DOCUMENT RESUME

ED 140 025 CE 011 294

TITLE	Evaluating the Provision of Employer Services: A Methodology. Final Report.
INSTITUTION	Camil Associates, Inc., Philadelphia, Pa.
SPONS AGENCY	Employment and Training Administration (DOL), Washington, D.C.
PUB DATE	Sep 76
CONTRACT	DL-20-42-75-43
NOTE	128p.
EDRS PRICE	MF-$0.83 HC-$7.35 Plus Postage.
DESCRIPTORS	Cost Effectiveness; *Employers; *Employment Services; Evaluation Criteria; *Evaluation Methods; Federal Programs; *National Surveys; *Program Evaluation; Research; Research Design
IDENTIFIERS	United States Employment Service

ABSTRACT

A research project was done to develop a methodology for a national evaluation of the employer services program conducted by the Employment and Training Administration (ETA) at State and local levels. Two program evaluation methodologies, each requiring different approaches and resource expenditure, were developed that could provide the needed information about the effectiveness of the provision of employer services, the customer relations component of the U.S. Employment Service (ES), which promotes the use of the ES among community employers by encouraging or directly soliciting job listings. Section 1 of the report is an introduction covering basic assumptions, reasons for developing two methodologies rather than one (The request for proposal called for a net impact evaluation while the contractors thought a survey technique was best), and the organization of the report. Section 2 provides a review of the project, tracing the steps in the development of the proposed methodologies (includes sources of information, types of information, and data collection techniques). Section 3 is a description of the purposes, structure, and processes of the employer services program and presents a categorization scheme for local office programs. Section 4 explains why experimental variation is necessary for independent net effect program evaluation and outlines the requirements of such experimentation. Section 5 presents the two alternative evaluation methodologies with net impact experiment being described in greater detail than the survey technique but both in depth considered to be sufficient to permit their use. A summary of 43 sources reviewed is appended. (EM)

EVALUATING THE PROVISION OF EMPLOYER SERVICES: A METHODOLOGY

FINAL REPORT

Contract Number 20-42-75-43

September, 1976

TO:

Employment and Training Administration
Office of Policy Evaluation and Research
Department of Labor
Patrick Henry Building, Room 9009
601 "D" Street, N. W.
Washington, D. C. 20213

CAMIL
ASSOCIATES, INC
37 S 13th St., 5th Flr
PHILADELPHIA
PENNA 19107
215-LO8-4700

This report was prepared for the Employment and Training Administration, U.S. Department of Labor, under research and development contract No. 20-42-75-43. Since contractors and grantees conducting research and development projects under Government sponsorship are encouraged to express their own judgement freely, this report does not necessarily represent the official opinion or policy of the Department of Labor. The contractor is solely responsible for the contents of this report.

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SECTION ONE: INTRODUCTION

Camil Associates, Inc., is pleased to submit this final report on development of a methodology for evaluating the employer services program. This methodology was developed under contract number 20-42-75-43 with the Employment and Training Administration, United States Department of Labor.

In the language of the request for proposal for this research, the goal of the national evaluation would be to measure "the independent net effects" of the employer services program. This requirement poses the greatest difficulties for methodology, and consequently it is to the subject of independent net effects that much of this paper is addressed.

Development of a research methodology involves rigorous consideration of numerous data-gathering and analytical alternatives. At many points in the development of our methodology -- as in any methodology -- we faced choices of approach. Most often, the decision was not between one clearly superior alternative and one clearly deficient one; rather, we were confronted with alternatives, each of which had its attractive and its troublesome aspects. At such points, we weighed each possibility in terms of its practicality, usefulness to the study, validity, and probable cost. We then

selected the path which seemed most feasible.

In this report, we have attempted to display such choice-points fully, so that the reader can consider the other options. The alternatives we "rejected" are presented in sufficient detail so that persons responsible for developing the national methodology could select them instead of the one which we preferred.

Our own preferences of approach are based on several assumptions:

- The goal of a national evaluation would be to aid USES in resource allocation to employment service activities and programs.
- Between two alternative methodologies likely to produce the same degree of validity and policy usefulness, the simpler is to be preferred.
- There are trade-offs between optimum precision and research costs. Some "better" approaches may be too expensive to be justified in terms of the actual information needs of ETA.

Working from these assumptions, we have tried to propose a methodology which will yield reliable results for resource allocation and planning purposes, at a reasonable expenditure of time and money.

As will be evident in the following pages, this report actually describes two methodologies for a national evaluation of the employer services program. That was not our original plan; our intention, as stated in the proposal and in earlier working papers, was to recommend a single evaluation approach, based on an inferential estimation of the contribution of the employer services program by means of management appraisal of local offices, and employer interviews. This approach was proposed considering the problems of time, cost, and the relatively minor (yet clearly

defined) role of the employer services program within the ES. The methodology was to be developed to the point that it would be possible to proceed immediately with a detailed implementation plan.

However, during the study, members of the project steering committee expressed an interest in exploring a more rigorous methodological framework, particularly by application of statistical net effect models. We proceeded to explore the potential for the use of net effect methods for an employer services program evaluation.

This was done somewhat reluctantly, not because of any bias on our part against net impact studies, but because a net impact evaluation would require a lengthy and expensive experiment, which we felt was inconsistent with the needs of policy-makers to determine the accomplishments of the program, and inconsistent with the magnitude of the program itself.*

Usually, when the basic need for a program is accepted, evaluators do not set out to perform a net-effect, zero-treatment model (one that attempts to determine if the program is "worth the effort"). Rather, one usually tries to optimize the program by means of a management appraisal, or some analytical technique to determine what program level produces the greatest pay-off. In the case of the employer services program, the problem with a net-impact approach is that the overall value of the effort is to be tested, yet it is not understood how employer services produce job listing behavior on the part of employers. Permanent modifications in employer use of the ES might be produced by a single ERR visit. Or, the ERR contact might produce little change in non-listing employers, but be essential

* For FY 1975, the employer relations program amounted to less than 5 percent of the total of employment service block grants; the technical services effort accounted for less than one-half of one percent. The percentages would be even lower if the total ES budget (incorporating WIN and other special monies) were used as the base, rather than the grants. Figures for FY 1976 are not available as of this writing.

to sustain the business of the employers who are already listing jobs with ES. Or, a reluctant employer might be convinced to use the ES by frequent visits from an ERR. Without any prior understanding of the critical intervention interval, one has to use a treatment/control experiment in which at least some proportion of the control group receives little service (or, preferably, none at all). If one is willing to accept the basic efficacy of the program, however, a marginal effect model can be employed which expresses the best way to provide services (rather than determining the value of the services at all). Frequently, this is well-suited to the actual information needs of program planners and policy-makers.

In any event, because of the expressed preference of the ES review task force for a net impact methodology, and our own belief that a "softer" evaluation would be both sufficiently reliable and at least equally useful for administrative purposes, we have developed both approaches in this report. The net impact experiment is presented in substantially greater detail than the survey technique; both, however, are described in sufficient depth to permit their being put into use in a national employer services program evaluation.

Because of the continuing difference of opinion about the efficacy of net impact methods for evaluations of this sort, we have taken the unusual step of devoting a section of this report (Section Four) to the problems and advantages of net impact evaluations. During the review by DoL of that section, several readers felt that its presence in the report indicated a bias on our part against net impact studies. This is not so; indeed, it would be a strange (and short-lived) research organization that permitted prejudice to rule out *any* useful study method. For each project, it is the task of research to find the appropriate methods and use them. There are many weapons in the arsenal of social research; all have their uses. That we feel the net impact approach is not best suited for the employer services evaluation does not indicate bias against that method in general. Our Section Four is not an

attack on the method, but a detailed discussion of the problems with using it in an evaluation such as the assessment of the employer services program. Net impact is not the only possible kind of evaluation (nor even the most desirable kind for every case). Our insistence on this point is scarcely bias; it is the opposite of bias, openness to a variety of possibilites.

Part of the problem may be due to a misconception about the "power" of the techniques associated with net-effect methodology. It was argued that a net-effect evaluation could be constructed from regularly reported ES program data, or with specially gathered data about the program, thus not perturbing the ES system itself. This is not the case. While there are some elements of ES operations that could readily be evaluated in that manner, the employer services effort is not one. The employer services program is inextricably imbedded within a complex ES structure. It is only one of a number of factors that can be associated with employers' job listing behavior, and certainly one of a large number associated with hiring decisions. To devise a net impact evaluation of the employer services program demands some manipulation of the program, some experimental design imposed on an operating system. This is not a step one takes lightly; indeed, our feeling that such manipulation would be unacceptable to the state employment services was a strong factor in our preference for another evaluation strategy. Local office managers are reluctant enough to let researchers into their offices to interview staff and review records. When it is a question of changing office procedures, that reluctance can be expected to intensify, particularly since state budgets are determined in part by success in meeting the standards of the balanced placement formula. While the design we put forward in Section Five of this report requires a less objectionable level of disruption than others that could be propcsed (such as eliminating the program entirely in some sites while increasing it heavily in others), persuading states and localities to participate is going to be no mean feat. If the experiment is carried out, consideration should be given to providing incentives to states to participate, or at least neutralizing potential disincentives (perhaps waiving BPF requirements for the

duration of the experiment).

With such accomodation, cooperation of states could probably be secured; once that has occurred, the rest of the experiment is straightforward, as described in Section Five.

Section Four, then, is a necessary digression, laying the groundwork for the presentation of a formal net-effect approach that could be used to determine the worth of the employer services program. It explains why experimental variation is necessary, and outlines the requirements of such experimentation. That discussion is not intended to dissuade the Department of Labor from such a methodology, but to show why its use in this case will be lengthy and expensive. In fact, apart from considerations of time and cost, we would opt for the net-effect approach ourselves. It is scientifically interesting, providing for an experimental variation in program operations that has not previously been attempted. But taking time and cost into account, we advise implementation of the simpler, direct observation method. We doubt that any planner or policy-maker needs such hard proof of the precise contribution of the program that only the most carefully designed experiment will do. An objective appraisal of the relative importance of employer services based on direct questioning of employers should suffice. But, if the net impact approach is preferred, we believe that the one we have designed will fill the need, at the absolute minimum possible disruption to regular ES procedure.

An entirely separate question, of course, is whether the information and policy needs of ETA justify an evaluation of the employer services program in the first place. This question is beyond the scope of our assignment; we have been asked to prepare a methodology which could be used should such an evaluation be performed. however, having spent considerable time weighing evaluative alternatives, we confess to a measure of doubt about whether a national evaluation of this program is going to prove to be "worth it," in terms of advancing ETA's knowledge of the effectiveness of the Employer Services Program significantly beyond what is already known and understood by state and federal officials responsible for the planning and

administration of this effort. This program is not a new undertaking. It has been an established part of the employment service for many years. Over the past few years, it has been subject to intense scrutiny both from within the USES, and from concerned employers who rely on the employment service for access to a pool of qualified job-seekers.* Experimental improvement designs have been implemented, and other significant changes made, as a result of this attention.

Through all of this -- and before -- the provision of employer services has been watched and worried over by USES, and notice taken of trends in any direction regarding changes in the levels of service provided. From time to time, provision of employer services has been roughly compared with changes in rates of listings, placements, and "penetration," variously defined. These monitorings, while falling short of any satisfying statistical evaluation, have served to enable decision-makers in ETA to flag disturbing trends in the program, and to initiate corrective action as needed. To get from this admittedly impressionistic approach to a determination of independent net effect with any degree of methodological confidence is, as the balance of this paper will show, a cumbersome and difficult operation. The question for ETA is whether the resulting advance in the state of knowledge about the employer services program will justify the expense, in time and money, of the evaluation.

The balance of this paper is structured as follows: Section Two provides a brief review of our project, tracing the steps in the development of our proposed methodology. Section Three is a description of the purposes, structure, and processes of the employer services program, and presents a categorization scheme for local office programs. Section Four is a discussion of "independent net effect." The concluding

* Congress has also maintained an interest in employer services. A General Accounting Office review of the employment service included an assessment of the employer services program; the results were reported to Congress in May. Some of the GAO findings appear in Sections Three and Five of this report.

section, Section Five, presents the two alternative methodologies. A summary of sources reviewed for this project is included as Appendix A.

In the course of this project, we have received extraordinary cooperation and assistance from numerous officials and staff involved in the employer service program at the federal, state, and local office levels. We made repeated visits to ETA offices to determine what data are available at the national office, and to interview program officials and other staff about employer services goals and outcome measures. The response to our mail survey of state ES officials was gratifyingly high, with all but two jurisdictions responding fully within several weeks of our initial mailing. An *ad hoc*, informal review committee representing several departmental organizations met several times during the project to review our progress, challenge some of our proposed directions, and contribute to the development of program goal statements and outcome measures. For all of this guidance and assistance we are sincerely grateful.

SECTION TWO: REVIEW OF THE PROJECT

The major task of the project was, of course, conceptual -- the systematic consideration of evaluation alternatives to arrive at a "best" approach from an array of possibilities. However, this essentially intellectual process had to be complemented by the gathering of considerable information and opinion from a variety of sources.

We began with a review of descriptive materials and previous research undertakings related to employer services. There was, we discovered, a wealth of data available; restricting ourselves to major studies and reports, we identified and reviewed some 43 documents and presentations of findings. We submitted a paper summarizing these materials, *Literature Search: Employer Services*, to DoL July 30, 1975.*

While that review was in progress, we also interviewed numerous national office ETA staff. These interviews had two broad purposes:

(1) To determine the nature, extent, frequency, and reliability of employer services data reported by the states to the national office.

* That paper appears as Appendix A of this report.

(2) To inquire of federal personnel involved with the employer services program their perceptions of the program goals, and their ideas about evaluation measures which would most appropriately provide an assessment of progress toward those goals.

Augmenting these national-level interviews, we sought similar information from state and local level ES staff, including details about the provision of employer services. Field visits were made to four jurisdictions -- three states and the District of Columbia -- and the remainder of the states were surveyed by use of a mail questionnaire. Key areas of inquiry were:

- Job descriptions (journeyman level) for employer relations and technical services staff.
- Descriptive information about state and local employer services programs.
- Methods used by state agencies to monitor and evaluate provision of employer services.
- Suggestions of state and local ES officials about evaluative criteria. (Question: "If you were evaluating the employer relations (or technical services) program, what performance criteria would you use?")

Response was extraordinarily good. All but two states replied to the mail survey, and most sent along additional materials: job descriptions, planning documents, monitoring and evaluation formats, and other helpful information.

The information gathered from the site work and mail questionnaire was used to develop a structured

description of the employer services program (as discussed in the following section, Section Three), and to determine what program objectives and performance measures, implicit or explicit, are viewed by program operators as most consequential.

In addition, the information from state and local source was reviewed for its implications for development of the methodology for a national evaluation of the employer services program. Among the significant points were:

- Variation in program goals and approaches from one state to the next, or from one local office to the next, are slight. The chief variations are in program size (number of staff, level of effort) rather than in philosophy, objectives, or techniques.

- Such local office variations in approach as do exist are frequently temporary, and addressed to the correction of specific, short-term situations. For instance, a local program may concentrate for a time on "trouble-shooting" with dissatisfied employers, or on breaking into a new industrial area. Several states were experimenting with a concentration on small employers to increase the variety in types of jobs available. Others were trying to identify and work with industries less affected by the recession than others. Over time, though, local employer services programs are much more alike than different, with regard to what they do and how they do it.

- There is widespread awareness among state ES officials of the experimental program variations,

particularly the Employer Services Improvement Program (ESIP). Even in states not formally participating in such programs, there has been movement in the direction of the key facets of the experiments, notably involvement of employers in ES planning and service provision, increased use of mass media advertising to enhance the "image" of the employment service among employers, upgrading of employer relations representatives, and use of monitoring and follow-up to gauge employer satisfaction with the employer services effort, and the ES in general.

- Provision of technical services is uneven; as a generalization, technical services represent an extremely small, and declining, portion of local office operations. There are striking exceptions. however; one state has 27 fulltime technical services staff at local ES offices, and 13 other states have at least some fulltime technical services personnel.

- While a discrete technical services program may be on the decline, some 11 states are providing, under the umbrella of the employer relations program, some technical services: testing, job analysis, and in-plant studies. Characteristically, these services are provided casually as part of the employer relations effort, by ERR's, and are not usually recorded in state reporting as technical services.

- Despite their diminishing role in the employment services, in some states technical services are far from dead. Of the 13 states with

some designated technical services personnel, six expect the program to grow over the coming five years, and none expect it to decrease. Typical of the reasons given: "When employers find out what we can do, there will be an increased demand. These services are not widely known." "New industries, such as those in the energy field, are creating brand-new jobs which need job definitions, test development, and other services."

- All states have some mechanism for monitoring and evaluating the effectiveness of their employer services efforts, and in most (43 states) evaluation is accomplished through a formal system of monitoring at the local office level, and on-site appraisals by state personnel. Formal, written procedures for evaluation have been developed in most states; occasionally, an entire handbook on the subject has been created and distributed.

- While evaluation approaches vary, a typical state strategy combines process evaluation (an examination of the "mechanics" of the provision of employer services) with outcome assessment (some measure of employer use of the ES). There is also a considerable, and growing, use of employer satisfaction measures, through follow-up visits with employers who use the ES as a hiring source. In some states, supervisors contact a random sample of employers visited by each ERR to check performance as viewed from the employer's perspective. In other cases, committees of employers

> review the employer services effort periodically, and perform special reviews of particular ES emphases (for example, a Christmas promotional program for retailers).

Responses to the question about suitable evaluation criteria for the employer services effort varied considerably. Some respondents felt the evaluation would have to be largely intuitive. One wrote:

> Keep it loose. Because of differences in nature of program in various states and specific services provided, a rigid evaluation format would not be desirable. The number of visits is not as important as quality. Service is very time consuming sometimes, so a mere count of visits doesn't tell much.

Other state representatives focused chiefly on process measures: number of visits, number of telephone calls. Many states set goals for employer services staff in regard to number of contacts, and staff are evaluated, at least in part, on their success in meeting those goals.

However, the measures most often mentioned were indicators of employer use of the ES: listing activity, and hiring of applicants referred by the ES.

A number of respondents indicated that their use of placement as the chief employer services evaluation criterion was a reflection of the national placement emphasis of the employment service. (Wrote one: "Things like keeping employers satisfied don't count.") Whatever the federal influence, most states seemed to feel that employer services -- in both its ERR and technical services manifestations -- must result in increased employer use of the ES to be justifiable. In determining effectiveness of the program, states were looking to such factors as:

- Proportion of employer's openings listed with ES.

- Proportion of job categories employer lists with ES.
- Proportion of employer's vacancies filled by ES referrals.

Even while discussing such measures, though, five respondents indicated that the recession had slowed hiring activity to the point where evaluating employer services on listing and placement alone would unfairly reflect on staff who were doing a good job under difficult circumstances.* Others said employer services staff (particularly technical services representatives) had been diverted to other office tasks, including unemployment insurance intake.

In sum, while those preferring placement-related evaluation measures constitute the majority of respondents, that preference is by no means unanimous, with employer satisfaction measures and process measures also frequently suggested and used by the states themselves.

As the information from the states was being examined, we re-interviewed a number of national office ETA staff to report informally on key findings, and discuss our developing approach to the evaluation methodology. An interim position paper, *Evaluation of Provision of Employer Services: Preliminary Approaches*, was submitted to ETA December 22, 1975, and discussed in a group meeting of DoL staff involved in employer services, or in broader methodological issues, on February 5, 1976. Debate primarily involved selection of outcome measures, alternative approaches to determining independent net

* One significant motif of both the field work and the mail survey was the pride states take in their employer services staff. To a question about program strengths, most states immediately responded in terms of ability of staff to relate to, and "sell" employers on using the ES. Some states went into detail about how ERR's (and office managers as well) were active in the community and particularly in civic and business organizations that bring them into freqeunt informal contact with important employers.

effect, and the use of cost analysis in the proposed evaluation. Each of these points is discussed in detail in later sections of this paper.

