San Marcos, a state university,to the private Universidad Católica), or by ensuring that the data were made available to the Coordination fairly early in the project (as happened in Chile).

In summary, it seems that ECIEL succeeded in avoiding severe political difficulties to the degree that the participants could control the political environment. The larger the context, the less control ECIEL could exercise, and the more the project was exposed to politically motivated difficulties. The project's experience also supports three other generalizations of interest. First, there was no association between the prevalence of political trouble and the degree of backwardness in the country involved. ECIEL did not systematically have an easier time in the more developed, or in the less developed, countries.

Second, there was a clear association between the likelihood of difficulty and the type of institute involved - specifically, whether it was an agency of the national government. The most basic difference among the ECIEL member institutes exists between private entities - either universities or independent research groups - and public bodies, usually statistical offices. In the

household budget study, the latter were more likely to be affected by national political events. 1/

Third, it should be noted that the politically crucial stage in the project was the transfer of the data to the Coordination. Political difficulties arising before the transfer had been completed could have put an end to the project; subsequent problems had much less effect since the possibility existed of analysis by the Coordination. Moreover, in some cases the transfer of the data, after initial reluctance, marked a shift in the institute's attitude and degree of cooperation. Worries about breaching condifentiality and exposing the data to unfavorable comparisons either substantitively or methodologically, tended to disappear. 2/ The responsibility for delay shifted to the Coordination, while the institute typically became impatient for results.

1/ The distinction among institutes is also relevant for the success of training efforts: more learning and re-orientation occurred in the private, academic institutions.

2/ Data transferred to the Coordination identified each family by a questionnaire number to facilitate checking back during cleaning, but all information such as names and addresses was removed, leaving data on only the stratum and the trimester. "Confidentiality" was simply a respectable-sounding excuse for delay in transferring the data, but never a real consideration.

III. PROBLEMS, SOLUTIONS AND CONCLUSIONS

Discussion in the preceding section emphasizes general objectives of the study and the kinds of problems that arose, or could have emerged, from competition and interaction among objectives. This section, in contrast, will consider specific problems and how they were dealt with.

A. Political Obstacles

The ECIEL household budget study was initially intended to include institutes in all eleven LAFTA countries. The Coordination completed the standard data processing for only six of those countries. In two cases (Bolivia and Brazil), a shortage of resources and waning interest as the project extended much beyond its expected completion date contributed to abandonment of the Coordination's work.[1/] In four countries, political difficulties either prevented the complete transfer of data to the Coordination (Argentina, Mexico and Uruguay), or delayed it so much as to impede the project's completion (Bolivia). No two of these experiences were exactly alike; all of them proved beyond ECIEL's ability to control since they occurred at the third or extra-institute level previously described.

The simplest case was that of Mexico: it was intended that the ECIEL member institute, the Colegio de Mexico, would obtain and analyze data from a 1968 survey undertaken by the Banco de Mexico. The Colegio, and therefore ECIEL, did not participate, except peripherally, in the design or execution of the survey, and had little influence over the Banco, a government agency. For more than two years the Banco de Mexico referred to the data transfer as imminent, but, in fact, no information was ever provided, and Mexico was dropped from the study.

1/ The Fundação Getulio Vargas proceeded, however, to analyze the Brazilian data and publish studies based on them. The fact that the institute could continue the project was one reason, among several, why the Coordination gave Brazil lower priority than some other countries where the institutes lacked that capacity.

In a sense, it was never part of the project except at a theoretical and methodological level.

In Argentina, a somewhat similar but much more complicated problem blocked the transfer of data. There the survey was actually conducted by CONADE, a government agency, and the data hence became the property of the national statistical office, INDEC. However, the survey and questionnaire were designed by the Instituto Torcuato Di Tella, a private research group and founding member of ECIEL. A formal agreement was signed between Di Tella and CONADE giving the latter priority of publication but assuring the former access to the data and freedom to publish. The Program also provided for several visits by a consultant (Robert Ferber) and Di Tella participated in the data cleaning. Following military intervention in the government in 1973, however, INDEC was put under the control of military officers who were ignorant of and unsympathetic toward research, and who refused to let the data leave the country. 1/ It was then agreed that Di Tella could, with the help of INDEC's computer facilities and staff, analyze the data and could publish a study once INDEC had prepared and published some results.

In order to make this agreement succeed, the Coordination sent a programmer to Argentina for several months and devoted substantial resources to instal the sequence of cleaning and analytical programs in INDEC. This effort eventually allowed Di Tella to compute all the standard results of the study as well as to obtain a clean copy of the data for other analyses (Dieguez and Patrecolla, 1979). Following the military coup in 1976, however, the Videla government

1/ The "confidentiality" excuse was involved, and so was the nationalistic argument that Argentina should not depend on foreigners for data processing. The Coordination's suggestion that the principal INDEC programmer accompany the data to the United States and supervise the work on them - a practice that succeeded very well for Brazil and Colombia - was also rejected.

effectively put a stop to most statistical activities; INDEC never published any results from the data. The protracted delay and rising costs made it impossible for the Coordination to continue supporting the work, and so, quite late in the project, efforts to finish the project in Argentina were abandoned.[1/]

Because the difficulty in Argentina originated at high levels of government, ECIEL could do nothing to prevent it. Nevertheless, the experience illustrates that the Program could respond quickly and flexibly to limit the resulting damage in an effort to salvage the project. Had it not been for the military intervention, the CONADE-Di Tella agreement would probably have been implemented by both parties since no bad faith existed between them. Later on, with slightly better luck, the emergency resources devoted to Argentina might have succeeded in saving the study there.

Similar flexibility, again resulting in additional work for the Coordination, was also successful in Venezuela. There the Banco Central, which had conducted the surveys directly or through the Universidad del Zulia, intended to process its own data and send only final results to the Coordination. The Banco Central succeeded in installing and using several of the data-cleaning programs, but due to a shortage of staff, the effort began to take much longer than planned; thus, in 1972, the Banco agreed to transfer the data to the Coordination and pay for some of the additional burden this imposed. The standard processing for Venezuela was completed in the next year and a half with the Banco's full collaboration.

