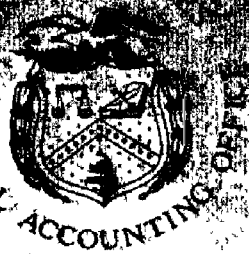
BY THE STAFF OF THE

General Accounting Office

Federal Economic Development Grant Programs: An Econometric Analysis of Projects, 1974-78

This study presents a technical discussion of GAO's attempt to estimate the effect on employment of incremental [illegible] economic development [illegible] an econometric model. [illegible] those evidenced [illegible] additional jobs, particularly [illegible] eligible works projects [illegible] alternative types of [illegible] (3) events that [illegible] areas. In addition, [illegible] provided [illegible]



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PREFACE

Concern over how many new jobs have been created by various federal programs has spawned numerous studies using a variety of methodologies. However, these various methodologies have produced noncomparable results because they generally involved using different standards, definitions, and criteria. The Chairman, Subcommittee on Economic Development, House Committee on Public Works and Transportation, asked GAO to develop a uniform and consistent method to estimate employment effects across various types of economic development programs. This is a technical supplement to a more concise nontechnical report entitled, Estimated Employment Effects of Federal Economic Development Programs, (GAO/OCE-84-4). In this study GAO presents and documents the econometric model used to estimate the employment effects of an incremental change in economic development assistance.

This study was prepared by Charles Vehorn, John Clapp, Deborah Bickford, and James Bell under the direction of Frank Frazier and Craig Simmons. Many other people both within and outside GAO contributed to this project with comments, suggestions, and support throughout its various phases.

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Contents

TABLES

FIGURES

ABBREVIATIONS

ARC	Appalachian Regional Council
CBO	Congressional Budget Office
CSA	Community Service Administration
DAs	development areas

DOE	Department of Energy
EDA	Economic Development Administration
ERS	Economic Research Service
FmHA	Farmers Home Administration
GAO	General Accounting Office
HUD	Department of Housing and Urban Development
IRBs	industrial revenue bonds
NBER	National Bureau of Economic Research
NRIES	National Regional Impact Evaluation System
OMB	Office of Management and Budget
OMBE	Office of Minority Business Enterprise
REA	Rural Electrification Administration
RPCs	Regional Planning Commissions
SBA	Small Business Administration

CHAPTER 1

INTRODUCTION

Evaluations of federal economic development programs have produced varying results that cannot be easily compared across programs. Yet, federal policymakers would find comparative analyses useful in their deliberations over how to allocate scarce resources for economic development. This lack of comparability led the Chairman, Subcommittee on Economic Development, House Committee on Public Works and Transportation, to ask that we develop a uniform methodology to evaluate the job-creating effects of various federal economic development programs. Our effort in responding to this request resulted in the development of a small-scale econometric model.

Preliminary estimates from this model were previously presented in testimony[1] and a concise nontechnical report[2] was issued later. However, certain details, many of a technical nature, were either not presented or not fully elaborated upon in those documents. Thus, the purpose of this present study is to provide the technical reader with a more comprehensive discussion of the analysis.

This study differs from the nontechnical report in three ways. First, it provides a more detailed discussion of previous attempts by evaluation researchers to isolate the effects of federal government programs on local or regional economic activity (see ch. 2). Researchers have employed two basic methods--the case study approach and econometric modeling. While both approaches have limitations, each can provide useful although different types of information for program evaluation. Information obtained from the case study approach is more project specific in nature than information obtained from an econometric modeling effort. Because we were asked to compare programs, not projects, we used the econometric modeling approach. This approach has the advantage of producing estimates that can be generalized and used for comparative purposes.

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[1]Statement of Harry S. Havens, Assistant Comptroller General for Program Evaluation, U.S. General Accounting Office, before the Subcommittee on Economic Development, House Committee on Public Works and Transportation, on the Effectiveness of Economic Development Programs, Feb. 15, 1983.

[2]Estimated Employment Effects of Federal Economic Development Programs, (GAO/OCE-84-4).

Second, this study provides a more comprehensive rationale of the conceptual model and a detailed discussion of the estimation techniques used (see ch. 3). The model is based on previous regional economics research both in Great Britain and the United States.[3] However, the model is smaller than large-scale multiregional models because it focuses on only one aspect of economic activity, the level of employment. An important contribution of the model is its attempt to distinguish the effects from different economic development policy instruments. The program data include disbursements as well as obligations for grants, loans, and loan guarantees. Furthermore, these program data are categorized by purpose, e.g., public works, community development, and business development. Another contribution is the manner in which distributed lags are used to capture the effects of policy variables over time.

Third, the study provides explicit technical details associated with the estimated results (see ch. 4). Regression results from the complete model are presented with a discussion of the various coefficient estimates. Several statistical tests and sensitivity tests, similar to those suggested by Leamer (1983), are presented to show the robustness of the estimates. Also, we contrast our results with previous studies.

This study represents a departure from the more traditional case study approach normally used within the government to evaluate economic development programs. The results presented here differ from those of case studies because our estimates illustrate what might have happened to employment with a slight increment in program funding (ceteris paribus), not how many total jobs may have been created by these programs. Also, the estimates should not be used to reach conclusions about the comprehensive effectiveness of these programs. Many of the programs have several objectives other than new job creation, for example, creating additional entrepreneurial opportunities or upgrading social overhead capital to improve the quality of community life. The model, however, only attempts to evaluate these programs on the basis of the job-creation objective. Finally, the approach we used was not designed to provide employment estimates of what would have happened if these programs had not existed. From a societal viewpoint, had these programs not existed, the resources devoted to them would have been freed for other public or private sector uses, which presumably would have had some effect on employment.

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[3]See Ashcroft and Taylor (1977) and Martin and Graham (1980). (The bibliography contains the complete reference.)

CHAPTER 2

PREVIOUS POLICY EVALUATION EFFORTS

Previous attempts to evaluate public economic development policy have relied on two basic approaches. Each approach addresses different policy concerns. One approach, the case study (survey research) method focuses on a small number of selected projects, where data are gathered through questionnaires, interviews, or applicant reports on file. This approach has the advantage of documenting the implementation process and detailing for policymakers the various problems and successes that each project experienced. However, unless the projects were selected from a valid random sample, this project-specific information is not reliable in making generalizations about the effectiveness of the whole program in meeting stated objectives.

Generalizations are possible with the other approach -- econometric modeling. Using data on all projects undertaken and various economic factors, an econometric model can estimate the effects of a development program relative to other government programs and to other economic forces. Here the policymaker receives statistically valid information on program effectiveness, but details of specific projects never surface. This chapter compares and contrasts the two approaches through a review of previous research. While this research is very extensive, we intend to concentrate on only one aspect of program evaluation: How can either approach be used to estimate the effect of economic development programs on new job creation? Both approaches have limitations, but as we will show, the econometric modeling approach is better suited for addressing the broader question of the program's relative effectiveness in creating new jobs.

THE CASE STUDY APPROACH

In a review of Economic Development Administration (EDA) experience with case studies, Shaikh and Salinas (1978) pointed out that most of these studies are designed to determine the number of new employees associated with a particular project funded by the federal government. With some exception, each new position at the project is counted as a "created" job, even if the individual in that position was previously employed elsewhere. The ratio of project expenditures to jobs created results in a figure that is extremely important to the Congress and agency officials--cost per job. The lower the cost per job estimate, the more effective the project. Some officials may even attempt to make an unwarranted generalization about the effectiveness of the program. Such an attempt, however, ignores both the difficulties in measuring new jobs created and the difficulties in generalizing from individual project results to estimates of general program effectiveness.