In summary, the project entailed pulling together information about the current state of the employer services program, acquiring the opinions of federal, state, and local manpower officials and staff about program goals and expected outcomes, and discussing alternative evaluation possibilities with persons expert in both the operations of the program itself, and the field of evaluation methodology. Out of all this, we developed two methodologies that we believe can provide needed information about the effectiveness of the provision of employer services.

SECTION THREE: THE EMPLOYER SERVICES PROGRAM: DESCRIPTIVE

The employer services program is, in effect, the customer relations component of the employment services. It promotes use of the ES among community employers, encouraging or directly soliciting job listings, and it is the ES' chief means of communications with current and prospective users.

The relative importance attached to employer services has waxed and waned over the decades, but an employer services effort has always been considered integral to the work of the employment service. It consists of two parts: Employer Relations, and Employer Technical Services. The definitions of these parts (from the functional activities definitions used by the employment service for staff time codes) are as follows:

(1) Employer services: includes time spent in developing and maintaining contacts with employers, including employer and union organizations. This includes personal, telephone, or mail contacts of a general promotional nature to

develop job opportunities for all applicant groups or to promote acceptance of or participation in special manpower programs.

(2) Employer technical services: Includes all time spent in providing advice or assistance to the employer, community and other government agencies in the identification, alleviation, or resolution of manpower problems in the area of work force selection, development, utilization, and stabilization. Also included are the use of tools and techniques for providing assistance on hiring policies; development of job specifications and related materials; onducting job analyses, job restructuring and upgrading studies; identifying training and testing needs; providing labor market information; interpreting manpower technical services.

While, in practice, the division between the two is not always clearcut, it is obvious that nationally the employer relations portion of the program dwarfs technical services, with employer relations accounting for about 95 percent of the overall employer services effort and money. In most states, the technical services effort is either absent or negligible. However, with the decline of a formal technical services program has come an absorption of some activities formerly under that rubric into the employer relations effort, notably guidance and help in preparing affirmative action plans and other compliance instruments, testing, and occasional in-plant studies. In most states, to speak of the employer services program is to speak, for all practical purposes, of the employer relations program.

3.1 GOALS OF THE PROGRAM

In Camil's interviews with federal ETA officials and staff, and -- to a lesser extent -- with state and local ES personnel, it was clear that the present-day emphasis in the employer services program is what might be called business development -- the securing of new listings and hirings from employers. The program is primarily valued to the extent that it can be shown to produce "payoff" as expressed in listings and placements.

While this focus is logical, and certainly consistent with the current ES emphasis on placement, it may be unnecessarily one-sided. Unarguably, an employer services program which seemed to have no beneficial effect on employer use of the ES -- or which seemed to have a deleterious effect -- would be of doubtful worth. But whether employer use of the ES should be the sole criterion of program accomplishment is questionable.

Any sizeable enterprise which provides products or services needs some kind of customer relations program. There has to be some way of maintaining a favorable impression, of responding to customer dissatisfaction, of enabling mutually beneficial communication -- not simply to hustle business, but also to keep present customers interested and aware of services.

That is true of all enterprise. It is doubly true of the employment service.

The employment service is, after all, a public service -- paid for in large measure by employers' taxes. They are entitled to it, without any corresponding obligation. And to the extent to which it meets their requirements and expectations, it has value apart from the listings and placements secured.

This point is supported by an examination of the objectives of the employer services program, as outlined in the *Employment Service Manual:*

For Employer Relations

- To promote the full use of employment service facilities and the prompt placement of applicants by assuring

that the desired kind and quantity of job orders are obtained from, and that needed technical services are available to, employers.

- To obtain current information about labor needs, employment opportunities, personnel policies, job requirements, and working conditions necessary for planning and providing services both to employers and workers.

- To identify the specific employment needs and problems of individual employers, and to interpret to them, in terms of those needs, the appropriate services available through the local office.

- To encourage and facilitate the employment of all applicant groups in the community on the basis of qualifications.

For Technical Services

- Assist in resolving problems relating to recruitment, selection, and assignment of workers.

- Assist in achieving full utilization of the skills and potentialities of the work force.

- Assist in securing desirable work force stabilization.

- Assist in developing the manpower resources needed for technological advancement, economic expansion, or national emergency.

Examining those objectives, the *employer relations* side shows a mix of emphases. There is the mention of "prompt placement" right at the top

of the list, establishing a link between the ERR effort and placement. Listings are similarly emphasized in the mention of "the desired kind and quantity of job orders." Looking at this objective through the evaluator's eye, it seems fair to expect the ERR program to "pay off" in terms of listings and placements.

However, the next two objectives are phrased strictly in language about services provided to employers, and the fourth deals with the somewhat different issue of serving *applicants* (though it could be argued, of course, that in facilitating employment "of all applicant groups" the ES is at once serving the applicant and the employer).

The *technical services* objectives, though, relate directly and exclusively to services which ES provides employers; there is no implicit *quid pro quo* that such services are to lead to increased listings or placements. The evaluation measures implicit in these factors would clearly require consideration of the opinions and behaviors of employers to determine whether such objectives are being attained.

We do not maintain that listings and placement are not appropriate measures of the effectiveness of employer services; indeed, as later sections of the report show, they are used as key outcome measures. Our argument is that they cannot be the *only* criteria of success. An examination of the role of the program, and its own formal objectives, demands consideration of employer perceptions and satisfaction as well as the presumed "bottom line" of listings and placements.

3.2 OPERATION OF THE PROGRAM

As suggested in the preceding section, differences between local office employer service programs are not so great that it is impossible to define a "typical" program. Before attempting this, though, it is worth considering what the chief variations are.

The biggest difference between programs is, by far, size. This apparently unexceptional observation

actually came as somewhat of a surprise, since we expected considerably more programmatic variation, in terms of approaches and methods, than was actually discovered. In the states visited,* programs were remarkably similar except for their size and other factors that are directly associated with size.**

The degree of size variation was noted by GAO in its May 1976 report on ES operations.*** Said GAO:

> . . . At the time of our review, the Department had not established staffing standards for employer relations at local offices. Understandably, the level of local employer relations activities varied considerably at the locations we visited.
>
> ***
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> In Philadelphia and New Jersey, employer services representatives were assigned by local offices. At the Philadelphia suburban office, two representatives were responsible for about 500 small employers and 150 larger employers in the area. They visited about 15 percent of the small employers and a third of the larger employers each month. In addition, they contacted about 200 more employers by telephone each month. In contrast, the Camden and Burlington, New Jersey, offices devoted few resources to employer services. At the

* Only in the site work did we get to the local office level. The mail inquiry went to state-level administrators only.

** In federal program assessment, it must always be remembered that many "programs" which sound complex and grandiose in guidelines, boil down at the local level to some portion of a single staff member's working day.

*** Statement of Gregory J. Ahart, Director, Manpower and Welfare Division, before the Subcommittee on Manpower and Housing, House Committee on Government Operations, on the Operation of the Federal-State Employment Service System.

Camden office one representative was assigned to service all 4,700 employers in the area; while at the Burlington office with 3,000 employers in the area, no employer representative was assigned.*

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The office serving the Los Angeles suburban area had approximately 18,000 employers in its jurisdiction -- three representatives serviced the area. Each month they visited about 400 employers in person and called about 100 others. The office serving the urban Los Angeles area allotted 4.5 personnel positions for serving 27,000 employers and making 500 contacts each month. If the new Department employers relations guidelines** had been in effect during fiscal year 1975, the office serving the Los Angeles suburban area would have to have contacted between 1,500 and 2,800 employers each month, and the urban office between 2,300 and 4,100. Services officials stated that additional personnel were not available to provide the needed employer relations services.

* In this statement, GAO leaves the impression that those 3,000 employers are left unserved by any employer services outreach. It is far likelier that many receive contact from other local offices, and from the district office level. The State of New Jersey assigns employer services staff (Manpower Specialist III) at the central office, district office, and local office levels. Further, many of the 3,000 employers cited hire from beyond the vicinity of that Burlington local office; Burlington is in the Philadelphia SMSA, only 15 miles from the Camden office reviewed by GAO, and 10 miles from the state capitol at Trenton.

** GAO is referring to guidelines recommending that local offices should contact 25 to 46 percent of the employers in their areas. This recommended quota includes both phone and personal contact.

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Denver maintained a higher level of employer relations activities. Both offices in Denver were served by staff assigned to the same regional office which was responsible for about 20,000 employers. The office was staffed with an employer relations supervisor, nine employment service representatives, and four staff assistants. In a typical month the staff contacted about 1,200 employers by telephone or in person.

Broadly, there appear to be three levels of employer services programs in local offices; for the purpose of this discussion, the levels are described in terms of the employer relations side of the program.

- *Level 1:* No ERR* staff *per se*. Employer relations carried out occasionally, and largely informally, by such other staff as interviewers, Veterans Employment Representatives, or counselors. An office with this level of employer relations activity typically has fewer than 15 staff persons, including Unemployment Insurance personnel. That staffing level represents the majority of ES offices nationwide.

- *Level 2:* One to three ERR's, working independently by dividing up employers on geographical, industrial, or other basis. No organized employer services

* States vary in their designation of employer relations staff, generally adopting "ERR" (employer relations representative) or "ESR" (employer services representative). For consistency in this report, we arbitrarily chose the more traditional "ERR."

"unit." A local office with this type of structure probably has a total of 15 to 40 staff persons.

- *Level 3:* Fully developed ERR unit, with a fulltime director. Staff assigned to employer "territories" as in Level 2. This is the likeliest level for use of mass media employer contact, employer committees, and structured provision of technical services. A Level 3 office will likely have a total staff of more than 40 persons. Fewer than 10 percent of all ES offices operate at that staff level.

This admittedly rough categorization has at least two specific implications for an evaluation of employer services. First, there is no reason to suppose, *a priori*, that the character and quality of employer contact is necessarily different from one level to another. The basic "sales job" may be quite similar whether it is being carried out by an interviewer with other responsibilites, or a member of a highly organized ERR team. Likewise, there is no reason to expect a different outcome from employer services contacts intiated from local offices, as against those initiated at a district or Job Bank level. While that kind of organizational variation would be a suitable area for differential analysis in a program evaluation (it is possible, for example, that a local office program is more effective in relaying employer feedback to other appropriate staff such as counselors and interviewers, thus enhancing the suitability of placements), the "message" and service to the employer are much the same regardless of the administrative level from which the contact comes.

Second, improvement mechanisms suitable for one level may be inappropriate for another. Innovations, such as staffing employer committees, may simply be

beyond the capacity of smaller offices. Conversely, the informal, first-name-basis relationship which often characterizes employer relations in small town offices may not be suitable in the metropolitan context.

The paucity of technical services makes a similar categorization unworkable; most local offices either offer no technical services at all, or provide them occasionally and informally through the ERR effort. However, several states are providing extensive technical services, making it possible to assess their value on a case study basis; this is discussed as an approach later in this report.

To provide more detail about the actual working of the employer services program, the following paragraphs present a picture of the day-to-day operation of a theoretical program; the description provided is actually a composite of offices visited during the site work phase of the project. To strike a middle ground, a Level 2 program is used in this example.

This local office is a combined ES-UI facility, with 25 staff. There are three fulltime ERR's, who report directly to the office manager; there are no technical services representatives. One ERR is a former interviewer, the second is a former assistant personnel manager from a local manufacturing plant, and the third is a retired naval officer. All are male; their age range is from 45 to 55.

The office is in the downtown area of an Eastern city, population 155,000; it is the only ES office in town. The city is the hub a metropolitan area, population about 260,000. There is a strong industrial base; major employers are an electronics firm, a steel mill, a public utility, and a truck assembly plant.

Each ERR has an assigned geographical territory. Each has a list of all known employers in his territory; these lists were developed years ago, and are periodically updated by the ERR's themselves. In the city, a relatively small number of large employers account for about three-fourths of all ES listings; these are designated as "major market" employers, and noted for special attention from the ERR's.

The ERR's meet with the office manager briefly each Monday morning to discuss the week's work. Office strategy varies from time to time; currently, the emphasis in on re-contacting present users of the ES to maintain favorable rapport and promote new listings, especially by encouraging employers to list more categories of openings than they are now listing. A secondary effort is aimed at cultivating new users. Each ERR currently has an assigned quota of 10 telephone contacts and 30 personal visits per week. Each ERR keeps track of his own contacts by maintaining a log file.

As "salesman" for the ES, the ERR is expected to promote ES services to employers, solicit job orders,* coordinate all employer contact with the ES, develop job openings and training opportunities for specific applicant groups, provide labor market information to employers, and serve as the ES representative to industrial development and community organizations.

When visiting employers, the ERR carries a folder with -- depending on the purpose of the contact -- material on federal contract requirements (mandatory listings), summaries of qualifications of applicants awaiting placement, labor market information, descriptions of available services, and other promotional materials.

Back in the office, the ERR shares any new information about the job market, including specific placement opportunities, with other staff. He maintains contact with "his" employers through telephone calls and occasional mailings. The ERR's also oversee the Job Service media program, which consists primarily of radio and newspaper advertising; other media efforts (such as billboards) are carried out above the local office level.

The work of the ERR's is subject to regular review. The office manager monitors logs to make sure the desired level of contact is being maintained, and track is kept of listings generated by each individual

* Whether the ERR actually takes the job orders varies.

ERR. About once a year, a representative of the state office pulls a random sample of each ERR's contacted employers, and interviews them briefly to see if they are satisfied with the ERR, and to correct any indicated problem areas.

This description of the work of the ERR is not intended to be either rigorous or exhaustive; it is, rather, provided to give some flavor of the nature of the program to be evaluated. As can be seen, what we are evaluating is the effect of a sales-oriented, systematic program which has both a general "public relations" component, and a specific orientation to the securing of job listings.

The technical services side of the employer services effort is more difficult to characterize; there is no "typical" case, unless it is the situation where technical services are negligible or lacking. The state with the largest technical services program (27 fulltime technical services staff at the local office level) devotes the bulk of that effort to testing prospective employees. Those states providing technical services during FY 75 indicated in our survey that the services most provided (in order of frequency of provision) were: job analysis, personnel policy review, training assistance, testing, in-plant studies, and labor market information. The low ranking of labor market information is probably due to many states' considering this service as an ERR, rather than technical services, activity.

If the local office sampling plan used in the evaluation had to be based on an *a priori* typology of technical services programs, a tedious process of inquiry on an office-by-office basis would likely be required during sample design. However, as will be discussed in Section Five, the sample can be constructed by choosing local offices on other measures of interest, and then characterizing them in terms of provision of employer services, including technical services. This will permit comparisons of situations with and without technical services, and with differing levels and types of technical services -- including, if desired, detailed case studies of situations where technical services are extensively provided.

SECTION FOUR: THE EMPLOYER SERVICES EVALUATION AND INDEPENDENT NET EFFECT

The problem with the evaluation of the employer services program is to design a methodology which can relate the presence (and characteristics) of the program to principal ES performance indicators. Although this concern is essentially no different from that in any social or manpower program evaluation, the employer services evaluation poses unusual problems. First, there is most likely a considerable temporal distance (lag) between the "treatment" (provision of employer services) and most ES performance measures, particularly job listing and hires. Second, the program is usually not a direct part of the job-listing or applicant-referral cycle. It is but one of many factors contributing to employers' decisions to list jobs and to their decisions to hire ES referrals.

These difficulties cannot be glossed over in any assessment of the program. Whether one is willing to settle for a fairly soft managerial appraisal of the program's worth, or insistent on a statistical test of program impact, the difficulties remain.

As an example of the problem of temporal lag, consider the possible outcomes of an experiment designed to increase ERR activity with a target group of employers. One might observe a rapid increase in listings following that activity, and conclude that the ERR effort does increase listings and is required to sustain them. However, one might be observing simply the transitory effects of a special promotional effort, not unlike that resulting from a massive advertising campaign. Long-term job listing behavior might not be affected at all. In fact, it could even be hurt if service to traditional users were diluted because of the special effort. Conversely, one might observe no increase during the time frame of the study. But, this would not necessarily indicate ERR activity has no effect. The change in listing behavior might be months or even years away, depending on the employers' need for workers.*

As an example of the problem of interacting variables, consider the complexities in sorting out the contribution of ERR activity to ES performance measures from among the scores of potential contributing variables. The ERR activities are strongly dependent on the nature of the employers in the area. Large employers who use the ES receive considerable number of contacts; small non-users, perhaps none. Thus, to begin with there is a strong *imposed* relationship between the program variable and the most important measures of performance. However, it is the measure of performance that is *causing* the variation in the *independent* variable and not the other way around.

In addition, the ERR program is tied to the nature of the job bank, nature of referrals sent to employers, and any number of other variables, many of which are more strongly linked to job-listing behavior than the ERR variable itself. Unless there were significant variation in ERR activity *independent* of

*There is some evidence that non-traditional recruitment methods are employed only when there is a need for rapid recruitment to meet the demands of business expansion or unusually high turnover.

other major ES internal factors and external events, the relative contribution could not be isolated. Moreover, even such variation does not guarantee success if the magnitude of the contribution is small relative to other variables or if it is linked to other variables in a logical "and" relationship; e.g., the ERR visits promote job listing only if referrals are suitable.

Considering these difficulties, one might well wonder if any methodology could be proposed to determine the effect of the program. Although no technique can ensure results, one can construct a suitable methodology from either of two directions: (1) an essentially inferential approach that determines the contribution of employer services to job listings or related measures by means of direct observation or questioning, or (2) a measurement of net effect by observation of an experimental and "control" situations. Each approach has strengths and weaknesses, and can provide the manager or decision-maker with reasonable information about whether the ERR program meets its intended goal by contributing to ES performance. Each approach is also beset by pitfalls, however, which, if not overcome, can result in meaningless, or even worse, misleading results.* Moreover, neither approach determines causality, but simply the observed relationship between ERR activity and performance: the one method by inference, the other by statistics. While the remainder of this report addresses problems inherent in both methods, it cannot determine which is intrinsically better for an employer services evaluation -- that question is in the realm of epistemology. All we can ask is which approach comes closer to letting us know the contribution of the program to presumed outcomes.

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*Even the "causal" associations now occasionally used in monitoring point to some puzzling situations. It has been observed in some studies that placement rates can *decrease* with increased ERR activity, or that "outreach" efforts by employer services personnel can be associated with *negative* perceptions of the ES by employers.

Without rehashing traditional arguments, it is certainly fair -- and necessary -- to consider whether our knowledge of the program's effects is better advanced by a statistical correlation, or by examination of feelings and recollections of employers about the reasons for their actions.

It must be understood from the outset that *no* approach will enable us to "know" the contribution of the employer services program to changes in employer attitudes and behaviors; the question really comes down to which allows us to make the more valid inference. At the end, the users of the evaluation -- that is, the DoL policy makers who must make decisions about the employer services program -- must be persuaded that the methodology selected was appropriate to answer those questions of policy interest, and was sensible and valid.

The choice of words like "sensible" and "valid" is deliberate. For some evaluation purposes, a "sensible" approach may tell us much more of real use than a presumably more scientific one. It should be stressed that a statistical correlation is not necessarily more truthful, nor more valid, nor more useful, nor more relevant to federal policy, than the views of a knowledgeable observer -- nor is it necessarily more believable. Its chief virtue is that its precision -- that is, the likelihood that the observed outcome could not have been produced by a simple random occurance -- is knowable.

These words of Irwin Deutscher, from *Words and Deeds: Social Science and Social Policy*, provide one perspective:

> In attempting to assume the stance of a physical science, we have necessarily assumed its epistemology, its assumptions about the nature of knowledge and the appropriate means of knowing, including the rules of scientific evidence... One of the consequences of using the natural science model was to break down human behavior in a way that was not only artificial but which did not jibe

> with the manner in which the behavior was observed... We concentrate on consistency without much concern with what it is we are being consistent about or whether we are consistently right or wrong. As a consequence we may have been learning a great deal about how to pursue an incorrect course with a maximum of precision.