1/ For a year or more, it was expected that, although delayed, the Argentine results could still be published together with those for Bolivia, Brazil and Paraguay; various plans for a second book, or a revision of the five-country Andean Group study, were discussed at seminars as late as 1976.

In Uruguay, a very different sort of political problem, internal to the institute but involving issues of national politics, forced the Universidad Oriental del Uruguay's department of statistics not only to stop the transfer of data to the Coordination after more than half of it had been sent, but also to withdraw from ECIEL altogether in 1972. This is the only case in which student agitation seriously affected an ECIEL institute; several other university departments are members of the Program, and most have experienced student strikes or pressures, but these have not affected the research. The problem in Uruguay could not be contained because the agitation focussed on the institute's association with a United States institution, and the university authorities did not have government support in facing the challenge. The institute subsequently indicated its interest in resuming participation in ECIEL, but that prospect was ended in 1973 by the Tupamaro violence and consequent political repression. A large part of the data was, in fact, transferred to the Coordination, but without the participation of the institute it was impossible to clean and analyze them adequately.

Finally, the ECIEL study in Bolivia was the victim of the military golpe that overthrew the Torres government in 1971. In contrast to the case of Argentina the new Bolivian government did nothing specifically to frustrate the project, but, as occurs after most changes of government in Bolivia, it closed temporarily the universities and fired large numbers of civil servants and academicians. This freeze, which sent the participants in the project looking for other work and led them to avoid the University of San Andres, would not by itself have ended the project. The data were saved, 1/ and were eventually

1/ In one of the most melodramatic episodes in the history of the study, one of the participants recognized the questionnaires among a pile of papers being discarded from the empty university offices, and without revealing his connection with them, had them saved for scrap paper.

transferred to the Coordination, but the delay and interruption and communication meant that priority was meanwhile given to other countries' data, and the Coordination's subsequent shortage of resources prevented completion of the Bolivian study. 1/ This case, more than any other, illustrates simple bad luck; when the golpe occurred, the data were nearly ready to be transferred requiring only an estimated three more weeks of work. Had they been transferred then instead of years later, sufficient contact could probably have been maintained to keep the Bolivian participants active in the data cleaning, and the analysis could have proceeded with only a few months' delay.

It is not clear what lesson can be drawn from these unhappy experiences, beyond the conclusions that: 1) government instability, bureaucratic secretiveness and uninformed political passion are bad for research, and 2) it is desirable to have copies of the data available outside the country very early in the course of a project. Part of the trouble in three of the above cases (Argentina, Bolivia and Mexico) stemmed from the weakness of private or academic institutions which in turn forced them to rely on government agencies. It does not follow, however, that ECIEL would have done better to associate itself with state agencies: aside from their generally weaker interest in research and lower technical competence, they are even more vulnerable to governmental upheaval than are private institutions. The ideal kind of institute to participate in such a project should be both strong and independent, and even then the absence of any violent changes of government while the study is underway would be a helpful factor.

1/ A further difficulty was that surveys were conducted some years apart in two Bolivian cities, with some potentially serious methodological differences and changes in the economic environment in the interim.

Colombia, Venezuela and Peru fulfilled these conditions in varying degrees between 1968 and 1974, and in none of them did the project face any serious political difficulty after 1968. The institutions involved were respectively a private university, a central bank (both more qualified and more safe from political pressure than many public agencies), and a university which is privately operated but depends heavily on public subsidies. There was also no political difficulty in Paraguay, where the private institution involved was technically weak but very cooperative, and operated in a stable climate.

In view of the history of the last decade, it is most surprising that political obstacles did not put an end to the project in Chile. Fortuitously, the survey data, collected in 1968-69, were transferred to the Coordination as early as 1970: this was possible because the Universidad de Chile was a relatively strong and competent institute, which proceeded rapidly with the study and shared the data early with the Coordination. The country's tradition of peaceful changes of government, in which most public jobs were unaffected, was also important, as was the intellectual commitment to keeping the study free of political interference. (In all respects, the contrast with Bolivia is dramatic.) Had the events of 1973-74 occurred three or four years earlier, or had the project been undertaken that much later, it is doubtful that the Chilean part of the study could have been completed without serious delay and disruption. 1/

1/ An undoubtedly helpful factor was that the nature of the research in the household budget study is relatively conservative by Latin American standards, and therefore attracted the kind of participants who were least threatened in the "purging" of universities following the 1973 golpe that overthrew Allende. However, the project still attracted the support of some participants and institute directors whose own political positions were relatively radical. It may be that what mattered most was the project's insistence on learning facts rather than on speculation: in this sense, empirical research is almost invariably "conservative."

B. Turnover of Personnel

The long lifetime of the project, coupled with the shortage of trained participants and their consequent potential high mobility, might have made turnover of research personnel a serious problem. In fact, this does not seem to have been the case: turnover was lower than expected, and its effect on the project was generally slight, certainly when compared to external political events. While three different individuals served as Technical Coordinators of the project, for periods as short as one year and as long as three, this change of personnel had little effect; each Coordinator continued to serve for a time as a consultant to his successor, and the change in Coordinator was not accompanied by changes in the research staff at the Coordination or among the institute participants.

It is hard to find any cases in which the departure of a participant significantly hurt the project. In one instance, resulting from a disagreement within the institute (in Colombia), the principal researcher left; thus, the institute did not retain a copy of the initial version of its data and documentation for it. However, the data had long since been transferred to the Coordination, from which it could be restored to the institute and used as if there had been no interruption. In addition, the researcher had already written and the institute published a large study of the results. 1/ The only lesson from this experience is that it was very helpful to have the project's "memory" stored in two places.

1/ Since this was the case, it could even be said that the Colombian project was finished - although the Coordination had not finished its analysis - by the time this change occurred.