Kinds of jobs created

Counting new jobs associated with a project may appear simple until one considers all of the problems involved in defining a new job. Jobs can be permanent or temporary, full-time or part-time, actual or expected, private or public. Since no uniform standards exist, each case study attempts to make reasonable decisions on what jobs will be counted and how these will be weighted, e.g., two part-time jobs for 20 hours per week may be counted as one full-time job. But variations in the approach adopted by different studies make it almost impossible to compare cost per job estimates from different case studies.

One reasonable decision made by many case study researchers is that the federal government should be credited with all the jobs created or saved provided that the jobs would not have been available without federal funds. Two tests have been used by EDA:

1. The substitution test: Would private business firms, state governments, or local governments have funded the projects without federal funding?

2. The relocation test: Would the project have been located in the local labor market area without federal funding?

If the answer to each question is "no," then federal outlays are credited with all the full-time permanent jobs associated with the project.

If the answer to the first question is "yes," then the federal outlays are deemed to have substituted for other investments that would have been made without the federal program. If the answer to the second question is "yes," then the federal funding has merely shifted the project within the local area. In both cases, the jobs associated with the investment are not counted as net jobs created or saved. Thus, the case study approach attempts to distinguish between gross jobs associated with the federal outlays and the net jobs that have been created or saved in the local area as a result of the federal program.

One major problem with these tests, sometimes referred to as "but for" tests, is that they cannot adequately deal with joint funding from several sources.[1] An example of this problem is provided by the experience of the Economic Resource Corporation of Los Angeles, an industrial park started in 1968 with a $3.8 million grant and loan from EDA, an equal amount from the Office of Economic Opportunity, and a $2 million commitment from the

[1] "But for" tests are also used by program administrators in making funding decisions. For example, applicants for Urban Development Action Grants must state "but for" the federal funds the project would not be undertaken.

Lockheed Corporation. By 1978, a field study performed by EDA determined that 30 businesses providing 750 jobs and $6.6 million in annual payroll occupied the park. The case study method would not fully account for Lockheed's contribution, along with other possible causal factors, and would credit the federal government with creating all of the jobs and income.

A second problem with "but for" questions stems from what Bartels, Nicol, and van Duijn (1982) call response bias:

> "In the case of policy questions, respondents might say that policy was important if this would influence the future availability of incentives.... The problem of ex post rationalizations permeates survey research. Thus, a different rationale may be attributed to decisions which conceals the real motives in the decision process (pp. 13-14)."

In addition to the distinction between gross and net jobs, another methodological distinction is made between direct, indirect, and induced jobs. Direct jobs are defined as those at the site of the project funded by the federal program, whereas indirect jobs are stimulated by the project but located elsewhere, such as increased employment in the industries supplying materials and services for the project. Induced jobs are stimulated by the expenditure of the income generated from direct and indirect employment. Survey research must rely on multipliers to estimate indirect and induced jobs. These multipliers are derived from estimates of purchases from suppliers (backward linkages) and sales to customers (forward linkages). For example, it might be determined that 50 jobs are generated by these forward and backward linkages for every 100 direct jobs. However, any bias in direct job estimates is expanded by making estimates of indirect and induced jobs, regardless of the accuracy of the multiplier.

Limitations of the case study approach

In summary, the case study approach has several inherent limitations. In a review of this method, Bartels, Nicol, and van Duijn (1982) pointed out that weaknesses include (1) unrepresentative sample selection, (2) difficulty in generalizing the results, (3) inability to sort out the effects of numerous causal factors, and (4) response bias.

THE ECONOMETRIC MODELING APPROACH

Several econometric models have been used to evaluate the effects of federal policies on regional economic activity. These models range on a continuum from large scale multiequation models to smaller single equation models. While these models are designed to analyze various economic sectors, our main concern here is with the employment sector.

The labor market in econometric models

Previous regional (and multiregional) econometric models have included employment sectors, with both supply and demand equations. These models have had more success in estimating labor demand than labor supply. Treyz, Friedlaender, and Stevens (1980) expressed labor demand in terms of regional input prices to estimate regional purchase coefficients. Milne, Glickman, and Adams (1980) derived labor requirements as a function of output and real wages. Their employment sector was part of a large-scale model designed to simulate regional impacts from changes in national growth, higher relative energy prices, or changes in government activity. On the supply side, theoretical specifications stress the role of labor force participation and net migration. Participation should be discouraged by high and persistent levels of local unemployment and encouraged by high relative levels of local wages. Migration is more complex because it may depend on quality of life, housing costs, or public assistance as well as wages, unemployment compensation, and welfare assistance (see Isard and Anselin (1982); and Fromm, et al.(1980)).

One advantage of econometric models is their capability to deal with interdependencies between the offer to work by households and the demand for labor by business firms.[2] The significance of this for policy evaluation has been investigated by Treyz and DuGuay (1980). A policy that successfully increases business demand for labor (e.g., a policy that induces firms to relocate to the local area) will increase local wages. This attracts migrants and increases labor force participation, but it also discourages some firms with marginal ability to pay the higher wage. Thus, a slight reduction in demand will offset, to some extent, the long-run effectiveness of the policy.

Econometric models are also capable of capturing interdependencies among the regions. An increase in employment and income in a given state will increase the demand for output in surrounding states because interregional trade will be stimulated. Distance deflated income (i.e., a gravity potential variable) has been used as a proxy for the extent of the market served by the given state. For example, the National Regional Impact Evaluation System (NRIES) includes distance deflated variables to measure the "expected trade flows between regions. It is an alternative to the modeling of actual trade flows data which are presently neither comprehensive nor up-to-date." (Department of Commerce, NRIES, 1980, p. 30.)

Evaluation of policy impacts

Econometric models have been used to evaluate the effect of federal policies on regional economic activity in two ways. First, policy activities are included as separate explanatory

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[2]Input-output models were excluded from our analysis in part because they assume a perfectly elastic supply of resources.

variables which explicitly estimate policy impact parameters. Here the relative effectiveness between policies can be compared. Second, the model structure is shifted by changing parameters or shifting variables so as to simulate the possible effects of government policies. This approach is used mainly in large-scale models.

To illustrate these two approaches, consider the following version of the labor sector of the economy:

$$(1) \quad e_i = f(F_{gt}, \mathbf{Z}_t)$$

where

e_1 = percent change in employment in industry 1.

F_{gt} = a measure of the g^{th} government policy designed to influence economic development (e.g., dollar grants for development) at time t.

$\mathbf{Z}_t$ = a vector of variables representing influences other than federal policy.

The policy parameter approach, used mainly in single-equation models, includes F_{gt} explicitly. The estimated coefficients on F_{gt} measure the associations between cross-sectional (spatial) or temporal variations in F_{gt} and the corresponding variations in e_1. These coefficients can be interpreted as the marginal effect of a change in F_{gt} on e_1. They are policy parameters because they represent direct estimates of the relative employment effects of reallocating money among programs and across regions. The approach that shifts model structure, used primarily with large-scale models, may omit the policy variable or measure policy at a less disaggregated level than single equation models. Policy impact is ascertained by shifting one of the **Z** variables, or a parameter of the **Z** variable, and tracing out effects of the assumed policy change. This approach is particularly useful in simulating the broad economic effects of various policy options.

One example of the policy parameter approach is the Ashcroft and Taylor (1977) single-equation model designed to explain the number of firms moving into areas of England designated as development areas (DAs). Two types of variables were included--those designed to capture the effects of government policy (F_g) and those designed to measure other economic influences on firm movement **(Z)**. Thus, their model was in the form of equation (1), where the number of firms moving to DAs replaces e_i.