The current belief that knowledge which is expressed in numbers and comes out of machines is somehow "better" than that which comes from observation, poses problems, and dangers, for policy-relevant program evaluation. To draw the issue more clearly, we must look more rigorously at the meaning of "net effect" in scientific experimentation, the purpose of this section. By definition, evaluation is the effort to understand, and if possible, measure the contribution of some experimental element, component, program, or condition to some measure of interest. This effort is usually called the evaluation of "impact" or "net effect." While the notion of "net effect" is scientifically simple, determination of net effect in the behavioral sciences is extraordinarily complex.*

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*We should not overlook at this point the principal lesson learned from the experiments conducted in the Hawthorne installations of the Western Electric Co. All one usually remembers about this experiment was that it was variation which improved performance, not the nature of the variation itself. This was, however, a secondary result of the experiment. The primary result was that in experimental situations, the group dynamics between experimenter and team proved to be more important then the methods, and the methods used in management sciences, largely taken over from the more formal sciences, were unreliable in the behavior sciences. Manpower evaluation may now be in the same state that the management sciences were in the 1920's, in the process of learning the limitations of purely "formal" methods.

4.1 EXAMPLE OF SIMPLE NET-EFFECT EXPERIMENT AND ITS RELATIONSHIP TO THE STUDY OF THE ERR PROGRAM

The fundamental problem is in understanding the relationships between the experimental element and the measure of interest. This is no simple matter even in the physical sciences where reasonable cause-and-effect relationships can be conjectured. In the behavioral sciences, where there are seldom, if ever, rigid cause-and-effect relationships, it is nearly impossible. To see this, let us briefly consider a classical experiment involving the test of a direct cause-and-effect relationship: the "impact" of Vitamin C therapy on the frequency and duration of upper respiratory viral infections.

The hypothesis suggested by the Vitamin C controversy is straightforward: a prescribed dosage will produce a statistically significant decrease in the average number and duration of "colds." The research design for its test is correspondingly simple. To control for the random effects that might also influence "cold intensity," researchers resort to what is known as a double-blind random assignment model. First, an adequate number of persons is randomly assigned to either an experimental group or to a control group. The experimental members are given a dosage of Vitamin C and the control group members a placebo, as identical in size, shape, taste, smell, and color to the experimental pill as possible. Then, to satisfy fully the conditions of double-blind experimentation, those persons administering the drug are also unaware of which person is in which group, and which drug is real.*

This model represents pure net effect. One assumes that the effect of the drug should be fairly constant over all population types so that reasonably small experimental and control groups are possible. Moreover, when one is just concerned with the general net effect, the average differences between the group can be tested for statistical significance. Since the groups

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*In theory, triple blind experimentation is required, the third condition being that the experiment itself does not alter conditions.

are statistically identical to a known degree of precision, because of random assignment, any difference in the observed frequency of colds between the experimental and the control groups is accepted (again, to a stated degree of risk) as being due to the presence of the Vitamin C therapy.

Despite the inherent simplicity of this experimental design, a number of contradictory experiments have been carried out producing widely different results. Interestingly, the proponents' experiments produced a significant difference, the opponents' experiments did not -- this despite the impartiality of the design.*

There are several features of this experiment which are of significance in considering the impact of the ERR program. First, this experiment illustrates one of the simplest experiments designed to test a hypothesis that a certain treatment or effect has a net impact on some measure of interest: in this case duration of colds. The division into a simple experimental and control group is adequate only because the average of colds' duration is relatively similar for all identified classes of persons within each group. If there were strong influences on colds' duration due to other considerations, e.g., age, sex, race, occupation, it would be better to structure regressions of the following form:

$$y = a + b_1x_1 + b_2x_2 \ldots\ldots\ldots b_nx_n$$

where y would be a variable defined on some measure of cold duration of intensity and the x's would be the series of independent variables all expected to have some influence on colds with one of the x's being the presence or absence of the Vitamin C therapy.

Such an experimental design would require considerably more data points to ensure that each variable occurs

*This is a good illustration of the fact that statistical experimental models control random variation and nothing else, particularily not bias.

sufficient times independent of combinations with other variables;* however, it would have the advantage of identifying the *relative* net effect of the experimental procedure compared with fixed characteristics of individuals and perhaps of their "treatments" which could only be absorbed into the overall average produced by the experimental/control group model.

Second, it illustrates experimental controls which must be used if net effect is really to be detected. The two groups must be identical, usually achieved by means of random assignment, the nature of the experimental treatment must be precisely controlled and administered; and the administration must be done in such a way that the actions of the experimentors could not cause the observed differences. In the case of the Vitamin C study, this meant the administration of identical pills, with the persons administering the pills not aware of which pill was which.

Third, the supposed effects of treatment are assumed to be fairly immediate so the experiment and observations of results can be contained in a reasonably narrow time period.

Fourth, the experiment shows that even the simplist designs to test experimental hypotheses present methodological problems: different experimentators produced different results.

4.2 THE ERR EXPERIMENTAL MODEL

Let us consider the problem of identifying the impact of some aspects of the ERR program on ES performance measures. If one is to isolate the effect of the ERR program (or its net benefit as current terminology calls it) then one either must be able to find natural variation in the ERR program itself, or must be able to structure an appropriate experiment similar to that described for the Vitamin C problem.

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*To isolate just the effect of the treatment, a reasonable number of observations of Vitamin C therapy and non-therapy should occur with every combination of other variables. Because of the sample blow up effects, only a few critical "controls" can usually be considered in such models.

The requirement is that ERR program activities must vary sufficiently in such a way that alternative levels of activities occur in combination with each other variable of potential importance. The implications of this are seen in Figure 4-1. If we consider ERR program variables shown in the rectangle on the left of the figure, then their relationship to a series of ES performance measures is shown by the arrow leading to each circle. In addition, other major areas of influence are shown by other rectangles impinging on the "performance circles."

4.2.1 ERR Impact Model A

The most immediate measure of ERR "effectiveness" would be some measure of the degree to which employers felt the ERR was useful to them. At this level, the relationship between ERR activity and the employer perception would be quite direct, very immediate, and subject only to external variation perhaps because of employer characteristics or some overriding labor market variable.

At this level, one could reasonably structure a study similar to that described for the Vitamin C experiment. Since the confounding variables are few, it should be possible to find a reasonable number of similar employers who were and were not ES users who had been visited by ERRs. The researcher could also identify a series of ERR activity types -- e.g., type of contact (phone, personal visit, mailing, mass media), frequency of contact -- and associate them with the "usefulness of ERR program" measure. Since the relationship between the ERR contact and the employer's perception is immediate, one could focus on the last few contacts, establish dependent variables on degree of importance, nature of information provided, nature of problem solved, or whatever, and then simply determine if contacts were perceived as important, and if non-contacts were missed.

Such a model presupposes that the measures of importance of ERR contacts with employers are to be found in the employer's perception of such contacts. If this simple hypothesis is true, then one can determine the relative success of different approaches. If the proper controls are used (now primarily limited to selection of

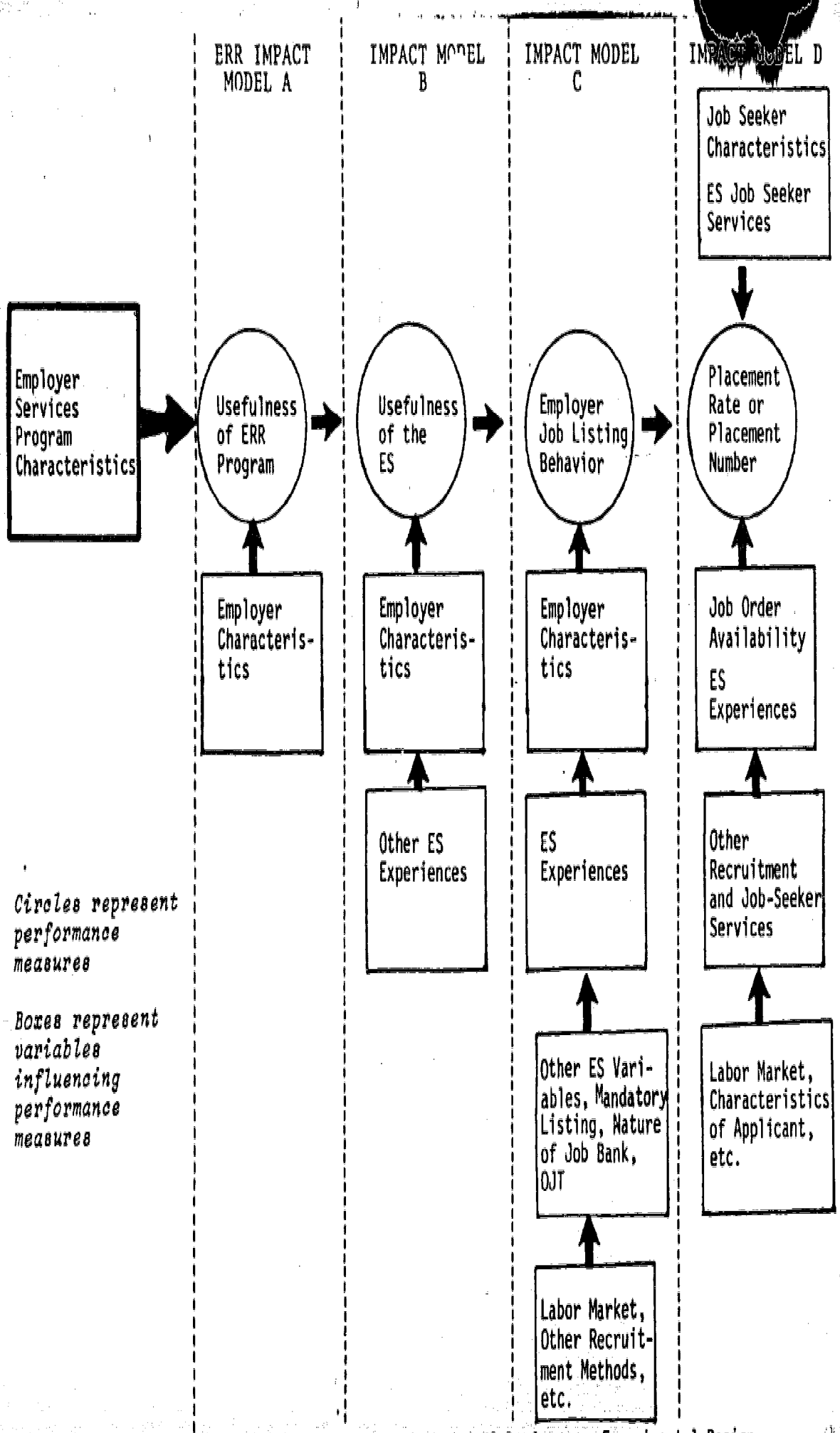

Figure 4-1: Visualization of Employer Services and ES Performance Experimental Design

identical employer types and proper identification of ERR activities) the net effect *on the perception measure* could be as precisely determined as the most carefully controlled experiments in medicine, agriculture, or the physical sciences.* However, as one moves to measures on the right of the figure, away from the ERR intervention block, the problem becomes more complex for two reasons: confounding variables and temporal displacement.

4.2.2 Impact Model B and Natural Variation

Outcome measures B through E become increasingly remote from the ERR variable itself, indicated by the greater distance in Figure 4-1, and affected by a larger number of variables, indicated by the number of other rectangles impinging on the circular performance measures. These "impact" performance measures are thus confounded by a number of other variables which would somehow have to be controlled by the experiment. For example, the ES perception measure would have to encompass not only the ERR activity, but also the whole range of experience with the ES, particularly responsiveness to job orders and the quality of referrals.** Even to detect the impact of ERR activity on overall perception of the ES, outcome measure B, the researcher would have to control for variations in job order response. This could be done either by design or by natural variation. In the first case, an experiment could be structured so that employers who had different experiences were grouped into experimental and control groups with the experimental

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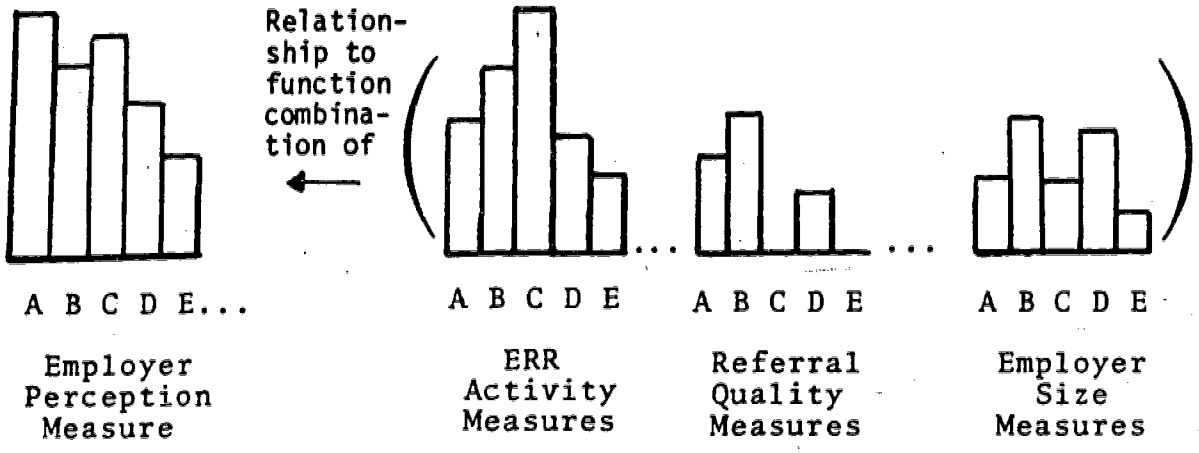
*Precision pertains to the degree to which random variation has been controlled by means of the experimental design. It does not, however, imply *accuracy*, which is a combination of all error and bias -- something too often overlooked in research models. The most precise experiment is not always the most accurate or the most valid.

**The quality of referrals sent against the order was the most important consideration from the perspective of the employer, and was the one subject to the greatest variation, in Camil's study of ES users and non-users.

group exposed to the level of ERR activity work being examined. This would require a much larger sample of employers than would finally be used to isolate the appropriate variation. Properly, of course, the experiment would not be correct if the use of ERR activity influenced job order response.* The structure of the "natural variation" experiment would be as shown below.



Letters identify different employer responses, experiences and characteristics.

Figure 4-2: Relationship Between ES Variables and Employer Perception Measure

*For example, this could occur if the ERR helped to define the employer occupational requirements and to identify special hiring conditions that might not be picked up by a job order taker. It would also occur if the ERR helped the employer formulate his orders better.

In the experiment represented by Figure 4-2, one would attempt to find some functional relationship between various measurement levels of the employer perception of the ES (indicated by measurement levels A,B,C,D,E, etc.) and corresponding levels of some ERR activity measurement, referral quality measures, employer size measures, and so on. The most common model assumes a linear relationship between the dependent and independent variables, and the functional relationship would take the form of a simultaneous set of linear equations, one for each measurement level, with the solution being of the form:

$$y = \text{intercept} + c_1 \text{ (ERR activity)} + c_2\text{(Referral Quality)} + \ldots$$

4.2.3 Impact Measure B and Experiment Variation

The experimental design procedure would produce a more rigorous test, one which would have a better chance of producing "causal" inference as opposed to "historical" associations of some interest. This model would call for the division of employers who used the ES into two groups. Although random assignment would produce comparable groups for all potential variables, a deliberate stratification would be possible for critical indicators: size, industry, occupations listed, degree of use, previous indication of satisfaction (if any), and previous ERR activity. The last variable, of course, poses a problem since this is the variable that should be absent, or at least greatly reduced, in the "control group." If it is not possible to structure two comparable groups that have not been exposed to a sufficient amount of ERR activity, the experiment would have to test a hypothesis of the form:

> a significant increase in ERR activity can promote satisfaction with the ES because of the direct liaison between the service and the employer enabling better description of employee needs...

Over time, a diminution of ERR activities for the "control" group and an increase for the experimental group would be adequate to test this hypothesis of marginal effect. By monitoring and measuring ES use and response over the period, the importance of ERR activity could be detected relative to other influences. It would also be possible to focus on critical stages of the referral process to determine if the job listing behavior, the quality of referrals, hire rate, etc., seemed to be influenced by the level of ERR activity. This would only require shifting the experimental model from that shown in equation (1) to a form similar to equation (2).

(1) $$y = a + b_1x_1 \ldots\ldots\ldots b_nx_n$$

where y is a measure of employer satisfaction
x_1 is a measure of referral quality and
x_n is the measure of ERR activity

(2) $$x_1 = a + b_2x_2 \ldots\ldots\ldots b_nx_n$$

Alternatively, tests of intercorrelations among the variables could be performed to determine their relative "independence" of one another to help formulate a theory of which elements in the employer services model are related and which are apparently correlated with increasing levels of ERR activity.

4.2.4 Other Outcome Measures and ERR Program Evaluation

Up to this point, the experiments have been relatively confined. The number of variables has been manageably small and clearly relatable to the hypotheses

being tested. Moreover, there has been no significant temporal displacement between "cause" and "effect." The ERR activity could be provided, or increased, for a certain group of users and the various measures of perception of the ES or of the quality of referrals, etc. determined over roughly the same period.* However, as we proceed to the next measure "C," listing behavior, shown on Figure 4-1, the problem becomes much more complex for several reasons:

(1) The relationship between ERR activity and job listing behavior is not as clearly understood as the relationship between ERR activity and quality of service provided to users.

(2) Other variables increase in number and magnitude to the extent that the detection of program effect becomes difficult to determine.

(3) The changes which occur in job listing may be considerably displaced in time from the intervention of ERR activity.

Each of these problems can make it difficult to design a study to determine the degree to which the employer services program influences job listings. To see this, consider two measures of job listing behavior, or more appropriately, the ES penetration into recruitment activities.

- The percentage of employers who use the ES

*Although there would be some time lag, it wouldn't be longer than the time between the visit and the order.

- the percentage of all job categories (e.g., a request for clerks) listed with the ES by employers who use the ES at all.*

For the first measure, the theory of the relationship between the employer services program and job listing would propose that employer use would depend on employer knowledge of the ES and its services. Thus, bringing the ES to the attention of employers would increase their use of the ES. There are two probems with this: (1) the reasons for non-use have more to do with a lack of need than a lack of knowledge, and (2) bringing the ES to the attention of non-users might only promote an employer to try it once as a part of recruitment activities, but would not necessarily promote a hire or re-use.**

Since it is by no means clear that the ERR's role could be limited to that of a publicity agent, or that a sudden surge in use would result in continued use, the proposed model would have to be considerably more complex than those previously considered: In particular, it would have to account for:

- Variables which influence the need for a recruitment: labor market variables, reasons for recruitment (normal turnover, expansion, change of business area), etc.

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*A number of measures of penetration can be proposed; e.g., percentage of all job categories recruited for listing with the ES, percentage of all vacancies listed with the ES, percentage of all employers who list at least one opening with the ES, percentage of all job categories recruited for listing with the ES for employers who list at least one category, percentage of all openings listed with the ES for employers who list at least one opening.

**The recently completed Camil study, "Job Search, Recruitment, and the United States Employment Service," found that non-users know of the ES; they simply didn't need it. Moreover, most were very satisfied with traditional recruitment methods and had no trouble finding employees. Since most recruitment consists of three or four methods, promoting use might only result in adding one method (the ES) to the activity, with little benefit to the ES. Only if the employer found the resulting ES service excellent and the referrals timely and as good, or better, than those from traditional sources, might listing behavior change.

- The time lag between increased (or diminished) ERR activity and modification in job listing behavior,*
- The variation in the nature of the job order/referral process, particularly if new users are in different industrial areas, or list different occupations from those traditionally listed,
- Differences between traditional users and non-users.

The approach to determine the net impact of the employer services program under such conditions would traditionally be to use communities (ES offices) as the sampling units and to construct a macro-model. Previously we have described experiments which were designed as micro-models; i.e., models based on observations of the *individual* elements actually affected by the experiment: the person taking Vitamin C, the employer receiving an ERR visit. Micro-models are usually desirable since the intervention of the test factor is directly linked to a unit of observation. When the experiment becomes complex, however, as is the experiment to determine the impact on listing, hiring, penetration, etc. of the ERR program, regressions often result with trivial coefficients of determination (R^2) because of the effect of the random fluctuations of extraneous variables. Similarly, control group designs are difficult because comparable groups cannot be constructed, or because the standard deviation within the groups is very large, making it difficult to detect the variation produced by a weak factor, such as the ERR visit. To overcome this problem one can often employ a macro-model in which the units of observation are the aggregates of the variables of interest, not the individual units themselves.