In the case of Venezuela, a principal researcher moved from one city to another, apparently causing the institute in the former city to suffer since no other investigator took over. The project as a whole, however, did not suffer because the two principal researchers who previously worked separately in two cities henceforth collaborated more closely. Some other instances could be cited in which a participant left his successor(s) relatively uninformed about the project, or upon being promoted or given other responsibility had less time to work on the study, but the effects do not seem to have been significant.

One reason why turnover was not a significant problem may be that the project attracted two relatively immobile types of participants - academicians and bureaucrats or technocrats in rather apolitical agencies. This was consistent with the Program's effort to avoid political positions and controversies, discussed in Section IID. Another reason why turnover was seldom disruptive is that the project was relatively well documented; a new participant could, without much trouble, inform himself on the history and status of the project. Finally, it may be true that the existence of the study actually reduced turnover among the people who participated in it. By offering researchers a stable, long-term project in which to work, with a chance both to acquire new skills and to produce publishable results, the study gave them strong incentives to specialize in the subject and to stay with it for long enough to repay the large initial investment of time and effort. In fact, those participants who stayed with the project for the longest time were often - though not invariably - also those best able to profit from the apprenticeship it offered. Researchers who had the opportunity to move into a significant government job often did so. To the extent that their experience in the project made them better trained or

more valuable, ECIEL may have promoted some turnover; however, researchers did not have to seek lower-level public jobs, or private-sector employment in order to survive, and in this way the project undoubtedly stabilized their situations.

C. Documentation

The volume of documentation produced in the course of the study was enormous, even without counting the published substantive results. Reading through that material now, it is easy to form the impression that much of it is superfluous and redundant. Such an impression, however, would be unfair since it overlooks the evolutionary nature of the project and its democratic mode of operation, both of which tended to increase the amount of discussion and revision that had to be recorded.

Documentation served various purposes and, therefore, took different forms. The most indispensable was to document the data themselves, to clarify what every variable meant in every country as well as how variables could be compared across countries. The end product of this process was a common codebook for the standard ECIEL files and country-specific codebooks for the national, questionnaire - level files. In addition, it was also necessary to document all the transformations and conversions in the data and the logical and arithmetic relations among variables on which the data cleaning depended. The success of this effort - probably the most detailed and exhaustive component of the project - is shown by the ease with which data have been subsequently used by investigators who had nothing to do with the project. 1/

1/ The point is not simply that an investigator can be given a data tape and a codebook which is reasonably clear and complete; the documentation previous to that stage has several times made it possible to answer detailed inquiries about the data, to return to earlier versions of the data to add or disaggregate information lost in the ECIEL-level harmonization, and even to track suspected errors to the original questionnaires. This early-stage documentation becomes harder to use with the passage of time, of course, since it requires more of individuals' memories and experience.

While the process could have been streamlined, not much could have been omitted except at the risk of leaving the data unintelligible within a very short time.

A second function of documentation, represented particularly by the Resumenes de Seminario or summaries of the (then semi-annual) seminars, is to discuss and record methodological choices and decisions. Many of these summaries appear in a series of documents which were typically prepared by the Coordination, distributed to the institutes, and discussed at the seminars: the series ran to number 84 before stopping. 1/ Some of the questions considered were never resolved and some solutions were modified, sometimes repeatedly. Still, the document series records virtually all the procedures adopted for structuring the samples, cleaning the data, testing the quality of the sample and adjusting it for several kinds of possible non-representativeness, and organizing the standard computational results. These documents facilitated for newcomers and outside observers the task of assimilating the details of the project, and they have subsequently been used by the institutes and by other researchers to guide new analyses of the data.

1/ The seminar summaries swelled and shrank in proportion to this kind of documentation. The sections dealing with the consumption study went from about 20 pages in 1968 to a peak of 130 pages in 1971, and declined to only 10 pages by the time the standard processing was completed in 1974. The documents do not cover 84 different topics, since several notes were sometimes devoted to a single problem as successive questions were resolved or procedures refined.

A third function of documentation, retrospectively the least successful or important, was to project the study forward in detail to discuss what would be done next and how the results would be presented. It can be argued that much of this effort was misguided not only because deadlines were commonly missed, but also because the attempt to specify every table or regression to be obtained before any empirical results are available is inflexible and inefficient. 1/ It is easy to see that this emphasis arose largely from the desire to produce common statistical results and maintain comparability through the analysis; in order to budget time and money, it was also necessary to look ahead continually. However, frustration at the slow realization of the analysis may have diverted effort to spelling out at great length what the results should look like. This rigidity left little or no room for experimentation. Some tables turned out to be relatively useless, or at least very difficult to interpret. 2/ While many slightly different concepts of income, consumption, family size or other concepts were available in the data, most analyses were based on just one definition usually chosen a priori rather than as a result of experiments. And while regression specifications were

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1/ The specification for the tables and regressions, together with some guidance as to how to interpret them, were assembled in the "outline for the first book" of the consumption study (Howe and Musgrove, 1973), a document of 153 pages.

2/ The best examples are the tables displaying means of the consumption/income ratio by classes of income or variables associated with income such as education or occupation (Musgrove, 1978b Tables 3-1 to 3-3). These turned out to reflect little but transitory income variation.

extensively discussed, experiments were conducted for only one country (Colombia), and these affected the specification of only a few variables; the choice of functional forms was not based on the study's own empirical findings. 1/ The rigidity of the standard statistical results is partly offset by the variety of relations specified in the several specialized studies which began to be produced in 1973. The argument here is that it would have been preferable not to specify the standard analysis in detail until some of these results, or similar experiments, were ready. Because the project eventually ran out of resources and ECIEL moved on to new studies, there has been little opportunity to generalize these non-standard analyses or extend them to more countries. One case, in which a variety of approaches was compared, is the summary of income-consumption functions (Musgrove, 1978a), which draws on several of the specialized studies.