Regional policy variables in the Ashcroft and Taylor model included (1) the number of permits refused (a method of limiting growth in the developed areas of the Southeast and West Midlands), (2) an index of the present value of incentives for investment in the DAs, and (3) an index of the real value of

labor subsidies for employees in the DAs. In most cases, they obtained significant positive coefficients on the three policy variables, with t-statistics ranging from +1.0 to +6.0, depending on the variables included in the regression.

Variables used to capture the effects of firms moving due to reasons other than regional policy include (1) the relative attractiveness of a region, measured by the relative level of unemployment, (2) lagged excess capacity, (3) lagged investment expenditures, and (4) the annual change in UK manufacturing output. The first two variables were employed to reflect aggregate demand pressure, the last two were included to capture investment demand, since industrial movement may be considered "investment on the move."

In another example of the policy parameter approach Martin and Graham (1980) used a model in the form of equation (1) to determine the effect of EDA dollar outlays on the growth rate of local (county-level) personal income. Their Z variables were designed to capture economic base and shift-share influences on income growth. Their policy variables measured the magnitude, type (public works vs. other), and timing of EDA outlays. Their t-statistics were +2.37 (all aid) and +4.47 (public works). The timing of aid was found to have a significant effect, but aid had no significant effect in the years after aid ceased.

An example of the model structure being shifted is the work of Treyz, Friedlaender, and Stevens (1980) who used an equation in the form of (1) but without the F variables to simulate the effects of a change in tax policy on employment. They first estimated employment using a multiequation regional model. Next, they adjusted factor cost parameters to reflect a change in tax policy, then used the model's estimated equations to simulate the effects of tax policy changes on employment. But some government policies cannot be readily translated into shifts in model parameters or variables. For example NRIES has been used to evaluate the direct and indirect effects of a federal policy that is successful in causing new mines to be opened. Under this approach, a key assumption is required:

> In this scenario it is assumed that because of Department of Energy encouragement, six new mines, each with 10-million-ton capacities, are opened in Montana: One in 1978, two in 1979, and one each in 1980, 1981, 1982. (NRIES, 1980, p.8)

This approach illustrates a problem relevant to the evaluation of federal outlays for economic development. To use existing regional or multiregional models, the analyst must shift one or more constant terms in the model equations. Then the model is used to evaluate the indirect and induced effects of the assumed

shift. The problem is that the analysis assumes part of the conclusion: If the direct program effects were known, then a large part of program evaluation would have been accomplished.

Limitations of econometric models

As with the case study approach that had both measurement limitations and inherent limitations, econometric models are also limited by measurement difficulties and problems inherent to the approach. One measurement difficulty is the poor specification of policy variables. Case study researchers can gather important project details such as the timing of funds, delivery mechanism, intervention of other levels of government, and the duration of the project. Econometric models are forced to use more aggregate data, such as the level of funds received by a locality, because project-specific data usually are not collected on a comprehensive and uniform basis.

In their review article, Bartels, Nicol, and van Duijn (1982) noted, among other weaknesses, three important limitations inherent in econometric models. First, these models must make the implicit assumption that all explanatory variables are independent from each other. Second, these models require the implicit assumption of constant coefficients, either over time and/or across regions. Both assumptions may be difficult to defend, and the results of the model become less reliable as the assumptions become more unrealistic. Thus, it is important to present sensitivity tests, along with the estimates, so that the reader may decide if the results are robust enough. Third, econometric models may be misspecified by omitting an important variable or including an extraneous variable. All econometric models are subject to this challenge, so it is important to present the rationale behind the choice of explanatory variables. Even then, one never knows for sure that all relevant factors have been included.

Finally, a weakness of both survey research and econometric models is an inability to evaluate the extent of fiscal substitution, the reduction in resources available for private and other public expenditures that occurs because any federal outlay must be funded through tax revenues or debt financing. The private or other public uses of the money may have created jobs or increased income if they had stimulated the use of slack resources. Thus, any federal expenditure may substitute, through the fiscal process, for other job-creating activity.

Because a counterfactual situation is difficult to analyze, most studies of program effectiveness have taken the program as given. The issue the econometric model addresses is not whether to eliminate the federal expenditure; rather, it is how to allocate the money among programs and across regions. As mentioned, the model is designed to evaluate the marginal impact of public policy. The analysis of what Bartels, Nicol, and van Duijn (1982) call the "policy off" situation, i.e., the elimination of

an existing federal program, is beyond the scope of econometric models that take the form of equation (1). Such an analysis would require the evaluation of the economic development implications of hypothetical alternative uses of resources. This type of counterfactual evaluation may be beyond the scope of any data analysis, econometric or otherwise.

THE NEED FOR FURTHER RESEARCH

Bolton (1980a and 1980b) has identified a need for further research on the "policy handles" available in regional econometric models. Gaps in the current state of the art include evaluating program differences and lag effects. While these gaps are partially due to data limitations, models that overlook these dimensions may not fully capture policy effects.

An example of a model that was not designed to capture program differences is NRIES. Because of the NRIES model structure, all federal programs must influence the economy through intergovernmental transfers. But some tranfers may be used for investment while others are used for consumption. Differences in types of transfers may have a differential effect on recipients. For example, the Comprehensive Employment and Training Act operates by training workers who take their skills to private industry, whereas a program like General Revenue Sharing provides unrestricted assistance for consumption or investment purposes. These differences cannot be evaluated with the existing NRIES structure.

This methodology would be particularly inappropriate for the economic development programs we sought to evaluate. Many of these programs work through grants, direct loans, and loan guarantees to private businesses. It is plausible that the effect on economic activity depends on the form of assistance and on how that assistance is spent. The direct effect on private business activity cannot be considered as part of state and local government expenditures and output.

A considerable time may elapse between government program outlays and indirect or induced effects on economic activity. It is very difficult to deal with these lags in the context of a large econometric model because it is difficult to experiment with alternative lag structures. Furthermore, regional econometric models would require a very long time series on program outlays, perhaps as far back as 1955 or 1960. But economic development policy evolved slowly, and data are not adequate to capture fully these early policy efforts.

Some single-equation models have attempted to deal with lagged effects. Ashcroft and Taylor (1977) introduced a simple 1-year lag in their policy variables, whereas the actual lag structure could be more complex. Our model departs from previous ones by experimenting with alternative lag structures as a way of improving estimates of indirect and induced effects.

With the introduction of a distributed lag function, equation (1) becomes

$$(2) \qquad e_1 = f\left(\sum a_k F_{gt-k}, Z_t\right)$$

where a_k is the weight on each of the F variables. The summation is taken from the current period (k=0) to the maximum lag period (k=N). The timing of federal expenditures with respect to employment growth is indicated by the parameters on the F_{gt-k} variables. This pattern of lag weights gives concrete evidence on the year-to-year impact on employment. We propose to use a distributed lag function to estimate the person-years of employment generated by economic development outlays. Thus, we dispense with the survey research distinction between permanent and temporary jobs.

SUMMARY

This chapter provides a critical review of alternative methods for evaluating the effects of public policy on economic activity. The two methods considered here--case studies and econometric models--attempt to estimate the relationship between policy and economic activity. In reviewing these methods, we touched on several important issues: the types of jobs counted (temporary, permanent, direct, indirect, and induced), how to evaluate the dynamics of program effects, and how to apply a consistent methodology across programs. We found that both methods were not designed to evaluate the counterfactual or policy-off situation because both take existence of the program as given.

While both methodologies have limitations, we found that econometric modeling was a better approach for analyzing effectiveness across programs. In the next chapter, we develop a model that can provide a consistent framework for evaluating alternative policy instruments. Model assumptions and limitations apply in the same way to all programs considered, so that the estimated impact parameters can be compared across programs. The econometric results are presented in chapter 4.