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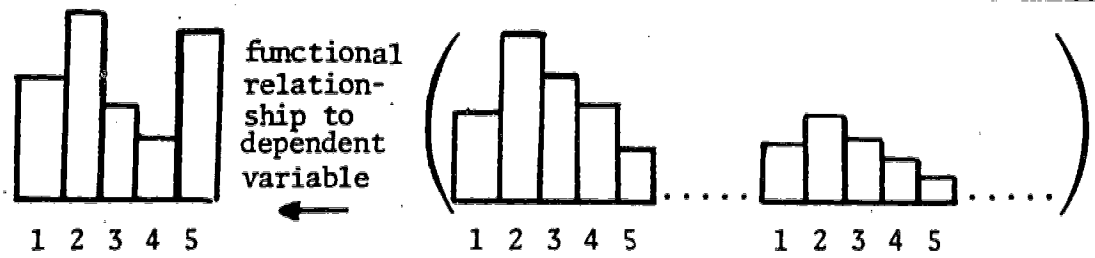
*Again, a brief increase in listing level after "prodding" from ERR's would not constitute a change in listing behavior. Many employers will use a method once because of prodding, as was determined by the Camil study of recruitment in connection with solicitation from private agencies.

4.2.5 Example of a Macro-model ERR Evaluation for Job Listing

In a macro-experimental model, and as shown in Figure 4-3, the dependent variable would be some assumptive measure defined on job listing: penetration in terms of openings, orders, employers vacancies filled, number of orders listed, number of orders filled, etc. The independent variables would be the aggregate measures of the variable of interest and other factors which could influence the measure chosen: labor market variables, other ES variations, shifts in other recruitment methods, etc. Certain "static" variables would be controlled by setting up the experiment in such a way that ERR variation took place across communities of each type; e.g., according to industrial mix, size, minority group levels, etc.



NOTE: Numbers refer to aggregate measurements obtained in different areas: 1,2,3, etc. This contrasts with the measurement levels defined by letters in Figure 4-2 which were based on individual elements: an employer, an ERR visit, etc.

Figure 4-3: Example of Macro-Design of Experiment to Find A Relationship Between Employer Use of ES and Level of ERR Activity

To construct such an experiment, one would proceed by first selecting an adequate number of communities controlling for selected static characteristics: size, industrial mix, area of country, etc. This presents the first problem. If the researcher attempts to control for all possible variables, it may be found that hundreds of communities are needed. If controls are placed on only a few, one risks having inconclusive results because the uncontrolled static variables introduce too much extraneous variation into the results.

Once the communities are selected, the researcher would introduce the factor of interest and carefully measure fluctuations in both other independent and dependent variables. Ideally, there would be a set of communities that were *tabula rasa* as far as employer services activities were concerned. While there are many ES offices with no employer relations staff *per se*, it does not follow that employers in the vicinities of those offices are unreached by employer services activity. Frequently, they are contacted by other staff (interviewers, counselors, veterans employment representatives), or served by staff operating from Job Bank facilities or district offices of the state employment service -- or even from another local office in the same metropolitan area. Such employers are also sometimes exposed to mailings and mass media advertising undertaken by the employment service. Thus, to guarantee adequate variation in terms of exposure to employer services, the experiment would have to increase dramatically the level of service in certain areas while holding it back in others. Other ES activities should also be varied, with appropriate measures developed.

The methods required to determine the impact of the ERR program would consist of examining fluctuations in the performance measure and other independent variables. Since the variables would not necessarily respond in the same way or over the same time period, some form of time series would be needed. This technique employs regressions in which each equation represents an observation at a different point in time; *viz.*, measurements would be taken of each of the variables at different times and structured into equations of the form:

$$y_{t_o} = a + b_1 x_{1,t} \cdots\cdots\cdots\cdots b_n x_{n,t_o}$$

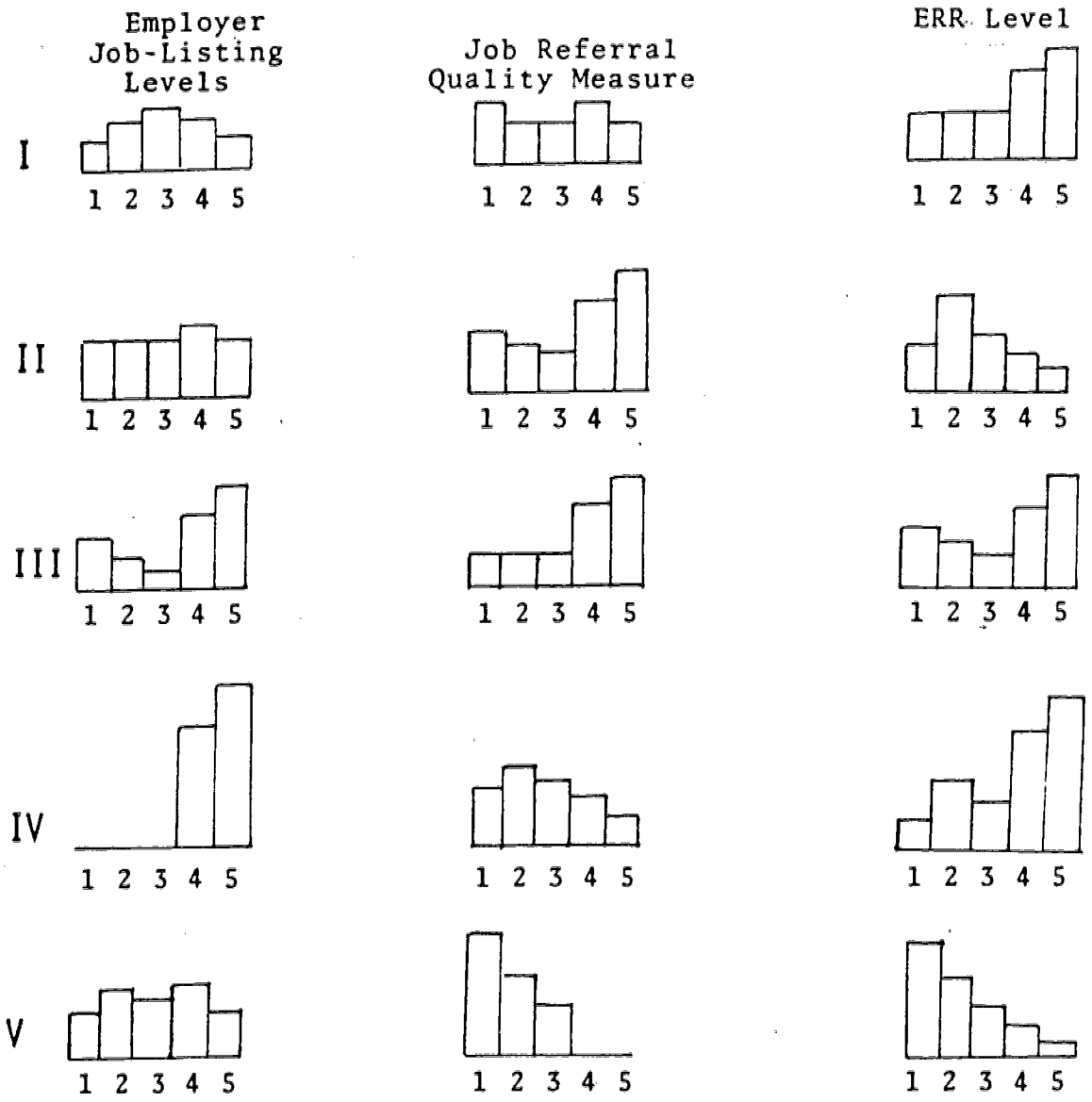
$$y_{t+m} = a + b_1 x_{1,t+m} \cdots\cdots\cdots\cdots b_n x_{n,t+m}$$

These equations are more problematic than static regressions because of lag effects and cataclysmic changes. If one assumes that there is some relationship between the test variable and the measure of interest, it is probable that the effect will be induced after some time. The effect on the dependent variable due to the other variables would also occur after some time had elapsed, but not necessarily the same time period. For example, the time required to produce an observed effect in listing behavior from ERR activity might be months to years but between listing behavior and response to an order, only a few months. One would have to observe fluctuations over a long period, probably years, and introduce lag variables in the equation to reflect the displacements of the effects of the independent variables. These processes are arduous, long, and problematic.

In the example in Figure 4-3, one would observe the level of measurements in each of the five communities and determine if there were a pattern. Suppose that one observed the series of measurements over the five communities as shown in Figure 4-4. One might well suspect a lag of one interval for referral, but three intervals for ERR activities, and structure the equations as shown below:

$$y_{t_4} = f(\text{referral}_{t_3}, \text{ ERR level}_{t_1} \cdots\cdots\cdots)$$

$$y_{t_n} + f(\text{referral}_{t_{n-1}}, \text{ ERR level}_{t_{n-3}} \cdots\cdots)$$



NOTE: Arabic numerals refer to different communities, roman numerals to different time periods.

Figure 4-4: Example of Time Series Measurements

A simplified technique that could be used, but that would have much less likelihood of establishing "causal" inference, would be to average the variables and treat the problem as a static model. This could be done by increasing activity over time in some areas and decreasing it in others and then observing the penetration or other measure of interest to determine if the "right" relationships occurred, controlling for extraneous factors. This procedure, though simpler, is also less sensitive because the points of observation are fewer and the fluctuations which would be observed in a time series analysis would be absorbed into the "averages" of the static analysis.

4.2.6 Possible Application for Micro-Models and ERR Evaluation of Job Listing Behavior

Although a micro-model could not be readily implemented for community wide penetration because of the difficulties of measuring the individual experiences of employers,* it would be feasible to determine the listing behavior of employers who use the ES at all. Selecting a sample of frequent users, controlled for size, industry, etc., one could develop baseline data about the use made of the ES. Then, after the imposition of ERR activity on a randomly chosen sub-sample, one could determine the listing activity, and see if a correlation could be established between increases in employer services activity and listing.

There are two reasons why this model is simpler than the pure penetration model:

(1) The relationship between ERR visits and listing or hiring decisions of users is more .

*In theory, a micro-model defined for a dependent variable (lists, does not list) will produce the same result as a macro-model. The problem is that identifying the individual variations is too difficult, and when they are not explicity identified they result in a reduced coefficient of determination. In a macro-model, the individual variations are usually "absorbed" into the model.

direct since one is "improving" rather than promoting ES service.

(2) An experiment is easier to construct since the universe is easier to locate, the differences among users are less than those between users and non-users, and previous ERR activity can be better controlled among a universe of users than among a universe of users and non-users, necessary for a penetration model.*

Thus, it is feasible to consider the micro-model for a study of improved penetration among users (including the important measure of improved referral acceptance rate) than it is to try to define a study for a broad-based measure of penetration defined on use/non-use.

4.2.7 The Impact Model and Placement, Measure D

The last stage of the series of evaluation models -- net effect of the employer services effort on placements -- is the most problematic, as would be expected. Whether an applicant gets a job through the ES is one large step removed from whether an employer decides to use the ES or not. In fact, it is not even clear if the two are related within the range of normal variation.**

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*The Camil study of recruitment found that ERR activity was strongly correlated with previous ES use: 40 percent of consistent users were visited, compared with four percent of establishments which never used the service.

**The Camil study of recruitment and job search also found that placement and penetration (defined on the percentage of job categories recruited for) were not correlated. If anything, the data, and some other works, suggest that they might even be inversely correlated. See also, Dodge, H. Ripp, Special Report: Employer Relations Program, Activities and Accomplishments, FY 1972.

Thus, one is confronted by a more complex form of the macro-penetration model discussed earlier. This time, moreover, one would have to include a series of variables describing the applicant services and the labor market as it affects supply, not only demand. Because of this complexity, and the fact that there is no established link between the placement of an applicant and the ERR program, it is unlikely that such a test can be constructed. The controls placed on offices would have to cover a broad variety of ES activities, which might prove not only difficult but also illegal if certain required applicant services had to be suppressed. As a proxy, however, penetration defined on hiring activity could be used under the reasonable assumption that if employers increase the percentage of referrals hired, this must be a corollary of placement. In any case, this should be considered as the most remote outcome of employer services. ERR's really cannot be expected to compensate for the supply side of the equation as well.*

Having addressed the uses of techniques for this type of evaluation, and considered the problems associated with them, we turn now to the presentation of the proposed methodologies beginning with a feasible net effect approach.

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*Dramatic evidence of this is available from the Human Resources Concept. During the late 1960's the ES became almost totally client oriented, and much of the ERR's job was to sell social consciousness to employers. Although this may have been admirable as a social objective, it resulted in a dramatic decline in ES performance measures -- hardly blameable on ERR efforts.

SECTION FIVE: THE PROPOSED ALTERNATIVE METHODOLOGIES

The design of a suitable methodology to evaluate the employer services program poses problems because of two principal features of the program:

(1) The ERR program represents a small part of ES operations. Its effect on ES performance is confounded by a number of other ES features and external variables.

(2) The ERR program does not vary greatly across offices, and where it does, the variation is tied to other office and area characteristics.

These features of the program restrict the options of the researcher. Whereas it is sometimes possible to use retrospective program data (or at worst to develop new

data about the regular operations of a program) to analyze a program's effectiveness, such a resolution is not possible for the ERR program. Its operation is so imbedded within a broader ES operational structure that the necessary independent variation is not available for analysis. Moreover, the probable contribution to ES performance measures is of a lower magnitude than other more direct variables: particularly the availability of suitable applicants and the nature of the referral process. Although neither of these problems is insurmountable, they pose such difficulties for any net effect model that an adequate design will have to be lengthy, expensive, and based on experimental variation.

The following paragraphs present an outline of such a model. At the same time, recognizing the problems of such an approach -- particularly problems of time and expense -- we also put forward for consideration a second alternative. This is a "softer" program evaluation which, while not "rigorous," will provide valid answers to questions of program concern at a much lower level of effort than that necessitated by the net-effect alternative. While we believe the second approach is preferable because of its economy, both methods are workable.

5.1 REQUIREMENT FOR NET IMPACT AND EXPERIMENTAL DESIGN

It is a common fallacy in manpower and social experimentation to believe that net impact or effect models can be readily applied to on-going programs by simply capturing an experimental and control group of suitable elements, measuring some difference, and declaring that the difference is the net effect. In fact, it is questionable if any successful net effect or impact model has ever been achieved in manpower research. First, most research fails to understand the requirements of "causal" methods in the experimental sciences, from which the rigorous impact models have supposedly been derived. In order to derive valid net effect measurements, the element or program being tested must range over the same variables that are characterized by the "control" set.

In practical programs, such random variation seldom occurs. If the managers of programs are assumed to be rational, then the program variations they institute can be expected to be due to other structural and environmental considerations. This is, of course, excellent from a management perspective, but it is disastrous from a research perspective.* Based on our review of the ERR program in the states, it is clear that the ERR program in terms of size and approach is determined by both office and area characteristics. Small offices and areas have small programs, offices in areas dominated by an industry direct their approach to that industry, and so on. Because of this, *no net effect of impact model of the ERR program can be constructed without experimental design.* And, this requires some interference with the procedures and operations of the local programs, as well as a lengthy and expensive research design.

For these reasons, the net effect model proposed is based on a careful restructuring of ERR programs so that any actual contribution to the outcomes of interest can be determined to a known degree of statistical precision. More important, because of the nature of the experiment, any difference can be said, with a good degree of certainty, to have been *caused* by the ERR activity itself. That is, there would be very little likelihood that the observed difference could have occurred because of happenstance or association between the ERR variable and another variable which is the one actually linked to the outcome measure of interest.**

*The problem is not so severe when dealing with net effect models for participants: the limited enrollment potential of all programs usually results in a sufficient number of persons like those enrolled, but not enrolled, to form a suitable comparison group. However, no research has adequately accounted for the "ghost enrollee" effect (persons receiving similar services from other sources), the overall expansion of the program market due to the existence of the program (violation of the triple blind condition), or conversely, the removal of options from the free market due to the presence of the program.

**Again, it must be stressed that unless the experimental method is rigorously adherred to, correlation does not necessarily imply causality, nor do the β's from a regression predict relationships.

(footnote continued on p. 56)

5.2 DIGRESSION: THE TREATMENT OF TECHNICAL SERVICES

As has been discussed, technical services are not nearly so common as ERR services, being either miniscule or altogether absent in many, perhaps most, local ES offices. Rather than structure a separate evaluation for technical services, we have chosen to include them into the definition of the overall employer services program in local manifestations, and use their presence (absence) and type as variables, just as other employer services program characteristics are used.

If, however, at the time of the evaluation there were a particularly strong interest in technical services, their consideration in the study could be greatly expanded by several methods. First, experimental variation could be used, similar to that proposed for the Level 1 sites in the statistical methodology described below; that is, by introducing a strong technical services component into offices where it had previously been absent, and determining the effect of this action, over time, on the outcome measures of interest. (Theoretically, this experimental approach could be further enhanced by withholding previously offered services in communities with large technical services programs, but is doubtful that such denial of services could, or should, be carried out because of political, not to mention, ethical considerations.)

Unless the β's derived from a regression analysis came from a well-conceived model in which the relationships are understood, and which is relatively time independent, one has nothing more than an interesting historical perspective on the association between some variables. Regression, in particular, is often misunderstood in this regard, and far more credence is given to the validity of derived β's than to most other statistical measures. In fact, the β's from a regression are highly sensitive to the number and the nature of included equations. For example, when a particular independent variable does not occur a large number of times independent of all other variables, intracorrelation between the independent variables can make a significant β highly unstable. The addition of a single additional observation, different from the others, or a slight change in one of the included equations, could result in dramatic shifts in the value of the β.

Second, technical services could be the focus of a "case study" approach, with evaluators concentrating on areas providing such services, and assessing their effectiveness by interviewing employers who receive them. Questions would center on the employer's assessment of the value of the services, including readily quantifiable items (e.g., "How much would you be willing to pay for this service?"). A sample of employers who have not received technical services could also be interviewed, using a market research approach, to determine the extent of "demand" for technical services.

Since the technical services program effort is slight and apparently low in priority, it would appear that a major investment of evaluation resources is not warranted. We believe it is adequate to simply note variations in technical service provision as one defining characteristic of local programs, and assess outcomes against it as against other characteristics.

5.3 MEASUREMENT OF ERR EFFECTIVENESS

Regardless of what methodology is employed, the effectiveness of the ERR program must be judged in terms of some measure which is external to the ES system itself.* All such measures should meet the following conditions:

- They must be observable and consistently measurable.

*In formal system theory, the system is defined by the changes produced in the environment. For example, a glass manufacturing system is defined in terms of the change produced when sand becomes glass; a labor-minded intermediary system in terms of increased employment, higher wages, etc., for a served population.

- They must be related, in terms of a reasonable theory of operation, to the objectives and characteristics of the system being examined.*

- They must be sufficiently important, in terms of the purpose of the system, to justify the time and effort required to examine them. Outcomes that are merely interesting are generally bypassed in favor of those that seem important in terms of policy.

There are at least 11 key measures, most based on traditional measures of penetration, for the ERR program which meet these criteria:**

*In the absense of some persuasive social theory, one would not seek to assess the contribution of the employer services program to some outcome measures. For example, one would not structure a study to examine relationships between the level of ERR activity and worker satisfaction, industrial productivity, area crime rates, or the Dow Jones Industrial Index.

**In assessing the employer services program, the GAO review emphasized a goal of persuading employers to use the ES "as a reliable source for meeting all of their personnel needs." Of employers surveyed by GAO, "almost 60 percent of the employers did not list all their job openings with the Service. We compared the type of jobs employers listed to the occupations of persons they employed, and found that most respondents employing clerical workers, laborers, and equipment operators listed those kinds of jobs with the Service. However, only 10 percent of the employers having managerial employees listed that kind of position. Similarly, less than 25 percent of the employers with professional positions, for example accountants and engineers, listed these openings." GAO also noted, "Most employers did not rank the Service as their first choice for referrals. They ranked newspaper ads and referrals by existing employees ahead of the Service." In a concluding statement on the future of the employment service, GAO says, "Since the Service has many more applicants than available jobs, its employer services program is particularly important if it is going to obtain more job openings. The Department has established criteria for personal visits and telephone contacts that employer services staff should make to employers and has established a broad goal to obtain a larger share of job openings. However, the program lacks direction in terms of kinds of job openings that should be sought... We suggest that the Department establish goals that would encourage the Service to upgrade the types of jobs that it can offer its applicants and to seek a wider range of jobs to better the needs of a larger number of its applicants."