The problem to which this criticism is addressed is, to be sure, a basic one in ECIEL or any similar project. To specify too little in advance would have reduced the comparability of results by letting each institute compute or request its own specifications; or it would have increased centralization still more by leaving all experimentation to the Coordination. A better balance would have involved experiments, discussion and decentralized analysis, a joint decision, and then another round of empirical results. Regardless of the sequence, it should be well documented: it is much better to over-specify and later modify procedures, than to leave any decisions or results inadequately documented.

1/ For a list of those completed by mid-1978, see Musgrove (1978b, pp. 22-24).

Finally, of course, the existence of so much documentation in the project greatly reduced the difficulties associated with personnel turnover and facilitated the training of new participants or research assistants as the project expanded. The Coordination was responsible for a large share of the documentation, and in some ways was also, naturally, the chief user of it; but the same working papers made it possible for the institutes to discuss, understand and apply the decisions taken in the project. Such documentation was, therefore, indispensable to the study for its contribution to substantive results as well as for its value as a training component.

D. Myopia, Heterogeneity and Costs

Sections IIA and IIB emphasized the tendency for: 1) costs to be shifted from the institutes to the Coordination, and 2) total costs to be much larger than expected, in part because choices that reduced the expense to an institute often led to a more than compensating increase for the Coordination. Much of the difficulty with costs in the project can be attributed to the problem of balancing competing objectives: A project designed to conduct good research at the national level but also comparable among countries, to be as decentralized as possible but with centralized assistance and resources available when needed, and to have a large training component, could not fail to be more expensive than a collection of independent national studies. The difficulty in anticipating costs can also be attributed to the pioneering nature of the project, not only with respect to survey work but also in the computer programming and procedures for testing and analyzing data.

When these factors have been taken into account, it still appears that the project was more expensive than it need have been. That is, cost problems

reflected some inefficiency as well as the constraints imposed by the project's objectives. Such inefficiency cannot readily be quantified because it is hard to determine exactly where the objective constraints were, or to what extent it would have been possible to change the project without forcing the sacrifice of one of its goals or the withdrawal of one or more institutes. Still, the sources of inefficiency can be identified: the two most important ones were heterogeneity or non-uniformity of data, and myopia regarding the cost implications of methodological decisions.

In principle, the problem of heterogeneity should never have arisen, for not only was the project committed in general to produce internationally comparable data, but also the content and structure of the questionnaire were extensively discussed in the early seminars. Nonetheless, early in 1968 when the Coordination obtained copies of the detailed questionnaires which had already been used in six countries, it was evident that they differed in innumerable small ways. They were similar, to be sure, but hardly identical; for the purpose of creating uniform definitions of variables in order that all the data could be arranged in a standard format and code, a small difference is nearly as expensive to adjust as a much larger one, up to the point where the differences become too great to permit any harmonization.

It is true that many of the differences among questionnaires could be resolved by aggregation since the same total might be divided different ways. Although this solution was often adopted, it was, nevertheless, costly since it effectively prohibited international comparisons at the questionnaire level of detail. The problem was most common, but also least severe, in the

disaggregation of expenditures. Much more serious difficulties arose in the data on income, even worse problems in data on assets and savings, and some insoluble difficulties in the classification of household members, their relations to one another and their identification with particular purchases. 1/

Even when transformations of the data could be defined, which resulted in the "same" variable meaning the same thing in all countries, there was still an immense cost to the process of harmonization of conversion. First, such a transformation took years to complete, a process which occupied the project Coordinators as well as the research assistants, programmers and institute participants (and sometimes institute programmers). It is probably no exaggeration to estimate that two years could have been saved if the data had been uniform from the start. Second, the conversion was often so complex that it created a breach between the original questionnaire-level data and the standard ECIEL data. Most of the cleaning and analysis were applied only to the latter, leaving many more errors in the former. It has been possible in some subsequent analyses to return to the original data for disaggregated variables, but the cost is typically very high, especially if more than one country is involved.

This experience may account in part for the emphasis, later in the project, on uniformity of analytical specifications. It is unfortunate in both cases that the emphasis was misplaced: there should have been much less

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1/ An example of this last problem is the classification of expenditure on clothing; some countries grouped boys' clothing with men's and girls' with women's, while others separated children's clothing, irrespective of sex, from that of adults. To make matters worse, the age separating children from adults was not always the same across countries or among different purposes. The problem of harmonizing the definitions of the household or consuming unit is treated extensively in Resumen XIV (Buenos Aires, 1970).

heterogeneity in the data, and there could safely have been more variation and experimentation in the analysis, at least before determining a common specification. The start of the fieldwork locked ECIEL into the non-uniformity of the data, but experiments in the stage of analysis would have been cheap and could have been discarded in favor of other approaches.

There are two reasons why this problem arose despite some efforts to anticipate and prevent it. One is simply the failure to recognize how important small differences could be, especially for computer handling of the data. Initially, all the participants in the project relaxed once the discussion of questionnaires reached what seemed like a well-defined common core. In particular, insufficient attention was paid to the consequences of an institute's omitting a variable altogether, or adding one which was not on the common list: since the core was not precise enough, independent additions or deletions had more effect on uniformity than anticipated. Employing a competent computer programmer to study this question very early in the project would have also helped.

The second and more difficult reason was political. The Coordination feared it could not "push around" the institutes without causing one or more of them to drop out of the project; and the institutes themselves sometimes insisted on their position for sound internal reasons such as matching the survey to a previous one or to other sources of data in the country. Unfortunately, the political issue prevailed even when insistence bordered on stubbornness and its cause was frivolous: institutes believe, before undertaking the study, that their countries differed more than they do, and sometimes insisted on variable choices and definitions which were later seen to be arbitrary.

In general, such insistence was harmless when it led to greater disaggregation of a well-defined variable but, on the other hand, was costly and unnecessary when it led to different rules for aggregation or initial definitions. Differences in sample design, arising entirely as a result of institute preferences, were still more costly because the Coordination had to prepare statistical and computing means for dealing with every design variant.