CHAPTER 3

THE MODEL

Both national and local factors interact to create new jobs. Nationally, the business cycle is one important determinant of firms' demand for workers and workers' job search behavior. But trends in national economic activity cannot fully explain new job creation in a particular local area because area specific factors also influence employment trends. Indeed, variation in local factors may be very important in explaining variation in job growth between areas. In particular, one important local factor may be the amount of federal economic development assistance received.

To estimate the separate effects of these various factors influencing employment growth, we constructed a labor market model which attempts to capture the influence of both national and local factors. The next section contains more specific details on the various equations used to construct the model. Following that, we discuss the rationale for our selection of variables, including measures of employment growth, business cycle conditions, targeting, and federal assistance. Next, we describe the various statistical estimating procedures employed. Finally, we discuss the model's limitations.

SUPPLY AND DEMAND EQUATIONS

The model is based on a simple supply and demand framework. It examines employment growth in I industries, $i = 1,2,\ldots,I$; J states, $j = 1,2,\ldots,J$; and T periods of time, $t = 1,2,\ldots,T$. Growth in the supply of labor, e^s_{ijt}, is assumed to be a function of the percentage change in relative wages, w_{ijt}; industry employment growth nationally, n_{it}; and a vector of other socioeconomic variables, $\mathbf{z}^s_{jt}$, related to labor supply. Growth in demand for labor, e^d_{ijt}, is assumed to be a function of the percentage change in relative wages, w_{ijt}; industry employment growth nationally, n_{it}; federal assistance, F_{gjt}, (over G program types, $g = 1,2,\ldots,G$, including both economic development assistance and other expenditures); and a vector of socioeconomic variables, $\mathbf{z}^d_{jt}$, related to labor demand. The economy is assumed to operate so that markets are cleared, i.e., supply equals demand.

Dropping, for simplicity, the state and time period subscripts, the equations can be specified in functional form as follows:

(3) Supply equation $\quad e^s_i = E^s(n_i, w_i, \mathbf{z}^s)$

(4) Demand equation $\quad e^d_i = E^d(n_i, w_i, F_g, \mathbf{z}^d)$

(5) Equilibrium condition

$$E^S(n_1, w_1, \mathbf{Z^s}) = E^d(n_1, w_1, F_g, \mathbf{Z^d})$$

From these equations we can derive the reduced form equations[1] for growth in relative wages and employment:

(6) $$w_1 = f(n_1, F_g, \mathbf{Z^s}, \mathbf{Z^d})$$

(7) $$e_1 = f(n_1, F_g, \mathbf{Z^s}, \mathbf{Z^d}).$$

VARIABLE SELECTION

Business cycle conditions

As one independent variable, a measure of the business cycle was used to account for employment growth that would have occurred because of improvement in general U.S. economic conditions. The rationale for this variable draws from shift-share analysis, i.e., the national variable, which is expected to have a significant positive sign, is entered to explain growth at the state level. This approach has the advantage of controlling for business cycle phenomena, while the other variables, such as relative wages and federal economic development assistance, capture variation among states.

Shift-share analysis has proved to be a useful descriptive tool that divides regional (e.g., state-level) employment growth in each industry into two parts. One is growth that would have occurred if local industry employment grew at the same rate as the national employment in the same industry; i.e., the regional proportion (or regional share) component of growth. The second is the difference between actual employment growth and the regional share component; this is the differential shift component.

Some may criticize the use of shift-share analysis as a method for explaining why regional growth differs from national growth, but we have not used shift-share for such a purpose. Instead, we use the shift-share framework to isolate the differential shift component for further analysis. For example, classical location theory, with its emphasis on the principle of comparative advantage, can be applied to the differential shift. The national growth in employment in a given industry controls for the complex influences exerted by the business cycle, whereas comparative advantage (e.g., relative wages) explains differences between national growth and regional growth.

Stevens and Moore (1980) argued that the shift-share framework simplifies the analysis of change in employment. They concluded that "shift-share is unrivaled in its ability to

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[1]The reduced form is useful in estimating long-run effects of federal economic development assistance. For more details see Phillips and Wickens (1978).

provide quick, inexpensive, and useful indications of past regional performance and to identify problems which may deserve the attention of public policy makers (p. 433)."

An assumption behind this application of shift-share is that there is a stable relationship between national growth and sub-national growth. Forthergill and Gudgin (1979) have recently used data from the United Kindgom to test the stability of the shift-share relationship at various levels of industrial aggregation and over several subperiods between 1952 and 1975. They found that "the technique is reasonably robust in that general conclusions are not usually seriously distorted by the potential difficulties in using shift-share (p. 319)."

Economic development assistance

This study attempts to quantify the incremental employment effects of various economic development programs. We expect these programs to have a positive effect on employment growth. But the effect may differ depending on the type of assistance received--a grant, loan, or loan guarantee. These three types of assistance can be distinguished by the amount of the subsidy and the way in which the subsidy operates. Grants for a given dollar amount represent an outright subsidy of the full amount. Direct loans, on the other hand, are a disbursement of federal funds to borrowers under the stipulation that such funds will be repaid--often at below market rates of interest. Since loans are to be repaid by borrowers, the interest rate differential represents the only subsidy. However, in cases of default by the borrower, the subsidy includes the unpaid amount. Finally, loan guarantees represent agreements by the federal government, as a third party, to repay the principal or the interest on a loan, in whole or in part, to a lending institution if a borrower defaults. Since these loans are only guaranteed by the federal government, no direct outlays are made except when a default occurs. Indirectly, there is an interest subsidy because a federal guarantee lowers the risk to lenders, so they are willing to lend at relatively lower rates of interest.

Selecting the particular government programs to be included in this study is complicated by the fact that almost all federal spending has at least an indirect effect on local or regional economies. Even defense programs can help develop a local economy and create jobs, but such programs are not economic development programs per se. We considered various definitions of economic development. For example, EDA defines economic development as follows:

> "Economic development aid is the planned investment of public resources to attract private investment to specific areas and communities in order to create permanent private sector jobs and strengthen local private economies. Economic development is a public investment program whose

return is jobs, incomes and revenues in those areas served. The unique characteristic of economic development programs is that they encourage the growth of private enterprise in a community which ultimately forms the backbone of the economy. To the extent that economic development programs are successful, the Federal subsidy is temporary."[2]

A key criterion in the EDA definition is job creation, so we looked for federal programs that had job creation as a primary objective in their legislation. Not surprisingly, many programs satisfied this criterion, but it was not our intent to include all programs with a legislative objective to create jobs. Thus, we restricted our review to programs with a job-creating objective in the major economic development agencies (see app. III for a listing of these agencies and the programs).

Our set of programs encompasses more programs than are classified in the federal budget as "community and regional development programs." We included all the major programs contained in that category of the federal budget except one--Disaster Relief. The programs we included accounted for 96 percent of "community and regional development" grant outlays.[3] We also included programs that operate from an off-budget status, such as the Department of Agriculture's Community Facilities Loan Program and the Small Business Administration's State and Local Development Company Loan Program.

Program effectiveness is assessed in this model by the estimated number of new jobs associated with a given increase in assistance. Because disbursements in the current year are expected to result in a flow of new jobs over an extended period of time, we constructed various distributed lags to capture the intertemporal effect of federal economic development assistance. The estimated coefficient measures the growth in annual employment relative to this year and previous years of assistance.

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[2]"The Development of a Subnational Economic Development Policy," Economic Development Administration, U.S. Department of Commerce, (Sept. 1977), p. II-4.