(1) Proportion of employers listing at least one opening with the ES during a given period. The measure of ES penetration would determine the degree to which the ERR program influenced employers to use the ES at all. The universe of observation would be the set of all employers recruiting during a given period, with the measure of success being the decision to list at least one opening with the ES.

(2) Proportion of job categories being recruited for during a given period listed with the ES. For this measure, the categories recruited for, rather than the employers, become the universe of interest. This measure gives a somewhat better picture of recruitment and listing since multiple recruitment activity by employers is accounted for. The previous measure would classify an employer as a ES user even if only one category out of 100 were listed.

(3) Proportion of job vacancies (by category or over all categories) being recruited for during a given period listed with the ES. This measure takes into account the actual number of vacancies recruited for. Since different job categories represent different vacancy levels, this measure gives the best feeling of the actual proportion of labor turnover handled by the ES. However, it can distort the role of the ES unless (2) is also used; otherwise, mass orders would be given undue weight. For example, if one employer were to list a single order

for 100 laborers in the 8xx occupational area, this could greatly outweigh scores of unlisted recruitment activities for professionals and clerical staff, each activity being for one, two, or three individuals.

(4) Proportion of job categories or vacancies listed with the ES by employers who list at least one opening with the ES. The strength of the ERR program may not be in the number of employers throughout the community who use the ES, but in the degree of use by those employers who rely on the ES at all. For example, if the ES is routinely used by about one-third of area employers, this measure would examine the relative use by the percent of all orders or openings listed by this class of employer. This percentage could be as low as a few percent of all openings or as high as 100 percent.*

(5) Proportion of employers hiring from the ES. This measure is related to (1) in that it would determine a relationship between the decision to list and to hire.

*There is some evidence from the Study of Job Search, Recruitment, and the Role of the United States Employment Service, that across the community recruitment patterns are reasonably stable, and that the greatest impact of an outreach program (which the ERR program is) would be to increase the use of the ES by its traditional users.

However, it would be a weak measure of effectiveness since a single hire (out of perhaps hundreds available) would classify an employer as having hired from the ES.

(6) Proportion of job categories for which a successful hire was found. This corresponds to (2), and a category would be considered successful if there was at least one hire.

(7) Proportion of vacancies filled. This measure corresponds to (3) and would be the proportion of all available vacancies filled by the ES. This measure would be an exact counterpart of the proportion of all job finders obtaining work through the ES since there is a one-to-one correspondence between vacancies filled and persons finding work. This measure could serve as a proxy for ES placement activity.

(8) Proportion of ES-listed categories or ES-listed vacancies filled by ES-using employers. This measure corresponds to (4).

(9) Employer perception of ERR usefulness. This a soft measure in that it is not related to ES production except insofar that relationships between satisfaction with the ERR program and production could be independently established.

(10) General employer satisfaction. This measure would be related to satisfaction with ES service in general. Again, this would not be tied directly to production unless an independent relationship could be established.

(11) Employer anticipated listing behavior. This measure would be in the form of any of the penetration measures discussed earlier except it would be related to the employers' expectation of future listing behavior.

These measures represent those which can be reasonably related to ES activity, and which are important in regard to major ES goals. It should be noted, however, that none of these are directly related to an ES placement rate -- the most important current ES production measure -- because of its remoteness from ERR activity.* Although one might assume that increased "employer" production would automatically lead to increased placement, such is not the case because of intervening and confounding events between the listing and the placement. Several studies have failed to find any strong relationship, in fact, between job-listing and placement activity.**

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*Again, there is no direct theory which can link ERR activity to placement because of the lack of correspondance between listings and placements.

**See Camil Associates', "Recruitment, Job Search, and the United States Employment Service;" The USES "Special Report, Employer Relations Program, FY 1972," prepared by Rip Didge, and Geblin and Levine, "Achieving Manpower Goals Through More Effective Employer Services Programs," MESC, Applied Behavior Research, Inc., Feb 1973.

As discussed earlier, this section presents two methodologies. The first is a formal experimental design in which the contribution of the employer services program can be measured within traditionally defined statistical limits. The second is the more "subjective" approach. The first has the advantage in that human appraisal of the program, from either the evaluator's or the employer's perspective, has been largely eliminated, and the findings can be represented directly in terms of changes in any or all of the production measures discussed earlier. The first has, however, the disadvantages of requiring at least 30 months, and preferably 40, and of costing upwards of $500,000. The second has the advantage of being able to be completed in only 12 months at about one-third the cost of the formal study. It has the disadvantage of measuring the worth of the ERR program only through inferential observation of events by the evaluators. Therefore, with present trends favoring pure net-effect models it would be less supportable than the first.

It should be understood, nonetheless, that in terms of developing knowledge about the program and its importance to the ES, neither has an intrinsic superiority over the other. Although there is considerable belief to the contrary, because of the apparent purity of computer-derived statistics, a highly structured statistical model can be less valid than a simpler management appraisal. It must be remembered that statistical theory *only* accounts for the effects of random variation. It does not account for bias, for faulty design, for a poorly understood model. Sometimes, an observation based on a conversation with a single knowledgeable individual can be more valid than the results from interviews with 1,000 randomly chosen individuals. In assessing the relative worth of the two approaches, one must consider their advantages and disadvantages as presented. One cannot assume that one is necessarily better because it is more "statistical."

5.4 THE NET IMPACT MODEL

The development of a net effect model for the ERR program must incorporate some experimentally induced controls. Ordinarily, this would require the selection of

comparable experimental and control communities because of the way the program is directed to area employers. And, this would entail such a disruption to local office procedures that it is doubtful that it could be done. There is, however, an alternative approach which could achieve essentially the same purpose without requiring offices to greatly modify their approach to employers.

The proposed design involves selecting a set of cities (there could be as few as one) and structuring the experiment as follows in each of the selected cities. First, a baseline measurement would be made of those production measures to be used in the study. For example, if a compound study were to be developed with could determine each of the 11 production measures, then a representative sample of about 400 employers could be used to determine all outcomes except measures (4), (8), (9), (10), and (11). These would be developed by means of supplementary samples of ES users.*

Within each area selected for the study, heavy users of the ES would be identified, most likely from the ERR files.** These employers would be assigned into two groups, controlled by industrial code and size. One group would then be targeted for ERR activity, the other group essentially excluded from it.*** This division

*This sample size could be used if one were interested in detecting a difference of less than 5 percent to a 95 percent confidence level. If one wanted to disaggregate results by city type, then approximately 400 would be needed for each type. If the larger sample size could not be used, a less precise estimate would have to be accepted.

**Throughout this discussion, the focus is on the ERR side of the employer services program, since this would naturally be of most interest in a national evaluation. It should be understood, though, that where technical services are also being delivered, sampling would include recipients of such services, and the same experimental approach followed.

***Requested service would be provided. The ERR's would just be excluded from initiating contact.

would ensure that the experimental and the control groups were as alike as possible for this critical group of employers; i.e., the group representing most ES orders. For all other employers, a simple decision rule would be used; e.g., all employers whose firm name begins with A, C, E... will be targeted for employer services activity, all whose firm name begins with B, D, F... will be excluded from employer services activity.* The reason for using the two methods, one for heavy users and the other for other employers, is to make certain for the critical class of users that as accurate a division as possible has been achieved. Since these users represent, on an average, only about 25 percent of the area employers, each sample of a few hundred employers from the target and control groups would contain only about 50 such employers. If a simple random assignment rule were used between the groups, the sample variance for the 50 would be too large to detect the difference due to program events. Careful stratification would reduce this risk.

Following the division, in each chosen office the staff would target, in a manner they felt appropriate, or according to formal instructions, the experimental group of employers.** Employers in the other group would not be contacted unless they requested service. The staff would be expected to keep accurate logs of all contacts.

*It is unlikely that the letter designation correlates with critical firm characteristics (size, industry, etc.). Therefore, it is a much safer rule to use than area of city or similar designation. To avoid even this problem, one could use two digits of the employer identification number. Although this would result in a more "random" distribution between the two groups, it would be more difficult to control since the ERR might not know the number at the moment a decision about a contact is being made.

**The difference in method of targeting would depend on what the experiment was designed to do. If the experiment were structured to test the ERR program as it is operated, each office would continue to provide services in whatever manner was normal to them. The only difference would be that the group of employers would be restricted. If the experiment were designed to test a formal ERR program, or alternative programs, each office involved in the experiment would be requested to adhere to the standards proposed in the experimental ERR manual.

Every three months following the start of the experiment, a sample would be taken from each group, and measurement of each of the selected study measures would be made. Because of the time lag between program activity and stable ES use patterns, these measurements should continue for at least six cycles, or 18 months after the start of the experiment. Over time, one would then have a set of point estimates for each of the production measures as shown in Figure 5-1.

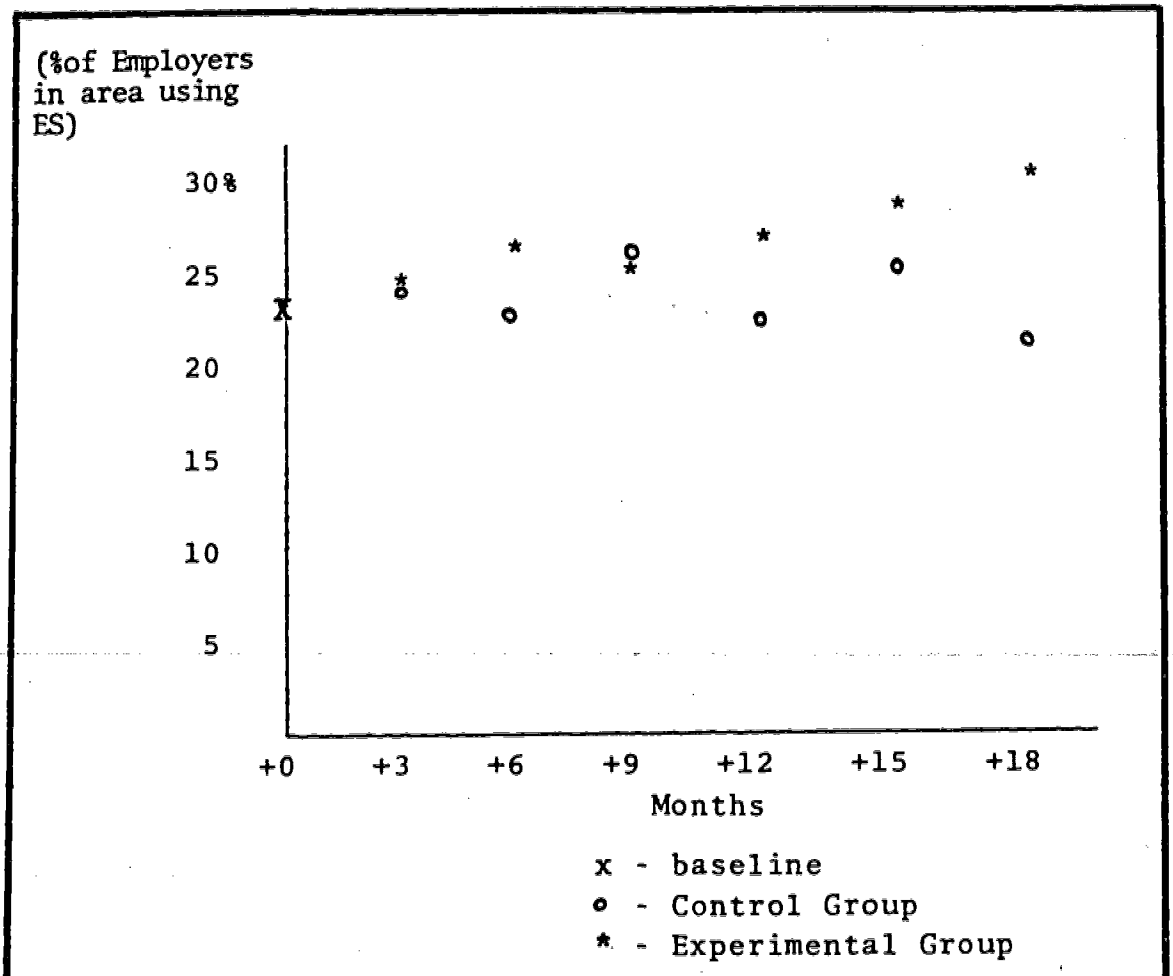

Figure 5-1: Point Estimates for One Outcome Measure

The plot in Figure 5-1 shows the employer penetration achieved for the control and the experimental groups. As noted, the baseline measurement for the community is 24 percent. Each subsequent plot (at three month intervals) represents independent measurements for the experimental and control group, the "*" and the "o" respectively. As seen in the figure, it would appear that the experimental group was being affected positively compared with the control group. Any number of techniques could be used to determine if, in fact, the observed differences across an individual community or set of communities were statistically significant. These tests could either be by means of static measurement of average differences or by means of trend analysis.

The remaining subsections of this paragraph discuss each of the steps of the proposed net effect model, discussing alternatives where appropriate.

5.4.1 Selection of Cities or Areas for Analysis

The number or nature of areas to be selected depends entirely on the hypothesis to be tested. In theory, a single city could be used to see if the ERR program can produce a difference in ES production measures, but one would not be certain that the findings (positive or negative) were due to characteristics of the class of cities to which the representative member belonged, or to anomalies within the city itself. If two -- or preferably three -- cities (areas) of each type of interest were selected, however, a good measure of the environmental effects on the program could also be determined.* For example, suppose one were interested in differentiating large, mid-size, and small communities. Three chosen from each group would provide an adequate universe to determine, by means of an analysis of variance or similar model, if city (area) characteristics also contributed to observed differences in the ES production measures.

*The sample size of employers would still have to be at least 400 for the smallest class of interest. This could result in a large, expensive study if precise estimates were desired for the effects of program activity within certain classes of cities.

Whatever the area characteristics of interest, one would probably begin by examining the characteristics of the 231 standard metropolitan statistical areas (SMSA's) to select SMSA's of interest for the study. While the sample could be chosen on any number of variables of interest, it would be useful to end up with groupings of communities chosen on a relatively small number of variables; e.g., population, industrial mix and characteristics,* unemployment rate. In the absence of the specific features of interest, it would be best to eliminate areas with abnormal unemployment situations, single-industry areas, and other anomalies. And, it would be desirable to have a reasonable geographical distribution of selected communities.

The inclusion in the community sample of areas not in SMSA's is arguable. Since it is reasonable to assume that situations and program characteristics are different in rural areas from those in SMSA's, an experiment confined to SMSA's will not be applicable to non-SMSA conditions. On the other hand, rural employer services operations are largely Level 1 situations,** with little opportunity for substantial program expansion or alteration. The experiment might well establish for such situations that significant increases in employer services activity could indeed improve outcomes, but this would be of limited interest unless the resources were provided, on a continuing basis, to permit such an expansion. Since rural areas and small towns represent only a small fraction of the overall ES production figures (the great bulk being concentrated in the 25 largest cities), any increase in listings in such areas

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*For all SMSA's, the Bureau of Labor Statistics maintains information on employment by industry, hours, average weekly earnings, and labor force characteristics. The Bureau of the Census provides number of establishments, number of workers, number of production workers, value of manufacturing, capital expenditure -- overall and by industrial classification.

**Program levels are defined in Section Three.

might be too marginal to justify the expense. A local increase of 20 or 30 percent might represent a national increase of only a small fraction of one percent. As with some other decisions the decision of whether to include non-SMSA sites could be made simply on the basis of policy interest: if there is interest in examining such situations, possibly with an intention of allocating more resources there if the evaluation shows such reallocation to be warranted, some non-SMSA areas could be sampled.

5.4.2 Program Analysis

Once the initial sample of SMSA's were selected (assume a sample of 25), the next step toward an eventual sample of local offices would be to examine the structure of the employer services program in the SMSA's of interest. This necessarily time-consuming process would entail -- through inquiry of state and local ES-personnel -- identification of the location of each local office providing employer services, and a rough description of the program. At this stage, it would be enough to know which level of ERR program was operating, and what technical services (if any) were being provided.

Since this type of experimental design measures the difference over time caused by withdrawing ERR acitivity to one group, and supplementing it for another, the type and level of activity can be important to the success of the model. For example, an ERR program which was directed only at a particular industry might be an interesting choice for the study since the experiment would be testing, except for that industry, a true zero-treatment effect. The other industries in the control group will never have been exposed to ERR services. Similarly, if one were interested in determining if there is a difference between activity resulting from a consolidated program in the job bank and individual effort at each office, appropriate areas and offices could be chosen.

5.4.3 Selection of Employers and the Baseline Study

The selection of employers is the most critical part of the study. It is important to distribute those

employers known to have been most closely associated with the ES between the experimental and control groups far more carefully than by simple random assignment. By using the ERR and technical services contact logs, the group receiving significant service would be divided into equal experimental and control groups, matched by size and industry. Since there is no economical way to divide all employers in an area into corresponding sets, the remainder would be divided, as earlier mentioned, by means of some simple random-sorting scheme. The one which would seem to pose the least risks would be to use "odd" and "even" letters of the alphabet, or "odd" and "even" numbers of the employer identification number.

One would end up with four strata as shown in Figure 5-2. If the study is to be conducted over a number of areas, the corresponding samples from each would be accumulated in the appropriate stratum.* Of course, only those employers in Stratum One and Stratum Two would actually be known. The divided letters would be "holding" categories for the employers they represent.

	Experimental	Control
Employers heavily involved with ES by industry and size	*One*	*Two*
All other employers A, C, E...	*Three*	*Four*

FIGURE 5-2: Design Model

*The strata for "users" actually consist of the sub-strata for industry and size. Whether these would actually be used as separate strata in selecting the sample would depend on the size of the sample and other design considerations.

At the same time that this division is taking place, a baseline measure of the production measures of interest would also be determined. A sample of employers (400 to 1,000 depending on the confidence desired)* would be selected from the two primary strata: those heavily involved with ES and all others. The suggested method would be the same as that used for Camil's study of job search, recruitment, and the USES. A working file, and subsequent sample would be taken from the ES 202 reports, which is the best local-level comprehensive listing of employers. Each selected employer would be contacted by telephone for a simple screening interview:

During the last three months, did you recruit for any staff?

(if yes)
Could you describe the occupations...

The number for which you recruited...

Were any of these occupations listed with the ES? Which?

Were any persons hired as a result of ES referrals?

*and so on...***

*The standard deviation of the estimate of the actual proportion used for a simple random sample would be $\sqrt{\frac{p(1-p)}{n}}$. A sample of 400 would yield a standard error of 2 percent, assuming p=.75. Thus, to a 95 percent confidence, the error would be $\pm$ 4 percent since $t_{.025,\infty} = 1.960$. By taking a larger sample from the first stratum, since the standard deviation is larger (the proportional usage is higher), and a smaller from the second (the proportional usage is lower), the error could be somewhat reduced. The error for a sample of 1,000 would be 1/1.6 as great, or $\pm$ 2.5 percent for the case of a simple random sample.

**This could be verified by reviewing the closed job order file.

For each measure of interest, this would constitute a t_0 measure for overall population. Displayed in tabular form, it might be as shown in Figure 5-3.

Measure	t_0	t_1	t_2	t_3	t_4	t_5	t_6	t_{max}	t_{min}	...
(1)	.25									
(2)	.22									
(3)	.36									
.										
.										
.										
(11)	.85									

FIGURE 5-3: Measurement Chart

NOTE: Each number refers to one of the measurement methods described on page 59 .

5.4.4 Structuring the Test

There are two potential approaches to the test: (1) predetermining an employer services policy which would be standard over all offices, and (2) allowing the employer services staff to pursue their contacts in whatever ways they have been accustomed to. There are advantages and disadvantages to each approach. In the structured approach, one is testing something more

consistent and definable than in the unstructured approach. However, what is being tested may not be readily implemented or implementable in practice. The unstructured approach measures the effect of what is actually done in the field, but if there is considerable variation in approach and "effectiveness" one is simply determining the effect of "what is" (undefined) rather than of "what could be."