Since institutes often got their way on these choices [1/] and since the extra costs were largely passed on to the Coordination - apart from the cost in time suffered by all participants - the evaluation of this problem may differ between the institutes and the Coordination. It also differed systematically within the Coordination. The General Coordinatior, who was responsible for the cohesion and credibility of the whole Program as well as for most of the diplomatic negotiation was most concerned that participation be maintained and friction avoided. In this he was largely supported by the project's consultant. [2/] The Technical Coordinators, who had to deal with the consequences of the heterogeneity of data, believed that the institutes were allowed too much latitude; in this view, they were strongly supported by the project's chief programmer, much of whose time was devoted to data conversion. The Technical Coordinators might even have been willing to lose one or two countries from the project in return for fewer costs and earlier completion.

1/ With the exception of institutes which joined the project late; one of them (Paraguay), for example, simply adopted the ECIEL common format as its questionnaire, eliminating the distinction between the two levels of data and the need for conversion.

2/ It was, of course, particularly important not to lose from the project any of the founding member institutes of ECIEL who were usually the stronger institutes, with previous survey experience or definite plans for analysis of the data. Unfortunately, an institute which was technically strong and politically important was more, not less, likely than others to insist on a costly and unnecessarily complicated procedure.

In fairness to the institute participants, it is undoubtedly true that they developed heterogeneous questionnaires and insisted on minor differences because they had no idea of the costs involved. Had the participants in the project anticipated how much non-uniformity would eventually cost, they would probably have agreed on more uniform data at the outset. In a sense, therefore, the problem of heterogeneity represents the most important instance of the problem of myopia, of concentration on meeting an immediate goal or reducing an immediate cost with too little regard for subsequent difficulties. In another sense, this problem is not so much one of shortsightedness as of a trade-off between different and only partly reconcilable objectives. There were numerous instances of simple myopia, or pursuit of false economies, especially in the early stages of the project. These centered around efforts to reduce the physical size of the data files - making variables as short as possible, minimizing the use of punched cards, adding identifying numbers to variables so that an empty variable (one to which no household responded) could be left out entirely - and so on. Any economies in cards, paper or tape achieved by these means were more than offset by increased costs of computing, especially programming difficulties created by variable field-widths. 1/ As the study progressed the kind of technical short-sightedness became less of a problem.

In large part, myopia was a consequence of the state of the art: too little was known about survey work, data-handling and computer analysis, especially in countries where no such project had ever been conducted. Because ultimate costs were difficult to anticipate - and because personnel costs were largely borne by institutes' regular budgets while additional funds had to be obtained

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1/ The problems caused by some of these false economies, including the excessive demand they made on human attention, are discussed in the context of general principles of data-handling, by Howe and Villaveces (1974).

for computing - there was a natural emphasis on the immediate, readily quantifiable components of cost such as punched cards. In retrospect, it appears that the participants worried too much about the wrong costs. Part of the problem, however, was that relative costs changed considerably during the life of the project. In particular, programming costs fell as standard, multipurpose statistical packages became available, and computing costs declined with the introduction of more powerful machinery and time-sharing operations. Thus, the early concern with computing costs reflected relative prices at the time rather than the prices which characterized the project several years later. It is not clear that relative costs will now be stable for a long time, but they are unlikely to change as much in the next decade as they have in the past decade.

E. Organization of the Coordination

Discussion thus far has emphasized the division of labor between the Coordination and the institutes; but the project also required that the Coordination's own work be properly organized, especially once the staff came to include a full-time programmer and four research assistants with responsibility for data from eight different countries. After some experimentation, a division of labor emerged which worked satisfactorily for several years. Three principles on which this organization was based merit a brief discussion.

First, it was necessary to separate sharply the computer programming tasks from research assistance with the data. Initially these jobs were mixed since it seemed reasonable that programmers should help determine how to organize the data preparation; this, however, turned out to be wasteful. It proved more efficient to have researchers specify what results were needed and let programmers decide how to obtain them. Yet, communication between researchers and programmers

was still essential, especially since one programmer might be working for several different research assistants. The present availability of statistical packages does not entirely remove this question from consideration, since organization and cleaning of data may require custom programming. Moreover, a programmer's help can still be very valuable at the early stage of questionnaire definition and coding.

Second, it was most useful to assign individual research assistants the responsibility for particular countries rather than, as was initially attempted, for particular stages of the work (and hence for the use of particular computer programs). While this arrangement is consistent with the first principle, the reasons for its adoption took into account other factors, namely: 1) it let individuals become extremely well acquainted with the data for particular countries, and 2) it let each research assistant see the data through all stages to final results. No one was assigned only the uninteresting stages of the process and no one had to wait for the person responsible for a previous stage to finish it and pass the data along. Thus, aside from the psychic benefits, the Coordination's time was used more efficiently.

Third, it was necessary to offset somewhat the practice of specializing by countries, by promoting communication among the research staff, including the programmers. This principle had two motives. One was to share discussion of problems which, having arisen over one country's data, might be expected to arise over others' as well. If a common solution could be found, it was important not to have each assistant deal with the problem separately even though each might have to adapt the solution to particular problems of the data for

which he or she was responsible. 1/ A second motive was to stimulate the staff and keep the goals of the project in view. In a long and often tedious study such as this, the quality of the results can be very sensitive to the diligence and enthusiasm of the individuals working on the data.2/ When the assistants are themselves students of economics, eager to work towards a PhD thesis or a publishable analysis, they of course share in the benefits of apprenticeship that were also enjoyed by several of the institute participants; but during the several years of data-cleaning and standard analysis none of the assistants in the project had that motivation.3/

1/ Problems which had been foreseen and discussed at siminars were, of course, known to all the staff; the difficulty lay with surprises which surfaced in the data processing and required action without waiting for a full discussion among the project participants. There were many such surprises.

2/ The problem of motivation was complicated by the fact that, for the reasons discussed in Section IIIA, processing was never finished for some countries.