[3]Special Analysis Budget of the United States Government, Fiscal Year 1979, Table H-9, pp. 191-197.

Targeting

Our treatment of federal economic development assistance was designed to minimize the problem of least squares bias (Garrison and Kort 1980; Mathur and Stein 1980). The problem--contemporaneous correlation between the disturbance term and federal assistance--could bias the estimated assistance coefficients. This bias could occur if federal assistance is related to omitted variables or if federal policy is endogenous.

Federal economic development assistance can be considered endogeneous to the system if the direction of causation runs from the dependent variable to a measure of current assistance. In this case, we could measure a targeting effect. Federal funds can be targeted on the basis of several criteria, including low incomes, high unemployment, or lagging growth in wages or employment. Although the model was not designed to analyze whether funds were well-targeted, we did include current grant obligations, which represent a promise of aid, as control for a targeting effect. We hypothesized that relatively more aid would be promised to the states with relatively low employment growth. A negative sign would indicate the presence of targeting on the criterion of lagging employment growth. But evidence of targeting on only one criterion is far from conclusive and cannot be used to answer the question of how effective the program has been.

Other variables

The model contains several other variables to isolate the employment effects of federal assistance. For example, movements along the supply and demand schedules are captured by the relative wage variable. The coefficient is expected to be positive in the supply equation and negative in the demand equation. The remaining explanatory variables (the **Z** vector) capture shifts in the supply and demand equations.

Positive shifts in the supply schedule occur when labor force participation increases, or when net inmigration occurs. It is hypothesized that employment opportunities in state and local governments will cause such a positive supply change.

In the supply equation, the expected sign of the coefficient on the variable that measures the degree of urbanization in the state can be derived inductively from previous empirical evidence. During the period fitted by the model, 1974-78, there was some deconcentration of the U.S. population (Heilbrun 1981). This relative movement from large cities to smaller cities and from cities to nonmetropolitan areas appears to reflect changes in individual locational preferences rather than being merely a spillover outside of city boundaries. The deconcentration trend leads us to expect a negative sign on this variable.

Also, we have included the unemployment rate in the supply equation. In a larger model, it may be advisable to treat the unemployment rate as an endogenous variable. But here the focus

is on the effects of economic development assistance programs and the unemployment rate acts as a proxy for conditions in the labor market. The unemployment rate's expected sign is negative because higher unemployment discourages workers. This lowers labor force participation rates and lowers inmigration. Furthermore, it acts as a propulsive effect causing outmigration, reducing labor force participation rates. On the other hand, favorable weather conditions (e.g., sunbelt climate) have attracted net inmigration over the estimation period.

State dummy variables were entered to control for outliers on both the supply and demand side. Alaska, Nevada, and Wyoming were identified as outliers through a cluster analysis. Experimentation showed that the supply and demand equations were sensitive to the inclusion of Alaska. Pipeline development caused Alaska to have a growth of 40 percent in total employment in 1 year; in specific industries, Alaska's growth ranged up to several hundred percent. Therefore, Alaska was eliminated from the second stage supply and demand equations.

Shifts in the demand for labor occur because of business relocation decisions, or decisions to expand and contract business operations at a given location. A special case of business contraction occurs when a firm decides to terminate its operations; similarly, the demand for labor shifts up (expansion) when a new firm emerges or when an existing one expands. The remaining variables capture the effects of relocation decisions.

The relative attractiveness variable is based on the fact that access to growing markets attracts firms: they will relocate to or expand in the growing area. The numerator is growth in labor and proprietors' income in the state. The denominator is growth in each of the other states, distance deflated and summed. Appendix II, the list of data sources gives more detail on this variable. The denominator represents growth in areas that are competing for the expansion or relocation of the firm. Distance deflation is used because relocation costs rise with distance.

Business tax rates at the state level can influence the relocation decisions of firms. At the margin, higher state corporate profits tax rates discourage relocation or expansion. Therefore, we expect a negative sign on the corporate income tax variable.

Business relocation decisions should also be influenced by housing rents relative to the wage rate. Housing rents have a negative influence on relocation and expansion decisions because firms conclude that it will be harder to attract employees. A recent survey of Fortune's 1000 largest corporations indicates that 96 percent offer subsidies to transferred employees who must sell a home, and between 60 and 75 percent offer assistance to purchase a new home. (See Graduate School of Management, 1982, p. 4.) Solow (1973) developed a theory that shows how firms account for local housing conditions. Empirical evidence supports

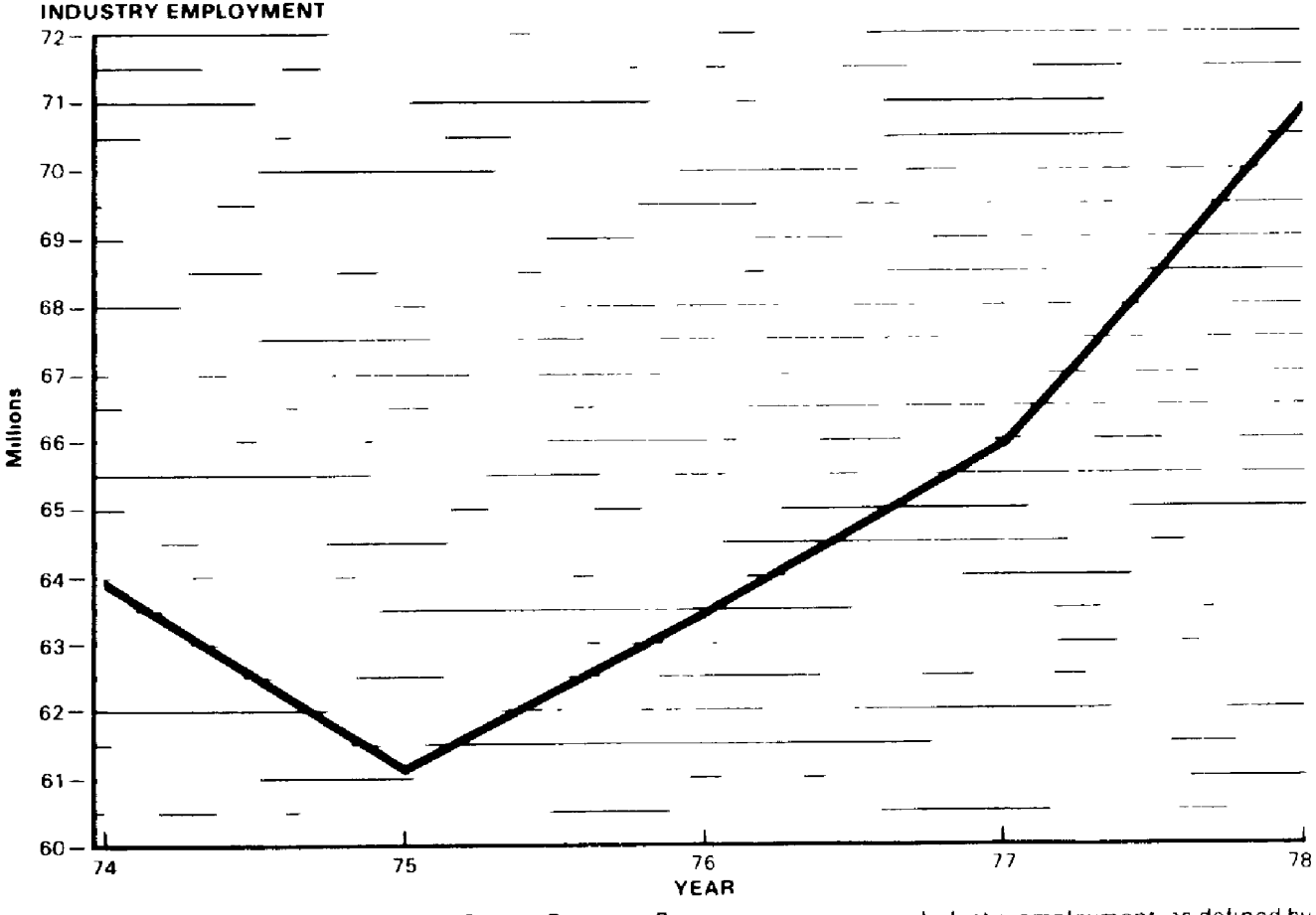
the theory (Clapp, 1980). Although the theory was developed with reference to intrametropolitan location decisions, it would suggest that firms may be influenced by the housing market in making interstate location decisions as well.