For either approach, however, a standard series of instructions for the program test would be given to each employer services staff member in each sampled office as follows:

> Here are two lists of employers who have received employer services from this office over the past year (lists 1 and 2). Until further notice, you are to make no further contact with the employers on list 2. You may continue to make contact with, and provide services to, the employers on list 1. However, should any employer from list 2 *contact you*, you may provide requested services. Log all contacts and services carefully.
>
> In making new contacts with employers not identified on the lists, restrict yourself to those employers whose firm names begin with "odd" letters of the alphabet: A, C, E, G, etc. Make no new contacts with unlisted firms whose names begin with "even" letters: B, D, F, H, etc. Should any employer whose firm name begins with an "even" letter *contact you*, you may provide requested services. Log all contacts and services carefully.

Whether the ERR's and technical services personnel would carry out their tasks in their normal manner or would follow a set of prescribed techniques would be determined by the nature of the experiment to be tested. Naturally, by simply increasing the sample size to include comparable community sets, one could both determine whether ERR services, as instituted, are effective, or whether special approaches are preferable. It is simply a matter of time and money. If there were also interest in the particular characteristics of approach or of staff that led to success, this could also be isolated, provided enough observations were present. For example, experience, training, or previous ES responsibilities could be identified and then related to outcome. Similarly, styles of approach (degree of formality versus informality, appointment making versus dropping in, dealing with personnel offices versus other levels of management) could also be isolated.

This last point is potentially significant, and apt to be bypassed in this relatively "hard" evaluative approach. Camil's previous exposure to the employer services program, which is substantial, suggests that one factor in the success of the program which can be overlooked is hard to pin down: the "personality" of the ERR or other staff member making the actual employer contact. Many effective ERR's function by personal, first-name-basis contact with individuals who make hiring decisions in key industries and businesses. They may also make the rounds of civic clubs, chambers of commerce, and fraternal organizations, cultivating essentially social contact with personnel directors and others who have hiring responsibilities. Such contacts may not be sufficiently formal to show up in staff logs or reports, but can be strong contributing factors in employer services success. To assure that such factors are adequately considered in the national evaluation, two precautions are necessary: (1) a thorough description of the "event" -- that is, the employer services program as it is manifested at the local office -- must be developed and used in reaching conclusions about the contribution of the program to the outcomes of interest, and (2) an end-of-study "debriefing" interview of employers, which includes open-ended questions aimed at getting an employers' eye view of the characteristics of effective programs, is required. It should be

noted that these steps are equally necessary for the "softer" evaluation approach (the employer survey methodology) as for the "harder," statistically rigorous, methodology. For the latter, they are necessary if one is to know what "extensive ERR activity" means.*

Some variation could be deliberately controlled. For example, some sampled employers could be contacted only by telephone, others only by direct visit. Similarly, some could be contacted with greater frequency than others, some could be aggresively offered technical services while others could not, types of technical services could be varied, and so forth. Such variations could indicate not only whether an event called "employer services" was associated with the outcome measures, but which configurations of services influenced which outcomes.

5.4.5 The Continuing Surveys and Analysis

Following the implementation of the design, quarterly surveys would be made of samples drawn from the two groups of employers in each of the cities. These samples should not be of a selected stable set of employers subject to special ERR activity, but of all employers in the groups to reflect the target-group concept of the design. Each quarterly survey should also be independent, rather than of the same employers each quarter.**

*This points up one of the most difficult areas in manpower research: measurement. Very often experiments are designed to determine the effectiveness of components such as counseling, ERR services, training, etc. However, there is such variation within each component area that composite measurements are often meaningless. For example, analyses of the worth of counseling will often lump together as counseling lengthy discussions and telephone interviews, or counseling geared to overcoming personal problems and counseling geared to making a vocational adjustment, etc.

**In theory, this reduces the precision of the experiment since the error sum-of-squares would normally be reduced by means of a longitudinal study of employers instead of independent group samples. However, there is a danger of bias being introduced because of repeated contacts with the same employers. Moreover, if one deals only with a small employer universe, the ERR program could be unrealistically concentrated.

This means that quarterly, independent samples would be taken from each of the four strata to determine the degree to which any difference in outcome is detectable. Unfortunately, a fairly large sample must be taken if one expects to measure the small differences produced by the presence or absence of the ERR program. To see this, assume only two strata, the experimental and control groups, and assume that the experiment should be able to detect a difference in a production measure of five percent. That is, the experiment should be able to detect that a difference of five percent (e.g., 20 to 25 percent) between the two groups is significant or not. Assume that the experimental group and control group sample sizes are the same and that the difference observed between the groups is five percent due to a 20 percent observation in the control and a 25 percent observation in the experimental group. Since the variance of the difference between means is the sum of the variance, our statistic would be:

$$t = \frac{.05}{\sqrt{\frac{.225(.775)4}{n}}}$$

where n is the overall sample size.

Therefore, $n = 280t^2$

if a 95 percent confidence is wanted, a sample size of 1,075 would be needed since $t_{.025,\infty} = 1.96$*

*If we are only interested in determining if the production increase is greater for the experimental group, a one-sided test would give a sample size of 758.

Although some reduction could be achieved because of the stratification, one is nevertheless dealing with an overall sample size considerably larger than the one needed for the baseline estimate to achieve the same level of precision. Because the difference produced by the ERR program will probably not exceed five to 10 percent, samples of 1,000 over the entire sample every quarter are probably reasonable, unless considerable interest is expressed in differential effects or sub-classes of employers, cities, offices, etc. -- which would call for larger samples.

Over the six recommended periods, this would give both six independent observations as well as cumulative totals of 6,000 observations. The reason for needing such large independent samples is to have adequate representation in case differences can be detected only in the late samples, t_5 or t_6. This would occur if there were considerable time lag in the effect of application and withdrawal of ERR service. If the effect accumulated linearly, one might expect a trend as shown in Figure 5-3.

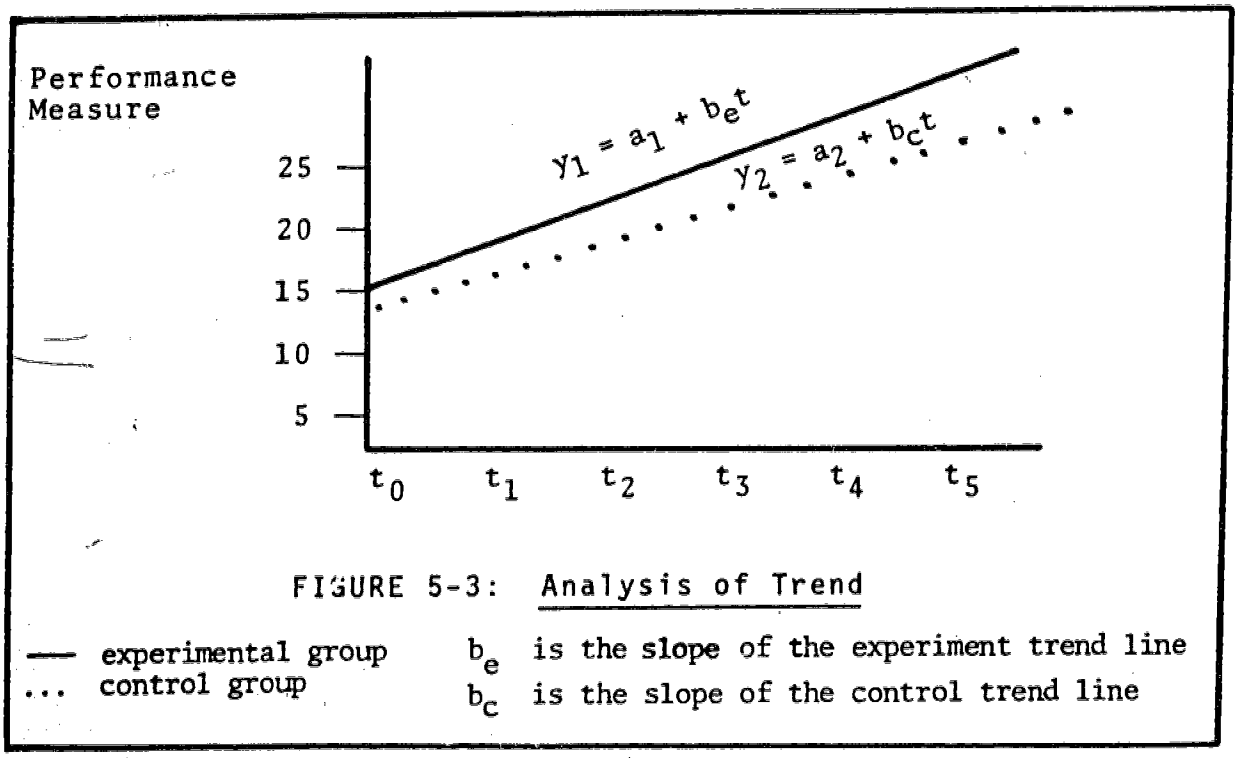

FIGURE 5-3: Analysis of Trend

This figure shows two lines and the associated equations for a given production measure. Since these are simple linear regressions, it would be possible to detect the differences between b_c and b_e to great degrees of precision if the ERR program contributed to any significant portion of the particular measure under test. In fact, if one could estimate the probable trends, prior to the experiment, reduced sample sizes at each stage would be possible.*

If trends are discernable, an analysis of variance models could also be used to determine the relative contribution of ERR activity compared with other distributional factors; e.g., city size, employers. As shown in Figure 5-4, a simple two-way design could test for the relative contribution of environmental versus ERR effects. Similarly, multi-variate analysis could also be used to test specific hypotheses. Since there are any number of methods which could be used to examine the data, whether for trends or static estimates, the important consideration at this time is not the methods themselves, but the reliability of a design which would permit an effective evaluation.

City Type	Reduced ERR Activity	Increased ERR Activity	Average
Large	$X_{1,1,1} \cdots X_{1,1,n}$	$X_{2,1,1} \cdots X_{2,1,n}$	$X_{.1.}$
Moderate	$X_{1,2,1} \cdots X_{1,2,n}$	$X_{2,2,1} \cdots X_{2,2,n}$	$X_{.2.}$
Small	$X_{1,3,1} \cdots X_{1,3,n}$	$X_{2,3,1} \cdots X_{2,3,n}$	$X_{.3.}$
Average	$X_{1..}$	$X_{2..}$	Overall Average $X_{...}$

FIGURE 5-4: Analysis of Variance Model

*It would also be possible to structure an impact model with the difference the direct result: e.g., $y = a + bx$ where x is "1" if in the experimental group and "0" if in the control group.

5.5 THE EMPLOYER SURVEY MODEL

This methodology contrasts significantly with the net impact approach. It relies on observations at a single point in time, rather than repeated observations over time. It requires no induced variation, nor any other change in normal local office routine. And it infers causality not through statistical association, but through employers' own views of what causes their ES-related attitudes and behaviors.

Like the net impact model, this approach requires a sample of ES offices, and sample of employers in the areas served by those offices. The local office sample could be developed in the same manner as that suggested for the net impact study (pp. 67-69).

The employer sample, though, would be developed somewhat differently since the survey approach does not entail experimental design. In each sampled community, there would be initial screening interviews of employers selected at random from the ES 202 files as described on page 71. Here, though, the screening would be considerably simpler than that needed for the net impact sampling. Employers would be asked whether they have recruited for any position during some period of interest (say, the past year). Those who answer no would be immediately dropped from consideration; those who respond affirmatively would be asked whether they listed any openings during that period with the employment service. Those who said yes to that question would be asked one additional screening question: whether they have been contacted by a representative of the employment service during the time period.

This would provide the basis for selecting three sub-samples for further inquiry:

(1) Non-users. Employers who recruited, but did not use the employment service.

(2) Unserved users. Employers who listed openings with the employment service, but were not contacted by ERR's or technical services representatives.

(3) Served users. Employers who listed, and did receive employer services.

Before selecting the final probability sample for each of these groups, groups (2) and (3) should be checked against employer dictations between events as reported by the screened employers, and as recorded by the ES. Because the event in question -- contact of the employer by the ES -- is historical and straightforward, it is unlikely that the employers' reports will often be contradicted by ES files. At sites where there is such contradiction, though, a special sample could be drawn of those employers whose accounts differed from those of the ES; i.e., who either reported contact where the ES files indicate there was none, or who reported no contact when the ES files indicate contact was made. While some of these anomalies will prove to be cases of faulty memory, it may transpire that employers' perceptions of what constitutes a "contact" by the ES differs from the contact event as defined and recorded by the ES itself.

After this checking process, random samples of employers from each of these groups, stratified by industrial code and work-force size, would be developed. While sample sizes depend on the degree of confidence desired, and the number and type of sub-populations to be examined within groups, it is probable that class (3) would be sampled more heavily than (1) and (2), since the served users group is in a position to contribute more insight of policy interest than the others, and is a group that it may prove useful to sub-divide into further categories as the study progresses.

Depending on the hypotheses to be examined, it may also be advisable to draw several special samples. Possibilities would include:

- A special sample of the employers in the community who are the greatest users of ES services; that is, who account for disporportionate numbers of listings and hires. (The "best customers.")

- A special sample of those employers who have been particular targets of employer services activity.

- A special sample of employers who have received technical services during the period of interest.*

There are many other possibilities, of course -- government contractors and other employers required to list openings, employers who frequently list but rarely hire ES referrals, employers who have served on ES task forces or advisory committees (perhaps under ESIP or some other special program), employers who have been "written off," for one reason or another, by the employment service -- the options are limited simply by research areas of interest, and by time and money. Unless an extraordinarily complex design involving many sub-samples were needed, an overall national sample of 3,000 employers, half of whom were served users, should be more than adequate for any study purpose.**

A separate questionnaire would be developed for each of the three main sample groups (with variations for any sub-samples, as needed). Because employers' perceptions and ideas are important in this methodology, a considerable proportion of the questions would involve probing, and would be open-ended. This suggests that, ideally, the interviewing should be conducted face-to-face. A less desirable, but also less expensive, alternative would be to interview most employers by telephone, reserving face-to-face interviewing for a randomly selected sub-sample of, say, 20 percent.

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*Because employers who receive technical services are a small universe, a special sample would probably be necessary to provide an informed view of the use and effectiveness of technical services. Such a special sample could be further disaggregated by type of service provided. It would be drawn directly from ES files, not from community-wide screening -- as would *all* the special samples.

**As discussed below, pp. 92 - 93.

This type of inquiry requires some subtlety; it is important to avoid launching into an immediate battery of questions about employer services, which can appreciably bias the results. Instead, interviewed employers should understand that the study is sponsored by the employment service (this is inescapable if their cooperation is to be secured), and that the subject is recruiting for, and filling, jobs. Then, the early portion of the interview is devoted to personnel recruiting and hiring decisions in general. The topic of employer services is first introduced in multiple-choice questions (several examples are given below) in which employer services possibilities are intermingled with others. Specific questioning about employer reactions to employer services, and their suggestions for improvements in the program, are reserved for the very end of the interview.

This is important; previous interview projects have shown that introduction of the element of interest directly into an interview focuses the respondent's attention on that element, producing an exaggerated account of its significance. It is better to ask the employer about worker-seeking methods in a general way letting the employer introduce the topic of the employment service, than to immediately focus in on the ES. Similarly, it is preferable to have the employer rank the contribution of the employer services program to recruitment decisions on a list which includes other factors, than to attempt a frontal assault: "How important is the employer services program to you?"

Thus, the interview would begin with general questions about recruiting and hiring. "What methods do you most often use to recruit (category of workers)?" Only after the topic of the ES has been introduced naturally would the questions move to the employer services program specifically.

The format of many questions would involve a ranking of factors that may contribute to decisions to list jobs, or hire persons referred by ES. For example, for users, such questions as the following could be used:

(1) Please rank, in order of importance, the factors on this list that cause you to list your job openings with the employment service.

__ Good experience with ES referrals

__ Required to list (e.g., government contractor)

__ Contact from local ES office (Do you prefer telephone contact ___ or personal visits by an employment service representative ____?)

__ Other sources of employees inadequate

__ Employment service does not charge a fee

__ Employment service has provided special services for my firm (testing, job restructuring, help with affirmative plans, etc.) Specify _____

__ Other (Specify) _____

(2) (For employers who became users for the first time during the past few years). Please rank, in order of importance, the factors that led you to begin listing jobs with the ES.

__ Good reports from other listing employers

__ ES advertising campaign (Radio___ TV___Mail___)

__ Required to list (e.g, government contractor)

__ Other sources unsatisfactory

__ Direct contact with ES (Phone___ Personal visit___)

__ Employment service provided special services for my firm. (Specify) _____

Non-users would be asked a number of questions about their reasons for not listing jobs.* Then, a scale such as the one below could be employed:

(3) Which, if any, of these factors might lead you to list jobs with the the employment service? Please rank in order of importance.

__ Nothing would lead me to list jobs.

__ Favorable reports from employers in firms similar to mine

__ A contact from an employment service representative to explain the service and learn about my personnel needs.

__ (Would you prefer contact by mail?___ Telephone?___ Personal visit?___)

__ Problems with other hiring sources.

__ Provision of a technical service (testing, job restructuring, help with affirmative action plans, etc.) Specify_____

__ Other (specify)_____

The most difficult area of inquiry with this method is the likely behavior of these employers who never list jobs with the employment service. Employers who choose the first option in the question above ("Nothing would lead me to list jobs") are pretty well lost to the ES, though some follow-up questioning with

*In the job search and recruitment study, most non-users simply didn't need the ES; they were able to fill all their openings through other methods.

them could still yield some useful clues for the employer services program. Why are they so adamant? Have they had previous unsatisfactory contact with the ES? Have they heard negative reports from other employers? Do they have a particular image of the ES that might be countered by effective public relations work? Or, are they simply satisfied with their present hiring sources (as were most non-users in the recruitment and job search study)?

For those who choose some other options in Question (3), additional follow-up is required; some of this should be devoted to the problematic area of what it would take to *keep* such employers using the ES (as opposed to simply persuading them to try it once). This could be explored with such questions as:

> If you listed a job with the employment service, and the job-seekers referred were satisfactory, would you be likelier to:
>
> __ Use the ES again, without any additional encouragement.
>
> __ Consider using the ES again, but only after additional contact from the ES office.
>
> __ Continue to use your former recruiting methods, but keep the ES in mind in case other methods prove unsatisfactory.

Similarly,

- If you listed a job with the employment service, which of the following would be likely to cause you to refuse to list additional jobs:

  __ No applicants referred.

  __ Too many applicants referred.

__ Too much delay before applicants referred.

__ Unqualified applicants referred.

- If you were dissatisfied with the service you received from ES, would you:

  __ Be willing to try again, if an employment service representative heard your complaint, took the time to understand your situation, and initiated corrective action?

  __ Discontinue listing jobs.

- If the employment service were working with you to straighten out problems of referral, would you prefer that kind of contact:

  __ In person

  __ By telephone

These question are, of course, hypothetical. Unlike the present users, who can speak "historically" to the question of the value of the intervention of the employer services program, the non-users can be approached only in terms of what they would be likely to do in given circumstances. However, since the representatives being interviewed are knowledgable about recruitment and hiring, the answers they give are not guesses -- they should be, if the interview is well-conducted, thoughtful responses about alternatives to the recruiting and hiring methods they are presently using.

The served users, after their use patterns and overall satisfaction with the ES (again, in terms of the outcome measures) had been developed, would be asked a battery of questions specifically about employer services.

These would probe for the degree to which their listing and hiring have been influenced by provision of employer services; questions could be both general, and geared to a specific "critical event" -- e.g., a recent decision to list a job or a category of jobs. Satisfaction with the employer services program, suggestions for its improvement, preferences for one kind of service over another -- all this could be developed through interviewing which was largely informal and conversational.