3/ One of the four subsequently used the data for a PhD thesis (Lamas, 1981). His earlier experience was undoubtedly crucial to that decision.

IV. SUMMARY REFLECTIONS

This presentation of the ECIEL consumption study experience has emphasized the difficulties faced, whether intrinsic to the project or arising from exogenous circumstance. The reason for this is partly because the study's success has already been well documented in the substantive publications described at the end of Section I. It also reflects the belief that any institution or group contemplating a similar research program can gain more from a warning of potential problems and an offer of possible solutions than from an emphasis on only what worked well.

A. Relevance of the ECIEL Example

An argument can be made that the ECIEL experience is hardly relevant today and, hence, that there is little to learn from it. At least three distinct justifications are worth considering. One is that because it was such a loose, voluntary arrangement with few formal contracts or clear lines of command, the ECIEL Program created for itself problems of heterogeneity and cost-shifting that could have been avoided with a tighter organization. Similarly, the lack of firm agreements at a high enough political level exposed the study to political shocks and interference more than was inescapably necessary. There is definitely some weight to this argument, but less than may appear. First, it would be hard to design any agreement that would guarantee against the overthrow of governments - it should be remembered that no fewer than five countries initially participating in the study were affected by golpes or comparable changes during the period, with adverse effects on the study in three of those cases. Second, even a stronger organization such as the World Bank or a United Nations agency which has the support of member governments must still negotiate agreements with participants in such a project, and cannot really enforce compliance. At best it can apply sanctions much more forcefully than ECIEL could.

In such a case, however, the consequences for the study - the loss of a participating institution or country - would be the same.

A second line of argument is that facilities for computing are now more widespread, more advanced and more stable - that is, their costs and capacities are changing less rapidly - than was true a decade and a half ago. This is certainly the case with respect to the availability of convenient statistical programs and of persons trained in their use. In consequence, no one should have to repeat the ECIEL experience of creating almost all the computer programs for such a project. It will still be necessary, however, to create special programs to clean and standardize data. More importantly, progress in computing capacity is still not universal: many countries and institutions today are in essentially the same position that several ECIEL institutes occupied fifteen years ago. 1/ Even where machines and programs exist, most programmers have relatively little experience with the problems of such a survey. And where some capacity exists but is hardly adequate for the project, the choices of centralized versus decentralized processing and of research versus training will again arise.

A third line of argument is that, despite specific difficulties, a research project of this nature now faces a much more propitious climate than before. Household surveys are now a standard research tool; political suspicions have diminished through experience; less novelty and more professionalism exists in such work; and so on. This is a hard argument to evaluate because on the one hand, at a certain intellectual level, it is largely correct. On the other hand, however, it is hard to detect any systematic abatement of the kind of bureaucratic jealousy that can still interfere with such research, and as with other aspects, there is enormous diversity among countries and institutions. Some

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1/ Judging from personal experience in assisting similar surveys in recent years, this is true even within Latin America, and it is just as likely in other continents.

political or ideological reasons seem clearly to have retrogressed during the last decade. Finally, even if it is in some general sense easier now to conduct research such as ECIEL pioneered, it does not necessarily follow that international collaboration has also become easier. The build-up of capacity in less developed countries may even cause some of them, or some of their agencies, to be less willing to accept foreign tutelage or coordination.

If, then, there are reasons to think that the ECIEL experience still contains some useful lessons, these should be explicitly set forth. Three topics are considered in the remainder of this section.

B. Definition of the Political Context

Even if little can be done to protect against political upheavals, no coordinator of such a project should suffer quite so much political vulnerability as ECIEL had to accept. It is probably advisable to secure understandings at high political levels. When dealing with a government agency, such high-level agreements offer some protection against bureaucratic indifference or politicking but they may possibly also be valuable to forestall interference with private universities or research institutions. An institution such as the World Bank already has standard procedures and channels for such agreements. All that is required is to specify clearly the terms of collaboration so as to avoid problems of cost-shifting or disagreements over compliance later on. Such specifications should not extend in great detail to the analyses to be performed or results to be obtained, since this could interfere with flexibility in research; rather they should concentrate on definition of the data to be collected and on access to them, terms of publication, and any training component to be included with the research.

It must be stressed here that a bias toward public institutions and formal, governmental agreements involves significant dangers. Bureaucratic difficulties

in such entities are typically more severe than in private universities or research institutions, while intellectual commitment may be less. Even high-level agreements can be overturned by changes of government or of personnel, or they can prove very difficult to enforce in the face of delays. The best prescription is probably to associate with the institution(s) in a country which are technically most competent and then secure as much political commitment and protection as possible.

C. The Right Degree of Uniformity

Much of the unanticipated cost - in both time and money - of the ECIEL study was due to the effort to create uniformity out of heterogeneous data sets and then to specify and conduct uniform analyses. Defining how much uniformity is desirable and at what stage of the project it should be imposed is, therefore, a crucial determinant of how difficult the project will be as well as of how comparable its results will be. Here some simple rules may be useful.

First, if new surveys are to be undertaken in several countries, they should be made as nearly identical as possible. This is most important for the questionnaire content, but uniformity in sample design is almost as desirable, since the exact structure of the sample affects the analyses which are feasible as well as their possible interpretation. 1/ There is no reason to tolerate the degree of heterogeneity accepted in the ECIEL study: an intuitive "closeness" is still a long way from identity. This is one instance where the coordinating institution can justifiably force its collaborators into more uniformity than they would choose, at least for a core set of questions and a basic sample design. Country-specific "modules" or supplementary questionnaires, or country-specific modifications of the sample design, would then be easy to accomodate provided they

1/ For example, the ECIEL samples ranged from almost strictly proportional to extremely non-proportional, with probabilities of selection in one income stratum more than ten times higher than in another. For analysis of the entire sample, these differences are corrected by the use of weights; but if it is desired to study different strata separately, the different samples have quite different standard errors even for equal overall sizes.

did not introduce any changes into the core.