Employment change

Since a major objective of this study is to estimate whether federal economic development assistance played a role in the creation of new jobs, the dependent variable was expressed as the growth rate in employment rather than the level of employment. The results, which are discussed in the next chapter, are based on the period between 1974 and 1978. This period includes the trough and recovery from a recession. Overall, employment fell for the first year and then rose rapidly (see figure 1). In 1978, 70.6 million people were employed by all industries, up almost 10 million workers from the 1975 low of 60.9 million. Employment change by industry varied substantially (see table 1). For example, in 1978 manufacturing employment grew 4.4 percent, while employment in personal services grew 10 percent.

Figure 1

NATIONAL EMPLOMENT FOR ALL INDUSTRIES, 1974-78
(IN MILLIONS)



Source U S Bureau of the Census *County Business Patterns* various years Industry employment as defined by *County Business Patterns*, does not include interstate railroad and government employment

Table 1

Growth in Annual Employment, by Industry, 1974-78

Industry	1974-75	1975-76	1976-77	1977-78
	. . . . . percent			
Household, retail, and wholesale trade	-1.3	5.5	2.8	8.2
Manufacturing	-11.3	2.9	3.6	4.4
Contract construction	-16.1	3.7	3.7	15.6
Banking, insurance, and real estate	- 2.3	3.2	3.1	6.3
Personal services	2.3	4.0	4.5	10.0
All others	- 1.0	4.0	3.9	3.6
All industries total	- 4.5	3.9	3.6	7.6

Source: U.S. Bureau of the Census, County Business Patterns, various years.

ESTIMATING PROCEDURES

We estimated short-run employment effects using a two-stage procedure. First, estimates of the coefficients of equation (6) were obtained for each industry, with the relative wage as the dependent variable. From this reduced form equation we calculated estimated wages, $\hat{w}_1$. Second, we substituted $\hat{w}_i$ for w_1 in the structural equations (3) and (4) to obtain supply and demand parameter estimates.

In the second stage we initially used the ordinary least squares procedure. However, the model pools cross-sectional and time-series data, so we also used two other estimation procedures--an autoregressive model suggested by Parks (1967) and a variance components model suggested by Fuller and Battese (1974). These latter procedures are designed to adjust for the more common error structures in the model's residuals. Our model, however, has limited time series information, with no evidence of problems due to serial correlation. The major statistical problem in our model was collinearity, and these standard procedures are not designed to deal with collinearity problems.

The collinearity problem also surfaced in dealing with the distributed lags on federal assistance. Following Maddala (1977), we tried the Almon and the De Leeuw lags. Both produced spurious results due to an extremely high degree of collinearity. The "condition index," as described in Belsley et al. (1980), an index to measure the presence of one or more collinear or near dependencies among the columns of a data matrix X, was well over 100 for many industries.

The collinearity problem

To reduce collinearity, we turned to ridge regression, which differs from the standard regression model by introducing a small bias to the coefficients. In the standard linear model

$$y = Xb + r \tag{8}$$

where y is a (n x 1) vector of observations on the dependent variable, X is a (n x p) matrix of observations on the p independent variables, b is a (p x 1) vector of parameters to be estimated, and r is an (n x 1) vector of residuals. The ordinary least squares estimates are obtained from the "normal equations"

$$b = (X'X)^{-1}(X'Y). \tag{9}$$

The ridge estimates are obtained by solving

$$br = (X'X + kI)^{-1}(X'y) \tag{10}$$

where I is the identity matrix and k is a small positive number. This model is then estimated for iterations of k; as k increases the estimated coefficients are biased more towards zero.

The objective is to develop a stable set of coefficients, stable in the sense that they are not sensitive to small changes in the data. We used the ridge trace, suggested by Marquardt and Snee (1975), to select the estimates presented in this report. Critics, such as Smith and Campbell (1980), claim that the ridge trace is an "ad hoc" procedure, and proponents, like Vinod (1978), while acknowledging there is no generally acceptable method to determine an optimal k, stress that the ridge trace provides useful visual insights. Our ridge regression estimates exhibited far more stability than estimates obtained from the other procedures.

Our long-run equilibrium estimates of employment effects were obtained by using ridge regression on the reduced form for employment change, equation (7). In most cases, as shown in the next chapter, these estimates are slightly lower that the short-run estimates because wages are allowed to vary in the long run.

Distributed lags

In constructing our distributed lags on federal assistance we employed the ridge procedure, including all the various past years of assistance, without specifying a lag structure. From these results we determined an average lag structure that was cyclical for grants (F_1) and loan guarantees (F_2), increasing for direct loans (F_3):

$$(11)\quad GRANT = .2(F_1\ t) + .3(F_1\ t-1) + .2(F_1\ t-2) + .3(F_1\ t-3)$$

$$(12)\quad GUARLNS = .3(F_2\ t) + .2(F_2\ t-1) + .3(F_2\ t-2) + .2(F_2\ t-3)$$

$$(13)\quad DIRLNS = .04(F_3\ t) + .23(F_3\ t-1) + .23(F_3\ t-2) + .5(F_3\ t-3)$$

LIMITATIONS

While the model estimates net new jobs, in the sense that it isolates the partial effect of each independent variable on employment growth, it does not estimate how many jobs would have been created if government development programs did not exist. Those resources, used for government funded development projects, would have been freed-up for alternative uses. Presumably some employment growth would have occurred from the alternative uses. The difference between what occurred and what would have occurred if the program had not existed can also be called net new jobs. The model does not estimate net new jobs in this latter sense. (For a discussion of substitution and displacement effects see Vernez and Vaughan, 1978.) However, the model does control for the general economic growth that actually did occur by estimating how many jobs were associated with an overall improvement in the national economy.

Another limitation is that the model is not designed to make forecasts. The estimates reflect what occurred during a specific period of time, 1974-78, for a particular mix of programs. The

model serves to track, over a historical period, the employment effects of the various types of federal economic development assistance programs.

Implicit in the model's framework is the restrictive assumption that each state has the same rate of response to federal assistance. This limitation, however, is made less restrictive because many variables are expressed as percent changes rather than levels. Thus, we assume a constant elasticity across states, not a constant absolute response. Also, we estimated the model on a regional as well as a national basis to examine how sensitive our results were with respect to this implicit assumption. The various estimates of the model are presented in the next chapter.

CHAPTER 4

THE RESULTS

Estimates of the model indicate that federal economic development assistance created jobs at the margin during the 1974-78 period.[1] However, estimated employment growth varied by type of assistance, industrial classification, and region. Grants were associated with relatively more additional jobs than loan guarantees, and loan guarantees relatively more than direct loans. This does not necessarily imply that grants were the most cost-effective type of assistance because the cost to the government in using each type of assistance differs. For each type of assistance over 50 percent of the estimated additional jobs occurred in the manufacturing sector. Our results also indicate that grants for public works were more effective than other grants, and that grants were more effective in creating jobs in states with relatively low employment growth.