This kind of interviewing is best accomplished using topic guides, rather than structured questionnaires. The topic guides specify the information required, but leave it to the interviewer to decide the wording and sequencing of questions as appropriate. Because this method puts the burden of data-collection on the interviewer, rather than on the questionnaire, it demands the use of professional staff knowledgeable about the program, and involved in the other aspects of the study. Such interviewing cannot be accomplished by hasty recruiting of local interviewers; ideally, it is carried out by the same staff responsible for the rest of the evaluation, from design through analysis and reporting. In any event, it is essential that the interviewers have a thorough working knowledge of the employer services program in general, and of the services provided to employers in the area in which they are interviewing in particular.

A portion of such an interview might go as follows:

INTERVIEWER: Thinking back to the last time you recruited workers... when was that?

EMPLOYER: Oh, about four months ago.

INTERVIEWER: What kind of job was that?

EMPLOYER: We needed some equipment operators for a new contract.

INTERVIEWER: How did you recruit for those openings?

EMPLOYER: We ran an ad in the newspaper, we contacted the employment service, and we told our employees we were hiring so they could refer people if they wanted to.

INTERVIEWER: Did the employment service refer anyone?

EMPLOYER: Yes, they referred two men and we hired one of them.

INTERVIEWER: Why did you list that particular order with the employment service?

EMPLOYER: I've been listing all my equipment operator openings for several years.

INTERVIEWER: How did you first learn of the employment service?

Examining this hypothetical excerpt, one can readily see how flexible this kind of interviewing must be. The interviewer's goal is to determine what kinds of factors go into the employer's ES use pattern. Had the employer indicated the order was not listed with the employment service, the interviewing would have had to veer off in another direction, as the interviewer sought to determine why not, and what it would take for the employer to be interested in listing. Following the sequence given above, the interview will continue to determine the contribution of the employer services program to the employer's listing and hiring decisions. Later, another portion of this same interview might go as follows:

INTERVIEWER: How often are you contacted by the employment service?

EMPLOYER: Every couple of months or so.

INTERVIEWER: Is that by phone or personal visit?

EMPLOYER: Phone. They call to see if I'm hiring, and how other people I've hired through them are working out.

INTERVIEWER: Is it always the same person who calls?

EMPLOYER: Yes, it's a Mr. Daley.

INTERVIEWER: Do you know his job title at the ES?

EMPLOYER: No.

INTERVIEWER: Would you prefer to be contacted by personal visit instead of by phone?

EMPLOYER: No, the phone is okay.

INTERVIEWER: How about the frequency of calls -- is every couple of months too often, or about right...?

EMPLOYER: Actually, it's not necessary for them to call at all.

INTERVIEWER: Why do you say that?

Again, it can be seen that the interviewer has to be prepared to follow the lead of the employer in getting the necessary information. This too, requires skill; the interviewer must have some sense of which digressions are leading to important information about the perceived effect of the employer services program, and which are just aimless rambling. The best guarantee of this is the interviewer's familiarity with the employer services effort.

The interviewer also uses his or her own judgment in determining the extent to which "critical incidents" (specific listing or hiring decisions) should be probed. The very first case discussed may lead in naturally to the

other information sought. Or, it may be a dead end, requiring consideration of another instance. The goal is to arrive at an understanding of the employer's listing and hiring decision, not simply in a general way, but specifically in regard to historical patterns -- the actual decisions that have been made.

The possible presence of a causal relationship between provision of services and decisions to list or hire (and other outcome measures) is investigated in this method by asking the respondents to consider the influence of services, rather than by statistical correlation. It is reliable only insofar as employers' knowledge of the factors that influence their staffing decisions is reliable. Both as researchers and as an employer, we believe that such employer perceptions *can* be relied on, particularly since the area under investigation is relatively noncontroversial. And we believe the essentially soft, employers-eye view of services that this approach will yield, will prove useful to ES administrators at all levels in allocating service resources, and in improving the usefulness of the services provided.

5.5.1 Analysis in the Survey Model

The analysis of this type of study design is considerably simpler than that involved in a formal net-effect approach. It consists of two related parts: (1) estimation of proportions and distribution parameters, and (2) estimation of relationships between key performance variables and other variables, including the presence or absense of employer services contacts program characteristics.

The first part (estimation of population measures) derives directly from the nature of the sampling plan. As discussed earlier, three classes would be selected: non-users, unserved users, and served users. Since sampling would be independent within each class to ensure enough examples for analysis, the relationship across the classes would be maintained by *a priori* weighting. In Figure 5-5, the relationship between the classes obtained from an ES 202 sample is shown with a tentative set of sub-samples from each group. In the example shown,

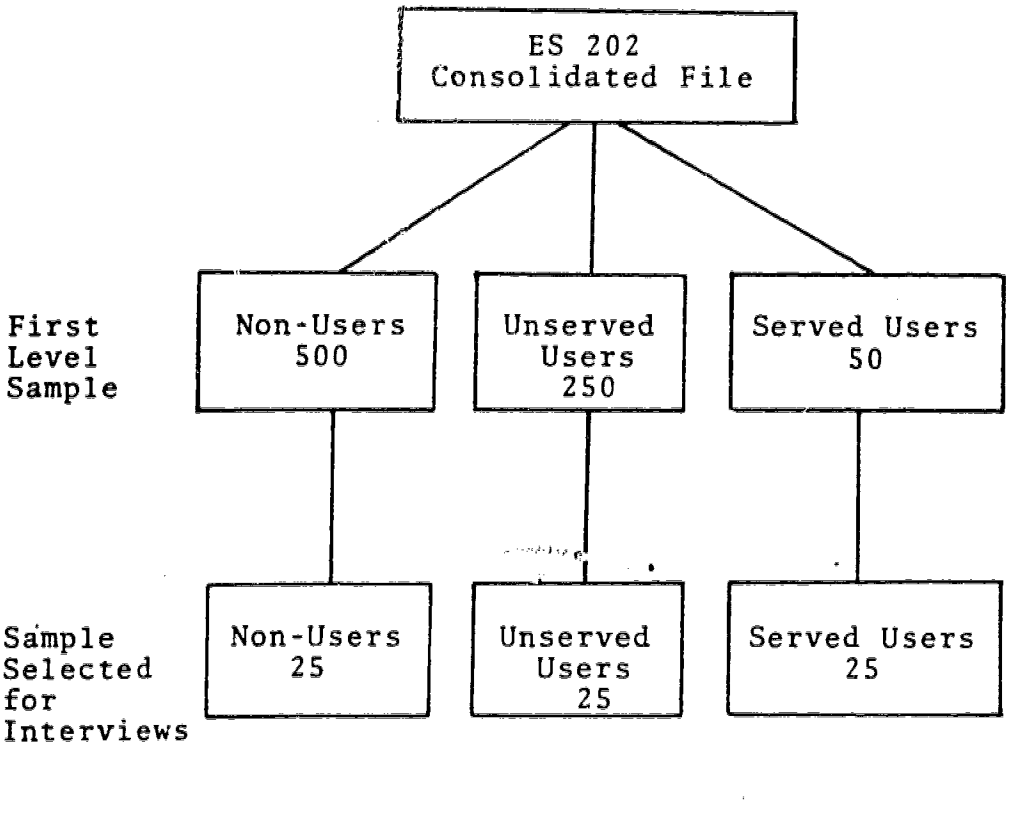

FIGURE 5-5: Sample Structure

each member of the non-user class would have a weight of 20, of the unserved user class a weight of 10, and of the served user class a weight of two -- each representing the relative proportion taken to the number potentially available.

In this type of weighting, recombination across classes has produced an unbiased representation of population parameters and proportions. The estimation of error is also performed by straightforward techniques for multi-stage, non-proportional samples. Although these are often mathematically complex, they are tractable.

The principal analysis, however, would be based on observations within each class. For example, the percentage of non-users who would use the ES under certain conditions; the percentage of served users finding the ERR contact of no value; the average number of orders listed with the ES by unserved users; etc. For these independent estimates, one is dealing with simple random, or at least, proportional stratified, samples within each class. In these cases, the directly obtained sample proportions and parameters are estimates of the population, and the standard error of the estimates is obtained by means of the estimator for the population standard deviation divided by the square-root of the number of sample points. It is therefore, unlikely that any estimator will present problems in such a sample design, unless some unusual estimates are required across classes.

The second part, (estimation of the relationship) is similarly statistically trivial in most cases. Because of the nature of the scales and questions, most relationships would be determined by means of proportional estimates rather than by means of standard correlation techniques. For example, the percentage of cases in which the ERR visit (contact) is ranked 1st, 2nd, 3rd,... or not mentioned at all provides the basis for its importance in the listing decision. Similarly, the percentage of persons who might be willing to list if the ES were explained to them, or the percentage would never use the ES after a bad experience (even with a visit), define the importance of the program activity in the same way that the trend analysis would for the model described in Section 5.5. However, instead of correlation between shifts in

performance measures and ERR events, one now simply measures the number of certain types of events themselves. If one assumes that the attitudes and statements expressed by employers approximate what their actual behavior would be, the two models will yield approximately the same results -- the latter by a more direct route.*

Some correlation may also be needed if one wants to estimate the relationship between certain employer program or ERR characteristics and the dependent performance variable, this time expressed by employers' indications of what their job-listing behavior was and what it would be under certain circumstances. For example, one might want to examine the ranking of a personal contact with the nature of the local office ERR program, or look at the percentage of orders listed compared to the ranking of the ERR visit, or the nature of the visit, or the nature of other program characteristics. These can be determined again quite simply by means of any of the multivariate or simple correlation and regression techniques discussed in connection with the net-impact model. For example, if one were to test the relative impact on the level of ERR importance ranking of employer and program characteristics, a simple regression equation could be constructed in which the ranking (one, two, three,...) would be the dependent variable, and a series of binary variables would represent alternative employer characteristics and ES program characteristics. Since there are any number of such straightforward techniques which could be employed, the problem is not in their application, but in the posing of the proper questions about the program.

A few final words should be devoted to sample size. In an experiment such as this, the principal factor

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*The problem lies not only in the degree to which expressed attitude and behavior coincide, but also in the extent to which the relationship is accepted. If policy makers reject the relationship between the two, such an experiment should not be conducted because the results will not be accepted. If policy makers accept that there is a reasonable correspondence, or that the extent of bias can be determined, the second model offers the more direct approach.

influencing sample size is the smallest cell one is interested in. If one wants to be able to state results by a combination of employer characteristics, e.g., SIC by size, for each principal class, the overall sample size would have to approach 10,000. To see this, suppose an equal number of employers were to be contacted in each class and that the SIC code was divided into nine categories and size into four categories, for the purpose of the study. This would give 36 independent classes, each of which would require at least 100 cases to differentiate accurately between the classes.* Since the ERR program is not expected to have an impact of much greater than 10 percent on most variables, enormous sample sizes would be required to differentiate impact for small clusters. We would suggest, therefore, that 3,000 should be a reasonable upper bound for the sample size, which would restrict one to 10 clusters within each major employer class. In other words, if one is willing to differentiate by size, by SIC code, by area characteristic, etc, but not by combinations, a sample of 2,000 to 3,000 would be adequate** If one must, however, differentiate by combinations, an even greater size may be needed, depending on the size of the class.

Because of this, special classes of interest should be identified prior to the study, and a sufficient number of cases taken for the class of interest.

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*For example, to test to see if one class was different from another, one would be concerned to see if the difference between the means were significantly different from zero. Since the variance of the difference is the sum of the variances, the standard error of a proportional estimate around .5 would be .07. Since $f_1 - f_2$ divided by the standard error has approximately a normal distribution, one could not say that a difference of less than 15 percent was significantly different from zero to a 95% confidence.

**This assumes a multi-site study. One would accept more modest results in a single site study, and would accept the limitations of a sample of 500 or fewer.

5.5.2 Use of Interviews in the Net Impact Model

It should be noted that some of the soft approach described above should also be incorporated into the net impact model, in the form of an end-of-experiment close-out, or "debriefing" interview with sampled employers. Such an interview could be much briefer than the one required for the employer survey approach, and could be conducted by phone (though again, face-to-face interviewing with a random sub-sample would be desirable). The focus would be on measures of satisfaction with the ES in general, and the employer services program in particular, with some questions directed to specific services received by the sampled employers. Suggestions for improvement of the program could also be sought.

5.6 UNDERSTANDING THE PROGRAM

Regardless of the method used to infer causality between the employer services effort and the outcomes of interest, one is left with the question: What is the "program" that produced (or failed to produce) the indicated results?

The question is important. The folklore of program evaluation is replete with examples of results attributed to programs, when both common sense and direct observation indicated the program was too weak (or too trivial) to have accomplished those results. In such cases, a second look often shows that outcomes are not the results of the "program," as its designers and funders understood it, but of either some exogenous circumstance unforeseen in the study design, or some program strategy (such as "creaming" applicants, or highly selective record-keeping) not implied in the program concept itself.

In the case of the employer services program, there are several choices. One could, notwithstanding the above caveat, treat the program as a single "event," and show its influence on the measures of interest. Or, one could categorize program types by straightforward variables: size, budget, staffing plan, level of effort -- and assess outcome differentially for each type. A rough categorization by operation level (pp. 24-25) is already "built into" the study since it is used in the selection of local offices (p. 69).

Such categorization allows for a more interesting presentation of findings, but is still of limited policy use. It could be enriched considerably by direct observation of the sampled offices to develop a more process-oriented typology of employer services operations. Any number of variables are possible, depending on research interest and budget: mix of service methods (i.e., personal visit, phone, mail, others), types of technical services provided, degree of autonomy of employer services personnel, participation of the office in one of the experimental models (ESIP, NCC, communication projects), involvement of employers in service planning and monitoring, degree of internal (or state office) monitoring and evaluation of the program, and so on. Even the "style" of employer services approach could be observed and categorized for later analysis against outcomes; as discussed earlier (page 74), the individual staff person's approach to his or her job may be -- and, in this case, quite probably is -- linked to success with employers. This suggests that conversations with ERR's and technical services personnel about their philosophy of employer services, perhaps coupled with actually watching the staff at work,* should be used to develop an idea of such important factors as the attention given to meeting with groups of employers (e.g., chamber of commerce committees, civic clubs, etc.), the degree of informality in staff-employer relationships, the strategies used to approach particular employers or specific industries, and other work methods that do not necessarily come from handbooks or program descriptions.

Approaches and techniques so noted can also be worked into the interviews with employers in the form of questions about preferred methods of contact. Thus, several questions on the employer interview schedule could be "personalized" for the local situation.

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*This is not as improbable as it sounds. In an unobtrusive process evaluation, one often has casual opportunities to observe staff in action -- even to the point of getting invited along on employer interviews.

These variations in approach are largely informal, and even personal -- that is, they depend on the personality, philosophy, and operating style of the individual contact person. They cannot be discerned without spending some time with the ERR or other staff members, engaging in conversation, and taking the time to see the job as the staff member sees it. Such differences of approach do not show up in state plans, nor local office budgets. But they can be important in developing a working description of the program "event," enabling an examination of which approaches are more effective. This kind of program description is inevitably intuitive, and relies on the judgment of the observer -- but it is also invaluable in program evaluation.

It should be noted that this variation of approach will not necessarily distinguish one local office from another, but may afford clues to more or less successful approaches within the same local office. It could not be economically used as a sample selection criterion, but can be used at each sampled site as part of describing that local manifestation of the program. Ideally, for the net impact model a process appraisal of each selected local office should be undertaken at the beginning of the experiment (when the other baseline data are being acquired), again about half-way through, and a final time at the end of the study, so that changes and trends in operations can be noted and examined against the time-series for the various outcome measures. For the employer survey approach, a single appraisal any time during the study would suffice.

5.6.1 The Treatment of Costs

For either evaluation approach, one variable which can be assessed against outcome measures is program cost. Development of reliable cost figures, however, will require some budget analysis, and some direct observation, at each selected local office.

Part of the employment service reporting system associates cost figures with charge numbers for ERR and technical services, and these figures are reported from the local office up. Levels of effort (person-time) are also reported. While the cumulated statistics generated from this reporting at the national level are generally

considered to be reliable, at the local office level the numbers alone do not permit accurate comparison of the costs of various services.*

In retrieving cost information, there are two kinds of problems:

(1) Total program costs are not simply the salaries paid to employer services staff. In addition to that "direct" cost, there are other, "indirect" costs: fringe benefits, costs of mail and telephone, travel to employers' locations, and a *pro rata* share of local office overhead: space, utilities, equipment, secretarial and administrative expenses, and the like.

(2) Even when accurately derived, the total program cost, by itself, is not a particularly interesting figure. The total cost should be broken down by type of

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*We are assuming that any evaluation of the employer services program, by whatever methodology, will investigate the relative effectiveness of alternative contact methods: visits, phone calls, mailings, media campaigns, and others. If the decision were made, however, to limit the evaluation to consideration of the impact of "the program," without further definition, cost analysis would be of marginal usefulness. (The total cost of the program, to a reasonable degree of reliability, is already known.) Only cumulations are reported uniformly by all states to the national office through the State Employment Security Agency Accounting System (SESA). For each account number (551, Employer and Union Services; 552 Employer Technical Services), monthly financial reporting provides funded and unfunded total expenditure totals for these items: Personal Services, Personnel Benefit, Non-Personal Services, and Total Costs. These data are reported on the national level, quarterly, in the National Activity Performance Report, which displays expenditures for Direct Personal Services and Total Costs for the quarter, and for the year to date.

activity, so that it is possible to compare the costs of alternative approaches: personal visits, telephone calls, mail campaigns, media approaches, or special efforts (e.g., "targeting" on a particular employer group, sending employers resumes of job applicants, working with employer organizations).

Getting around these probems is not particularly difficult, and entails nothing very sophisticated in the area of cost accounting -- but it does require a painstaking review of the local office budget, conversations with managers and employer services staff, and observations of the employer services program.

The first area, identification of indirect costs, can be approached from several directions. The simplest approach is to take the local office budget, and segregate it into two categories: direct salaries, and everything else. The ratio between the two categories is then calculated making possible a statement like: "For every dollar spent in the office for salaries, 75 cents is spend on something else (overhead)." This factor is then applied to the cost of the staff time devoted to the employer services program. In the example, for each dollar spent in salaries for the program, a "loading factor" of 75 cents would be added to give a total direct plus indirect cost figure.

Another method is to determine, by staff interviews and observations, what overhead items are specifically associated with the employer services program. It is likely, for example, that the program uses more than its "share" of the office travel budget, and accounts for a disproportionate amount of the telephone bill. It may also, in some situations, make a heavier drain on secretarial and duplicating services than do other office components. Interviewing managers and employer services staff, and observing the program in action over several days' time, could identify such factors and provide the basis for allocation of indirect cost somewhat more accurately than is possible through use of a uniform loading rate applied to staff salaries.

The second problem area -- calculating cost by type of activity -- similarly requires in-office interviewing, and direct observation of the program. If time-logs are kept by employer services staff, these can be sampled to determine the amount of staff effort devoted to each type of service. If not, this information can be developed by observation, coupled with having staff keep such logs for a brief period (say, two or three weeks). These would be neither time cards nor contact logs, but a kind of combination of the two -- a diary showing employer services activities, and the time devoted to each. (For example, 2:30 p.m. to 2:45 p.m. -- telephone contact with Taylor Products Co.) This would establish the staff time (direct costs) associated with alternative service delivery methods; the indirect costs could be computed using either of the approaches discussed above. The second method (staff interviewing plus direct observation) would be preferable, since it could associate service types with actual expenses: mileage, telephone charges, postage, etc..

The product of the cost analysis is, then, a verified program budget (by whatever line items desired), broken down by types of program activity. This provides a basis for examining whether, in particular situations, one kind of approach appears to be more cost-effective than another. More broadly, it could be used to show the "price" of a unit of a given service type, and variations in this "price" from one area to another.

The value of the cost-by-service-type data is that they can be compared with the effectiveness of alternative strategies. Using any (or any combination) of the outcome measures, a cost-per-success for each method could be derived, permitting a comparison of service approaches in terms of economy.*

* If cost considerations were a major emphasis of the evaluation, it would also be feasible to structure a controlled experiment in which some sites increased their employer services budgets, while other comparable sites did not. The increases could be devoted to specified uses: hiring of additional ERR's, saturation telephone solicitation, directed mailings, or whatever strategy was to be tested. (Or, there could be simple budget increases, with the use of the additional money left to the discretion of the local office manager.) Properly structured, this experiment could show what can be bought -- in terms of the outcome measures -- with an identified level of extra funding.