Second, if the project is to be based on existing, already different, surveys, then it is probably a mistake to invest the effort into creating uniformity at the level of data. Where simple transformations can be defined to make variables coincide, they should be used - but this effort should not be pursued where the differences in the data are difficult to reconcile. Instead, attention should shift to the stage of the analysis, and the effort should be directed to defining models and calculations which will be relatively insensitive to the data differences. Uniformity of data is a sufficient condition for strictly comparable statistical results, but it is not necessary for many interesting lines of research.

Third, whether or not the data can be made uniform, experiments should be conducted with slightly different definitions of a concept. This is of interest for any one country's analysis, and it helps clarify international differences. Some such experimentation was undertaken in the ECIEL study, especially in studying the relation of consumption to income and in defining poverty, but more would have been desirable. And the marginal cost of such experimentation is trivial compared to the marginal cost of variation in the survey design or content.

Last but not least, a balance needs to be struck between analyses which are identical across countries and those specific to one country or set of data. As described in Section III, the ECIEL project initially over-emphasized the former partly because 1) the goal was always to produce results that would measure real inter-country differences instead of artifacts of the data, and 2) failure to achieve uniformity in the questionnaires increased costs. This emphasis was offset later by the specialized studies (including doctoral theses) for which the data were used. Nonetheless, the balance failed in two senses:

a) the participating institutes were not often able to share in the experimentation with non-standard results, and b) experiments that gave interesting results for one or a few countries could seldom be extended to others due to lack of resources. Planning for the project should view these considerations seriously, providing resources for pilot studies and for replication of results. (Research based on a country-specific questionnaire module would, of course, not be replicated.)

D. Bottlenecks and Scarce Factors

The ECIEL experience permits two unambiguous conclusions on this subject. First, the scarcest resource throughout the project was human analytic capacity. It is true that early in the project there were great difficulties with computing, and that some notable false economies were pursued in dealing with this bottleneck. However, the computer was never a problem in and of itself. The shadow prices of other factors never fell to zero, because computer bottlenecks were invariably programming problems as well, and the difficulty of writing and testing programs was, in turn, a function of the difficulty of specifying just what was to be computed. As the machine and programming difficulties diminished in the course of the project, the pressure on analytical capacity increased. This was partly due to the weakness of several of the participating institutes, which forced more work onto the Coordination; and it is certainly a problem that can be alleviated with the participation of more analysts. It should still be expected, however, that analytic ability will be the worst, or most-frequently-occuring, bottleneck in any such project. It is much easier to estimate a mass of regressions than to interpret them, much easier to handle data than to keep track of what they mean.

The second conclusion is that since bottlenecks will occur and capacities of one or more factors will be exceeded, it is essential to define how these problems will be resolved and who will pay the cost. In the ECIEL project, the Coordination became every institute's reserve of virtually every resource, except - in some cases - money. Such a situation was unfortunate for both the level and the sharing of the costs, but it was probably crucial to the project's success. If in a similar undertaking some one institution is to play that role, controls will be necessary to keep costs within bounds and to clarify when and how problems can be shifted to it. If no institution is the supplier of last resort, the initial negotiation of the project should specify at least generally how bottlenecks are to be treated and whose responsibility they will be.

E. Benefits and Costs

Both the benefits and the costs of the ECIEL study have been discussed in several contexts throughout this paper, typically with reference to interactions between different kinds of benefits (such as research vs. training) or different allocations of costs (between the institutes and the Coordination). Factors tending to raise total cost, in time or money or both, have also been emphasized; however, it is impossible to provide a systematic accounting or balancing. Primarily, this is true because the benefits of a research project are extremely difficult to measure. Few if any are sold to external consumers. Further, there is much leeway for subjective differences of evaluation, not only between insiders and outsiders but also among those who participated in the project. Monetary costs are considerably easier to measure, but even they would be virtually impossible to judge precisely. They would have to be compared in many different currencies, requiring in most cases shadow exchange rates; and inflation over a decade, very rapid in some countries, would necessitate the construction of indices for which little basic information exists. Non-monetary costs

present much less of a problem than non-monetary benefits, but a full accounting should still take some account of divergences between resource cost and money outlays, particularly those caused by unexpected delays, shifts of personnel, etc. Finally, any balancing of benefits against costs would require some choice of a discount rate, which would greatly affect the apparent value of the project since both streams were spread over many years and there was an interval of several years between the bulge of costs associated with the surveys and the bulge of benefits realized in publications.

If a complete accounting is impossible, it is perhaps also not very relevant to the conduct of similar research in the future. The unit costs of various elements of the project have changed so much in the last decade and a half that any monetary figures could be quite misleading, even if correction for inflation and purchasing power parity were possible. Computation is the clearest example of such cost changes, but prices and productivities have undoubtedly changed for other components as well.

Despite these difficulties, it may be valuable to conclude this review of the ECIEL experience with some further remarks on costs and benefits, quantified where possible. A first observation is that a very rough comparison suggests the ECIEL consumption study was not any more expensive than a similar survey carried out in the much more favorable conditions of the United States. The ECIEL study spent something of the order of five million dollars (without adjustment for purchasing-power parity) to collect about 10,000 observations of about 1,000 variables each: the unit cost of an observation of a variable was, therefore, in the vicinity of fifty US cents, circa 1970. In 1972-73, the US Census Bureau conducted for the Bureau of Labor Statistics a two-year consumer expenditure survey which cost upwards of fifty million dollars and collected somewhat

more than 1,000 variables from each of 68,000 households: thus, the cost per datum was of the same order of magnitude. This comparison is far too crude to tell which survey was more expensive, but it does serve to indicate that ECIEL did not suffer unit costs higher by an order of magnitude. Either the ECIEL project was conducted relatively efficiently or the difficulties caused by including many countries and three languages were offset by lower costs for many components, especially in the fieldwork. In this comparison, low real wages of interviewers, coders and other relatively unskilled personnel are of course, exaggerated by the favorable exchange rates of the late 1960s; the dollar value would be considerably higher if the study were repeated today. Also, if the comparison is based on unit cost per datum carried through to analysis, and not simply collected, the abandonment of work in Argentina, Bolivia and Uruguay raises ECIEL's costs somewhat. The general conclusion, that unit costs were comparable, would not be affected.