ESTIMATES OF EMPLOYMENT EFFECTS

Estimating the number of additional jobs associated with an increase in federal economic development assistance requires a three step process. First, coefficient estimates are derived for each industry and type of federal assistance. These coefficients measure the change in the employment growth rate given a unit increase in federal assistance, holding all other factors constant. Second, these coefficients are used to derive elasticities of employment change with respect to federal assistance. These elasticities estimate the responsiveness of the employment growth rate to changes in federal assistance. Finally, the elasticities are used to estimate the incremental numbers of jobs associated with an increase in federal assistance.

Coefficient estimates

Two sets of coefficients involved in the first step are of interest. As described in the previous chapter, our model constitutes a simultaneous equation system that describes the behavior of the labor market. Employment growth rate and relative growth in wages are two endogenous or dependent variables whose values are determined simultaneously within the market. The model also includes a series of other socioeconomic variables related to labor supply and demand and federal economic development assistance variables whose values are not determined within the market (at least as modeled here) but outside it--these are exogenous variables.

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[1]We attempt to show in this chapter that we have been as careful as we can to check for statistical problems. Nonetheless, no one econometric study can be treated as conclusive evidence of job creation effects.

The appearance of endogenous variables among the explanatory variables of at least some of the equations leads to statistical problems that are resolved in two ways. First, we used two stage least squares analysis to estimate the coefficients of the structured equations. Second, the original structural equations are rewritten in "reduced form," a form in which each of the endogenous variables are written as a function of the exogenous variables in the model (not the other endogenous variable). The reduced form equations show explicitly how the endogenous variables are jointly dependent on the exogeneous variables and the error terms in the system. (See Kmenta (1971) for more details.)

As a result, two sets of equations exist: the structural and reduced form equations. Two corresponding sets of coefficients are derived, each with a different interpretation, and both useful. For this reason we present both sets.

The structural form coefficients for the federal assistance variables measure the partial response of the employment growth rate to a unit change in federal assistance, with relative wage rates, the other exogenous variables, and the disturbances held fixed. This, in a sense, is the short-run response to assistance change, since that change has not yet had a chance to work itself out in the market through adjustments in the relative wage rate. The coefficients for the other demand equation variables and the supply equation variables are similarly interpreted: they measure partial response.

The reduced form coefficients, on the other hand, measure long-run response, i.e., wage is allowed to vary. Thus, the reduced form of the model is the appropriate form to use for associating long-term employment growth rates with given values of the exogenous variables and the error terms. The coefficients of the exogenous variables in the reduced form, as Phillips and Wickens (1978) point out, can be interpreted as impact multipliers.

Table 2 presents the variable definitions and table 3 the regression estimates. One noticeable aspect of the coefficient estimates in table 3 is the consistent significance of the shift-share variable (SFTSHAR), regardless of whether the coefficients were from the structural or reduced form models. This variable was used to capture employment change due to general economic conditions. The coefficients' relative magnitude support the notion that a large amount of employment change can be explained by broad business cycle conditions, i.e., conditions beyond the realm of any particular development program. The reduced form coefficients are smaller than either the supply or the demand structural coefficients, indicating that over time the effect of broad economic conditions on employment growth rate is somewhat attenuated. Other factors in the model explain variations not accounted for by the shift-share variable.

Table 2

Variable Definitions

Endogeneous variables

e_{ijt} = annual percentage change in employment, for industry i, in state j, in time period t. In the remaining definitions, the subscripts have been dropped for simplicity.

RELWAGE = annual percentage change in relative wages (state payroll per employee by industry relative to national payroll per employee in that industry) (estimated from the reduced form wage equation, equation 6)

Independent variables

SFTSHAR = shift share factor (national growth rate in employment, by industry)

(Federal assistance variables)

GRANTS = disbursed federal economic development grants per $1,000 of income, distributed over 4 years (a weighted distributed lag)

GUARLNS = obligated economic development loan guarantees per $1,000 of income, distributed over 4 years (a weighted distributed lag)

DIRLNS = disbursed economic development loans per $1,000 of income, distributed over 4 years (a weighted distributed lag)

TARGET = a proxy for the targeting effect (obligated federal economic development grants per $1,000 of income)

OTHRFED = level of other federal spending (military and highway obligations per $1,000 of income)

(Other socioeconomic variables)

RELATTR = relative attractiveness factor (annual percentage change in labor and proprietors income deflated by distance)

GOVEMP = employment opportunities in state and local government (state and local government employment per capita)

METRO = percent of state population living in metropolitan areas

Table 2 (continued)

UNEMP = state unemployment rate

PCTSUN = percent mean annual possible sunshine

DUMNV = a dummy variable for Nevada

DUMWY = a dummy variable for Wyoming

BUSCOND = a proxy for business conditions (state corporate income tax revenue relative to personal income)

HSRENT = percentage of payroll per employee used for housing rental expense

Table 3

Coefficient Estimates for the Structural and Reduced Form Equations, by Industry [a]
(* = significant at .05 level, dependent variable is percentage change in employment)

	Trade			Manufacturing			Construction		
Variables	Supply	Demand	Reduced Form	Supply	Demand	Reduced Form	Supply	Demand	Reduced Form
Constant	-.9808*	-.4339	-.3710	-1.106*	-.6949	-.4320	-.5528*	-.2533	-.1623
SFTSHAR	.9980*	.4274*	.3805*	1.247*	.6348*	.4819*	.4367*	.2054*	.1818*
RELWAGE	.2617	-.0019		.0126*	.0163		.8775	.1878	
GRANTS		.0059*	.0049*		.0233*	.0184*		.0069*	.0043
GUARLNS		.0019*	.0013*		.0062*	.0033		-.0043*	-.0044
DIRLNS		.0010*	.0002		.0032*	-.0014*		-.0037*	-.0053*
TARGET		-.0007*	-.0007*		-.0019*	-.0018		.0002	.0004
OTHRFED		-28.13	14.93		-180.0	6.37		16.32	-57.64
RELATTR		.0010*	.0012*		.0018*	.0046*		.0002	-.0002
GOVEMP	-.0001		.0006*	.0005		.0018*	.0033*		-.0011
METRO	-.0004*		-.0001	-.0005		-.0001	.0002		.0002
UNEMP	-.0017		-.0032	-.0223*		-.0165*	-.0031		-.0066*
PCTSUN	.0004		-.0001	.0008		-.0007	-.0007		.0008
DUMNV	.0618*	.0268*	.0268*	.1084*	.0508*	.0457	.0370	.0293	.0259
DUMWY	.0250	.0149*	.0090*	.0339	.0329	.0128	-.6953*	-.2967*	-.2888*
BUSCOND		-.7306*	-.5228		-1.64	-1.27		2.08	1.57
HSRENT		-.00007	.0001		.0002*	.0001		.0002*	.0002
R^2	.73			.61			.39		

	FIRE			Personal Serv.			All Ind. Total [b]		
Variables	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.7726	-.2892	-.2216	-1.167*	-.0531	-.4071	-.9531*	-.4347	-.3475
SFTSHAR	.8143*	.3038*	.2503*	1.196*	.5252*	.4305*	.9386*	.4096*	.3392*
RELWAGE	-.1003*	-.0039		1.04*	.4507*		-.4759*	.0579	
GRANTS		.0065*	.0061*		.0075*	.0070*		.0075*	.0064*
GUARLNS		.0023*	.0019*		.0031*	.0024*		.0025*	.0018*
DIRLNS		.0004	-.0001		.0009	.0002		.0012*	-.0001
TARGET		-.0013*	-.0015*		-.0007	-.0054		-.0003*	-.0005
OTHRFED		-5.495	32.21		-53.81	13.49		-29.31	19.96
RELATTR		.0006	.0009		.0004	.0012*		.0010*	.0014*
GOVEMP	.00002		.0006*	.0002		.0007			.0008*
METRO	.0001		.0001	-.0001		-.0001	.0006		-.0001
UNEMP	-.0027		-.0026*	-.0056		-.0054	-.0002*		-.0052*
PCTSUN	.00001		-.0003	-.0001		.0001	-.0041*		.0001
DUMNV	.0570*	.0161*	.0162	.0571	.0297	.0281	.0005	.0298*	.0276*
DUMWY	.0392	.0102	.0052	.0315	.0226	.0150	.0564*	.0202*	.0135
BUSCOND		-.0060	.1536		.3511	.4642	.0265	-.4777*	-.2948
HSRENT		.00008*	.0001		.00006	-.0001		.0001*	.0001
R^2	.38			.35			.75		

[a]Demand equations were estimated using ridge regression with k = 1.0. Discussion on k's selection is presented in chapter 3. The R^2 was not calculated in the computer program (SAS) using ridge regression.