There are severe limits, though, to the use of this kind of information and analysis. It is most useful at the extremes: that is, it can show that an approach is clearly not "worth it," or that one is a real bargain. The in-between ground is hazy, particularly when it happens -- as it will -- that "cheaper" approaches are shown to have a favorable impact on some outcome measures, but not on others.

Even more troublesome is the comparison of costs between local offices. Differences in pay scales, space costs, and other fixed expenses from one area of the country to another make comparisons difficult, and can lead to absurdities. For instance, programs operating out of old, run-down space might appear more cost-effective than those in more modern (and more expensive) lodgings. Programs operating with a high proportion of inexperienced (and cheaper) staff would have an edge in cost-effectiveness analysis over those with more seasoned personnel. Programs in low cost-of-living areas would appear superior to those in high ones. The resulting recommendation might be to staff all programs with entry-level staff, and house them in the seediest space available.

The attention paid to program costs is not for any such absurd purpose. It is, rather, to shed light on program options as they are shown to be more or less effective, by indicating -- at least roughly -- how much they cost. It is for the employment service to weigh the various trade-offs implied, and decide where resources should be used.

5.7 CONCLUSION

This report has presented two methodologies. They are quite different from one another, but either can be used to assess the effectiveness of the employer services program. As we have previously confessed, our preference is for the "soft," employer survey approach, which can yield findings at least as reliable as those of the experimental model, at far less effort, expense, and program disruption.

We have also confessed some misgivings about whether any national evaluation of the program would be worth its cost, in terms of new insights. The program is not a novelty, and it is not mysterious. Several experimental variations are already being tested. The employer community is being heard -- from the national level down. Nearly all states have some formal system for monitoring employer services, and many use systematic check-backs with a sample of employers to see if provided services were adequate.

Whether a national evaluation would add substantially to all of this is problematic. A better idea, perhaps, would be to include an examination of employer services in a larger study of the labor exchange function of the employment service.

APPENDIX A:

SUMMARY OF SOURCES

1. Abt Associates, Incorporated. *Evaluation of the Comprehensive Model for Local Office Reorganization (COMO) of the U.S. Training and Employment Service*. Final Report, Two Vols. Cambridge, Mass.: 1971.

   This comprehensive study of the Employment Service and its potential for improvement was strongly oriented towards determining how ES could improve its services to both the disadvantaged workers and employers. Evaluation findings from the six COMO model cities and twelve control cities (six with job banks, six without) were the basis for recent ES reorganization, and include the following:

   1) Employer Services. Although COMO Employer Services Units may improve employers' attitudes toward ES, most employers in all the cities surveyed did not think highly of the quality of ES services.

   2) Community Relations. COMO staff was more concerned about community involvement, but better relations did not evolve, perhaps because of staff limitations.

   3) Labor Market Information. In COMO cities there was no increase in labor market information available or increased number of users of this data. Very little LMI was directed toward the disadvantaged worker. One reason indicated may have been the inefficient functioning of units due to unclear division of authority and blurred perception of the differences between LMI and employer services units.

   Abt also addressed the issues of applicant services, component task performance, and the COMO concept itself, which generally called for redirection of effort to improve services to the disadvantaged workers.

2. Abt Associates, Incorporated. *Job Development Supervisory Training*. Cambridge, Mass.: 1974.

   A collection of training manuals developed for Region VII, these materials sought to combine efforts toward employer relations and job development training for supervisors in their appropriate units. Materials prepared were both for the trainers and participants for the sessions run by Abt. The main thrust is towards development of effective communications with employers, in both the creation of jobs for ES applicants and the improvement of relations between ES and employers.

3. Abt Associates, Incorporated. *Job Development Supervisory Training.* Cambridge, Mass.: 1974.

A collection of training manuals developed for Region VII that sought to answer program needs in both employer relations and job development training for supervisors. The materials in this collection were for participants and trainers in the sessions run by Abt. They focus towards the development of effective communications with employers, in both the creation of jobs for ES applicants and the improvement of relations between ES and employers.

4. Abt Associates, Incorporated. *Job Development Training.* Cambridge, Mass.: 1974.

The companion series of training manuals developed for Region VII, the materials in this collection give more depth to the actual operations of employer relations programs. The Participant Handbook on Job Development Training includes a section on analyzing needs of employers as well as a checklist for analyzing an employer's reasons for difficulty in obtaining and/or retaining employees.

The Index of Self-Development Resources for Job Developers is a bibliography of books, periodicals, and reports to help the job developer increase his effectiveness. The seven topics covered are: basis reference materials for job developers, job development studies and information, manpower program information, labor market functioning, labor market information, selling skills, and an appendix of addresses for use in obtaining the various publications.

5. Alchian, Armen A. "Information Costs, Pricing, and Resource Unemployment." *Western Economic Review,* VII (June, 1969), 109-128.

The intent of this paper is to demonstrate that economic theory can be formulated consistently, with each person acting as an individual wealth maximizer, without constraints imposed by competitors and without conventions or taboos about wages or prices. Shortages, surpluses, unemployment, queries, idle resources, and nonprice rationing are connected with price stability. The goal of the analysis is to consider ways of providing information more efficiently and then, given that information, to consider substitute arrangements that would economize on search costs.

6. Auerbach Corporation. *Human Resources Development.* Final Report. Vols. I and II. Philadelphia: 1969.

This study of ES ability to work with the disadvantaged recommended an intensive services package similar to the considerations by Abt. Auerbach found that ES did not recognize service needs of the disadvantaged, and would require massive reorientation to handle the HRD program.

7. Baum, John F., and Ullman, Joseph C. *An Analysis of the Effectiveness of the Mandatory Listing of Job Openings on the Labor Market Role of the Public Employment Service.* Preliminary Draft. Lafayette, Indiana: Krannert Graduate School of Industrial Administration, Purdue University, 1974.

The evidence found during this study indicates that mandatory listings have contributed significantly to the progress of ES offices in eight cities where the program was in effect. The key performance measure is placement, and this increased substantially from 1972 to 1973 in contrast to placements in control cities.

8. Blau, Peter M., and Schoenherr, Richard A. *The Structure of Organizations.* New York: Basic Books, 1971.

This study of organizational structure involved state and local ES agencies. The authors investigated the inter-dependence among elements in the structure of these offices, e.g., the effect of size and complexity on administrative policies, and the effects of automation and surroundings. Their intent was in showing that these structures exhibit certain regularities that can be explored.

9. Cohen, Malcolm S. On *The Feasibility of a Labor Market Information System.* 3 Vols. Ann Arbor: Institute of Labor and Industrial Relations, University of Michigan - Wayne State University, 1974.

This report describes a demonstration project to develop a series of information technologies that would improve delivery of LMI. The prototypes were developed to study any economic implications for similar lage-scale versions.

The Feasibility of a Labor Market Information System Continued
This effort hoped to clarify the needs of state manpower agencies and planners as well as determine computer potentialities for LMI. The three prototype systems were:

1) Systems that improved access to information, e.g., a computerized information retrieval system that could be accessed by non-computer specialists.

2) Systems that improve the quality of existing LMI, e.g., automated computer graphics.

3) Systems that would improve the manpower planning process, e.g., a data base using administrative data.

10. Dodge, H. Ripr. *Special Report: Employer Relations Program - Activities and Accomplishments, FY 1972.* Washington, D.C.: Manpower Administration, 1973.

A personalized investigation of the employer relations program, this study found that ERR activities - job openings received and placements were on the upswing. There is a noted lack of correlation between openings received, filled, and placements. Negative factors are the drop in capacity of ES local offices to fill employers' job openings. High ratios of short-term placements (in some states) spoiled the otherwise high placement rates.

Heavy increases in ERR personal visits may not indicate effective operations. Some states may not be able to fill openings after promotional efforts have developed them.

Suggested action is to bring practical operations back to ERR activities and placement functions, emphasize the role of employer services as promotional and technical assistance in support of placement. Emphasis should be on regional responsibilities and ERR unit's role in training regional staff. Training materials should be developed and distributed and ERR seminars convened. Also considered was a fast ERR and placement reporting system.

The study called for a reinstatement of the placement process as a kingpin of the national ES system.

11. Greenleigh Associates, Incorporated. *Report on Employer Services Improvement Project in Illinois and Pennsylvania.* New York: 1973.

This report covers the concept of having employers and ES staff join forces in an effort to resolve common labor market problems. Employer services units would act as liason between two groups. Two states were chosen to be sampled because of their sharp decline in ES job listings, a varied industrial and employer base, and different geographical areas represented.

Problems identified by employers were: ineffective man-job matching by ES, belief that the ES system rewards quantity more than quality, need for an aggressive PR campaign, the combination of ES and unemployment compensation offices in the same place, and the need for an employer information service.

Success seems to require a staff (especially the manager) that is sympathetic to community and employer needs, a catalytic agent to effect changes, *ad hoc* committee of employers who will voice their problems and suggest solutions, and a task force of ES staff who will be responsible for the program and its formulation.

12. Greenleigh Associates, Incorporated. The *Employer Services Improvement Program: A Year of Action. Report of Implementation in Ten States.* New York: 1974.

Building on its previous work in Illinois and Pennsylvania, Greenleigh evaluated a one-year effort to modernize the public employment services in twenty communities in ten states. Key components of the project followed the recommendations from its research program (as indicated in the previous review).

Qualitative improvements were seen in task force and change agent capabilities, the PR function, ES-employer-community relations, planned and implemented ES improvements, and employer support. In addition, the state ES decided to replicate the project at additional sites.

Quantitatively, attitudes of applicants, ES personnel and employers improved as did employers' use of ES services.

13. Institute for Re[illegible]
[illegible]

This survey [illegible]
for the U[illegible]
showed that [illegible]
[illegible]

14 [illegible]wa employment [illegible]
[illegible]

This handbook [illegible]
office and local [illegible]
program. [illegible]
employer service[illegible]
personnel in[illegible]
placements needed [illegible]
wh[illegible]

15. [illegible] Lawyers Committee
National Urb[illegible]
[illegible]
1971.

In this report, [illegible]
failure of the 19[illegible]
the less competitive
was also criticized for [illegible]
placements. Implied was [illegible]
manpower agen[illegible] as the [illegible]

The two groups recommended [illegible]
that its functions [illegible]
agency handle[illegible]
and then let them [illegible]
wanted the ES aboard for [illegible]
job listings for the [illegible]
that part of the [illegible]
in the private [illegible]
listing of [illegible]
[illegible]

[illegible], Margaret [illegible] by Gaylen L. Gordon and [illegible], [illegible] Institute of Industrial Relation, University of California at Berkeley, California, 1970.

This report concerns the impact of governmental programs and policies (during the War on Poverty period) on placement and counseling for school students. The survey sought to determine the relationship between employment agencies and schools, especially in terms of LMI.

The outcome of this study was an explicit need for better LMI to help educational institutions guide their students in the vocational area. This result prompted a further study to explore needs for the LMI, sources of the data, and costs of permitting these gaps to continue. This second survey was Thal-Larsen's Requirements and Design of a Labor Market Information System for a Larger Metropolitan Area.

[illegible], Margaret (with Lauer, Stephen and Mayall, Donald). [illegible] Final Report. Berkeley: Department of Industrial Engineering and Operations Research, University of California, 1972.

This project was developed to meet the needs of educators and vocational counselors. The findings state that the LMI system must be viewed as an entity in itself.

1) LMI must be presented as part of a structure and must be detailed.

2) Information, not data, is desired by intermediaries, who should retrieve it through their own staff's efforts.

3) Present LMI prevents adequate planning in many areas.

4) The LMI components identified suggest the basis for a better LMI system.

5) An evaluation mechanism is proposed to structure user needs and provide better system performance.

[illegible] should play a critical role in developing LMI [illegible] multiple area needs.

28. Ultrasystems, Incorporated. *An Evaluation of Results and Effectiveness of Job Banks.* Final Report. 2 Vols. Newport Beach, California: 1972

This study of job bank operations concluded that the job bank was not meeting its design objectives. Although the job bank did disseminate information more widely than previously, and users were responding positively to self-service installations, the system nevertheless had an adverse effect on employer ES relations.

29. Ultrasystems, Incorporated. *Labor Market Information Needs.* Draft. Newport Beach, California: 1974.

This job-seeker survey found that labor market information is used mainly as a source for learning about jobs and companies, although it is also valued for data on community facilities, training programs, and transportation.

30. U.S. Department of Labor. Manpower Administration. *Developing Your Manpower.* Washington, D.C.: U.S. Government Printing Office, 1970.

This report covers the development of a hiring program, employee orientation, and personnel policies. One feature is ES employer services, e.g., job analysis and restructuring, worker recruitment, interviewing, and testing, referral, use of workers in area training projects, turnover, and labor market information.

An appendix presents manpower forms and checklists that would help employers deal with employee problems such as absenteeism.

31. U.S. Department of Labor. Manpower Administration. *A Handbook for Analyzing Jobs*. Washington, D.C.: U.S. Government Printing Office, 1972.

This handbook provides a procedure for obtaining and recording job analysis data. The premise is that data about job and worker requirements will help programs concerned with manpower potential.

Employers will use job analysis data for recruitment and placement, better utilization of workers, job restructuring, vocational counseling, training, performance evaluation, and plant safety.

32. U.S. Department of Labor. Manpower Administration. *A Handbook for Job Restructuring*. Washington, D.C.: U.S. Government Printing Office, 1970.

This handbook defines job restructuring and explains its usefulness. The methodology involves the analysis of each job in terms of: tasks of the worker, function of the workers in relation to data, people, things; minimum education, estimation of aptitude for satisfactory job performance, and other worker traits such as physical demands, temperament, and interests.

Also included are a variety of forms used to accomplish this task.

33. U.S. Department of Labor. Manpower Administration. *Manpower and Operations Research Studies of the U.S. Employment Service and State Employment Services, 1958-1967: A Selected Bibliography*. Washington, D.C.: U.S. Government Printing Office, 1968.

This listing of research studies includes employment office services to workers and employers. Other topics are job opportunity research, improvement of employment and related establishment data, improvement of unemployed labor force data, special manpower and job market studies, occupational analysis research, test development research, counseling research, studies of agricultural workers, and HRD studies.

34. U.S. Department of Labor. Manpower Administration. *Suggestions for Control of Turnover and Absenteeism.* Washington, D.C.: U.S. Government Printing Office, 1972.

This document is essentially a technical manual to help employers deal with turnover/absenteeism.

35. U.S. Department of Labor. Manpower Administration. Division of Program Evaluation Studies. Special Evaluation Group. *Evaluation Study of the Employer Technical Services Activity.* Washington, D.C.: 1973.

This in-house report had, as one objective, the development of measures of employer technical services programs' effectiveness. However, their finding on this aspect is that the activity did not lend itself to clear measurement of outcomes.

The evaluation provides an insightful summarization of the history and scope of technical service activity. Though issues of productivity are essential for long-range economic planning, and the report states that technical services could be an important delivery system for increased productiveness in industry, the relegation of ETS to lower order priorities is still in effect in the country. On the state level, there has been in many cases, a deliberate policy to reflect public relations activity away from this component of ES services.

Recommendations call for additional R & D projects in technical services activity, to create, test, and refine tools, improve staff functions, and create better communications with other state agencies. They also call for updating Industrial Services Handbook, and giving more emphasis on training representatives in the tools for more effective service. One important recommendation is to evaluate the positive and negative aspects of employer technical service activities still in operation in the country.

36. U.S. Department of Labor. Manpower Administration. Division of Program Evaluation Studies. Special Evaluations Groups. *Evaluation Study of the Mandatory Listing Activity.* Washington, D.C.: 1973.

An in-house study of the immediate problems of implementing Executive Order 11598. Conceived as a short-term study, only five states were selected and sampled according to a narrowed set of criteria. Federal, local and state officials were interviewed, and added to data gathered from interview forms with employers.

The study found great problems with identification of mandatory listing activity; problems with coordinating the volumnous data collected on employers, and other associated problems connected with the start-up of a new program. It dealt minimally with the effect of EO11598 on the broader areas of employer services, and indicated that these activities were mainly grouped around securing the compliance of employers in local labor markets.

37. U.S. Depatment of Labor. Manpower Administration. Office of Manpower Program Evaluation. Division of Special Studies. *Evaluation Study of the Employer Services Improvement Program.* Washington, D.C.: 1975.

This evaluation covers two original pilot programs developed by Greenleigh in 1972 and seven of 20 replication sites launched in 1974 to improve employer services. Local employers and ES staff were to review and monitor ES operations to increase local employer use of ES services.

Though the program held promise, it under-utilized its *ad hoc* employers committee, had inadequate interfacing of ESIP elements, and found difficult transferring ESIP responsibility to local management. Also needed were more specific local project goals and more spillovers effect to other employers. The study found no major employer constituency and the need for a consultant.

38. U.S. Department of Labor. Manpower Administration. United States Employment Service. *ES Manual Section 7000: Employer Services, Employer Relations, and Industrial Services.*

The employer relations section of this manual concerns local office relationships with employers as they are carried out through personal visits, telephone calls, and direct mail. Topics covered include program objectives, policies significant in promotional relationships, preliminary program planning including selection of employers for regular promotional efforts and assignment of responsibility for employer accounts, plus the methods and records for implementing the program plan as well as the "plans of service" for the individual employers.

The industrial services section describes those services that apply developed tools, techniques, and methods to help employers, unions, and other organizations solve their manpower problems involved with selecting workers, making the most use of employees' skills and potentialities, stabilizing employment by reducing turnover and absenteeism, improving personnel management practices, and identifying training needs to meet an employer's requirement for workers in certain occupational areas.

39. U.S. Department of Labor. Manpower Administration. United States Employment Service. *Industrial Services Handbook.* Washington, D.C.: U.S. Government Printing Office, 1970 (reprint).

This handbook gives a detailed breakdown of the role of an industrial services representative in helping employers resolve manpower problems.

Appendix Two covers the evaluation of industrial services activities.

40. U.S. Department of Labor. Manpower Administration. United States Employment Service. *Mandatory Listing Handbook.* Draft. Washington, D.C.: 1975.

This handbook deals with historical basis of mandatory listings and then gives practical office details for getting the program into operation and meeting the listing requirements. It offers ways to incorporate this policy into employer services programs, details methods for employer contacts, and suggests attention to federal contract awards to gain lists of businesses under the domain of the program. The book is the source for analyzing the way local offices have used this order. Forms required for listing are also indicated.

41. U.S. Department of Labor. Manpower Administration. United States Training and Employment Service. *USTES Employer Services Training Resource: Master Guide and ESTR Series 100-400 and 900. Guides and Training Materials.* Washington, D.C.: U.S. Government Printing Office, 1974.

Section 200 of this material covers employer services. Included are specific employer services, employer technical service responsibilities, labor market information, testing, turnover and absenteeism, supportive services to employers, job and skill information, case records - information bank, job analysis, and job bank.

42. U.S. Department of Labor. Office of the Associate Assistant Secretary for Program Review and Audit. *Management Audit Survey Handbook for Supervisors.* Washington, D.C.: U.S. Government Printing Office, 1974.

This handbook covers a system for analyzing employee perceptions of organizational operations as a way to improve use of the Dept. of Labor's resources and thus gain better employee morale and more effective operation.

The booklet gives the 100-question form to be used, along with scoring explanations and interpretations. Among the areas covered in the questionnaire are fairness of management, climate for innovation, work satisfaction, performance feedback, satisfaction with pay, morale, and workload balance.

43. Virginia Employment Commission.

This kit discusses the VEC commissioner's plans for an advertising campaign and a program to contact each area's ten largest employers. Brochures to help employers and job applicants will become part of a kit distributed to each large employer and others who request it. This effort is aimed to improve employer relations, make the community aware of the ES, and increase placements.

Materials from the Arizona Employment Service are contained in this folder, and will also become part of an employer relations kit for that state. Among the pamphlets included are: The Jobs Optional Program, Sample Outline of Supervisory Training, Checklist on Exit Interviews, and an Outline for Employer Handbook.