Second, it is possible to quantify roughly the personnel resources required for the project's coordination. Through the end of the standard data processing - that is, the eight years from mid-1966 to mid-1974 - the Coordination required: eight man-years of relatively skilled labor (the Technical Coordinator), supplemented by perhaps one man-year of computer programmers' time (nearly all of it provided by one person); roughly twelve man-years of research assistants' time, the staff growing from one to two to four people at about two-year intervals; and close to six years of a secretary's time. On the assumption that skilled labor cost about twice as much as unskilled labor, the total input was approximately 42 man-years of less-skilled labor, or half as much if measured in higher skill units. Such a comparison should not, of course, be taken to mean that the two kinds of resources could be substituted

at a 2:1 ratio throughout the project; and it does not adequately take account of the great increase in the quality of both kinds of labor as experience accumulated.

This accounting of the Coordination's human resources requires three important clarifications. In the first place, it does not include about another three man-years of each kind of labor, devoted after 1974 to the preparation of publications. In the second place, at least three-quarters of these resources were required because the data were cleaned and analyzed by the Coordination; under a different division of labor, comparable amounts would have been distributed among the institutes. Finally, it is a reasonable guess that about two man-years of skilled and four man-years of less skilled labor were consumed by the extremely difficult problem of harmonizing the data from different countries. Making all three of these adjustments gives a very rough estimate of the resources required for "pure coordination" under more efficient (more uniform) initial conditions: about four man-years of each kind of labor.

Less can be said about the project's use of human resources in the institutes because many participants had other duties as well, such as university teaching, administration or work on other statistical efforts. Once the field work was completed, some institutes probably used as much as four man-years of relatively skilled labor to oversee the data-cleaning and prepare analyses for publication. Other institutes contributed substantially less, or labor of substantially lower quality.

A third observation is that the benefits of the project became available at very different times. Thus, changes in the discount rate used to sum up benefits (if they could be quantified) would affect the total as well as the relative importance of each kind of benefit. Training in the art of survey fieldwork

was the first benefit realized, followed as quickly as possible by the obtention of weights for a consumer price index. 1/ Other kinds of training and of statistical results came later. It appears that training was more nearly continuous throughout the project whereas the statistical output was bunched toward the end, so that, on average, training came earlier. For a given institute or country, the two types of benefits were highly complementary: those that learned less from the project also made less use of the results, in both cases because of incapacity. There were no institutes in the ECIEL group for which only the results mattered - that is, none could not benefit from the training opportunities. As was argued above, individual formation of human capital and "learning" acquired by a whole institute were also highly complementary.

Fourth, some of the training or experience acquired by individual participants in the project was rapidly returned to the study and institutes as a benefit when those individuals moved to positions in their governments from which they had more power to promote or hinder research. Particularly in the first generation of participants, there were perhaps as many as a dozen persons who became institute directors, heads of bureaus in government agencies, or even Cabinet ministers before the project was completed. ECIEL can hardly take full credit for their rise to those positions, but the project did increase their acquaintance with, and enthusiasm for, empirical research.

1/ These weights were a chief justification for the project in the eyes of the government agencies which conducted the surveys or helped pay for them. Since both national governments and the ECIEL study of price comparisons urgently wanted the weights, they were usually based on the initial, questionnaire-level uncleaned data. The implicit rationale for this short cut was the expectation that errors in the data would have a zero mean or "cancel out." This is an attractive but very dubious assumption in household survey data.

A fifth and final observation concerns the ultimate benefits, if any, attributable to such research. In one view, these seem to be of two rather different kinds. One is the increase of knowledge concerning how the world works, the "pure" or scientific or academic objective of research. The other is the use of that knowledge, or of particular pieces of information, for what are usually called "policy purposes" - that is, having some entity, usually a public agency but sometimes a private enterprise or a non-profit organization, act differently and in such a way as to increase economic welfare in some manner. If these are the ultimate criteria, then training is simply an intermediate product, valuable only so far as it contributes to increased knowledge or welfare. From a much narrower viewpoint, often adopted by governments and funding agencies, only the last objective matters, and even the increase of knowledge should be considered an intermediate product. This utilitarian approach gives no direct weight to the effect of research on subsequent research, to the "impact" of publications through their citation and use by other investigators, or to similar consequences of a study.

From the experience of the ECIEL consumption project, it seems clear that the benefits look larger as a greater number of different kinds of benefits are treated as final products, valuable in their own right. At least three reasons support this tendency. First, with some possible exceptions in early governmental use of statistical results, the training and publication benefits - the increase of knowledge - preceed the application of knowledge to policy issues. Second, while those two kinds of benefits are difficult to measure, 1/ they

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1/ One can define units of both types easily enough - man-years of apprenticeship or course-equivalents in one case, number of publications, or pages published in the other - but the fundamental problem is one of quality, which is hard to measure without some tests of output, such as examinations, journal evaluations, etc.

can, nonetheless, be quantified a little better than "impact" in the world of policy, where changes in laws or public actions respond to many different pressures and increased factual knowledge is often relatively unimportant. To the extent that policies are shaped by a whole intellectual climate, it is also difficult to connect specific research results to specific actions. The final reason is simply that while the first two kinds of benefits were, to a large degree, under ECIEL's control, the third was not at all. Training people, and producing knowledge are not easy tasks, especially when there is little control over the inputs of human resources; but, at least, a monotonic connection exists between resources devoted to those tasks and the benefits obtained in return. That relation is very hard to discern for the third kind of benefit. If the benefits actually obtained are so far rather small, the reasons lie almost entirely outside ECIEL and the academic community in general.

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