[b]This includes other industries not appearing in this table.

The breakdown of employment growth by industry reveals that this growth, when analyzed in a pooled cross-sectional context, is not very sensitive to changes in relative wages (RELWAGE). One reason for this could be the notion that institutional arrangements exist between states or regions leading to uniform movement in wage rates. Beaumont (1983) found that the relativities among wage rates were quite stable during the 1958-78 period.

The federal assistance variables are significant in many cases. In the short run, grants (GRANTS) and loan guarantees (GUARLNS) register significant employment effects in each of the industries shown. In the long run, grants continue to register significant employment effects in all industries shown except construction, while guaranteed loans are significant for retail and wholesale trade; finance, insurance, and real estate; and personal services.

The direct loans (DIRLNS) variable is significant in three of the five industries in the short run and in manufacturing and construction in the long run. Notice that the size of the coefficient declines when moving from grants to loan guarantees to direct loans, in both the short and the long run. The statistical significance of these differences is important, since we would like to know whether economic development is stimulated more as we move from direct loans to loan guarantees to grants.

We used restricted regression to determine whether the coefficients for each type of assistance were significantly different from each other. For the retail and wholesale trade; manufacturing; finance, insurance, and real estate industries; and for the total of all industries (not just the five industries appearing in table 3) the (short run) coefficients were significantly different from each other. For the personal services and construction industries, the coefficients on the three types of assistance were not significantly different.

The proxy variable used to pick up the presence of targeting (TARGET) is, as hypothesized, negative and significant in three industries in the short run, two industries when wages adjust in the long run. Thus, it appears that relatively more grants were promised to states lagging behind the nation in employment growth in household, retail and wholesale trade; manufacturing; and finance, insurance, and real estate.

The relative attractiveness variable (RELATTR) is positive and significant in the two largest industries, manufacturing and wholesale and retail trade. This variable is based on the idea that access to growing markets attracts firms, i.e., they will relocate to or expand in the growing areas; hence, labor demand increases.

The various socioeconomic factors (Z_k) do not register significance in every industry. For example, employment in state and local governments (GOVEMP) is positively related to employment growth in the construction industry in the short run, while the state unemployment rate (UNEMP) is negatively related to employment growth in the manufacturing industry, both in the short and long run. Some of these variables are significant for one industry, others significant for another industry. Some are significant only in the short run, some significant only in the long run, and still others are significant in both or neither. This differential effect suggests that it is important to consider employment change by industry, rather than total employment change for an area, and to consider short- and long-run responses.

Elasticity estimates

To estimate the employment effects of federal assistance programs, it is first necessary to obtain employment elasticities. The elasticities are based on the mean values of appropriate variables, and are calculated using the formula

(14) $$b_{ig}(F^*_g/e^*_i) = x_{ig}$$ where

b_{ig} = the estimated coefficient for the i^{th} industry and the g^{th} type of federal assistance, $i = 1,...,5$, and $g = 1,...,3$

F^*_g = the average amount of federal assistance per $1,000 of income for assistance type g

e^*_i = the average growth rate in employment in industry i

x_{ig} = the elasticity of employment change in industry i with respect to the g^{th} type of federal assistance.

The elasticity estimates from equation (14) are used to calculate the number of additional jobs associated with a 1 percent change in assistance using the formula

(15) $$[x_{ig}(.01)e^*_i]E^*_i = J_{ig}$$ where

E^*_i = average employment in industry i

J_{ig} = the number of additional jobs in industry i associated with federal assistance type g.

To calculate the cost per additional job from a 1-percent increase in assistance, it is first necessary to derive the dollar amount (cost) of this percentage increase in assistance. This can be found using the equation

(16) $$F^*_g(.01)Y^* = A^*_g$$ where

Y^* = average income in thousands of dollars ($30,254,952)

A^*_g = the cost of a 1-percent change in the average dollar amount of federal assistance g.

(F^*_1 = \$1.80, F^*_2 = \$2.37, and F^*_3 = \$2.46.)

A^*_g divided by the estimated number of jobs gives the estimated marginal cost per job from a 1-percent increase in assistance

$$(17) \qquad A^*_g/(\Sigma J_{ig}) = C_g \qquad \text{where}$$

C_g = the marginal cost per job from a 1-percent increase in the g^{th} type of federal assistance.

This cost estimate is based on a small incremental change in assistance. It is not appropriate to apply this estimate to a substantial increase in assistance because one cannot expect a greatly expanded program to have the same marginal cost as that estimated here.

Since changes in assistance are the result of a political process, and such changes are debated on the basis of dollars and cents rather than in percentage terms, here it is perhaps more relevant to estimate costs in terms of a given dollar change in assistance. We selected \$500,000, which is slightly less than a 1-percent increase in grants, as a dollar standard across types of assistance. The estimates in tables 4 to 6 show that a \$500,000 increase in economic development grants would be associated with an increase of 216 jobs at a cost per additional job of \$2,315. A loan guarantee of similar amount would be associated with 57 new jobs, implying \$8,772 in additional guarantees per additional job, and a direct loan would be associated with 19 new jobs, implying \$26,316 in additional loans per additional job.[2]

Interestingly, Martin, Kiker, and Graham's (1980) cost estimate is very close to ours. Using an indirect approach, they calculated a cost per job of \$3,188 for EDA grants. Their regression model, based over the 1962-74 period, estimated income, and they indirectly derived employment influences from the income effect.

The figures in table 4 were derived using the coefficients of table 3. Recall that there is a statistically significant difference between the coefficients for trade; manufacturing; finance, insurance, and real estate; and the industry totals over the short run. Since the job estimates represent linear combinations of the coefficients, the same holds for them.

It is tempting to conclude that grants were more cost effective than loans and loan guarantees, but such a conclusion overlooks the possibility that the relative importance of the job

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[2]Corresponding figures showing the additional jobs asociated with a 1-percent increase in federal assistance, as well as the elasticity estimates, can be found in appendix I, table 13.

Table 4

Estimated Additional Jobs Associated With a $500,000 Increase in Economic Development Assistance Grants for the Average State in the Average Year Over the 1974-78 Period

Industry	Initial response	Long-run response
Household, retail, and wholesale trade	28	23
Manufacturing	139	110
Contract construction	8	- [a]
Banking, insurance, and real estate	10	9
Personal services	31	29
Total[b]	216	171
Amount of assistance per additional job	$2,315	$2,924

[a] The dash (-) indicates the estimate was not statistically different from zero.

[b] Estimates for other industries--agriculture, mining, transportation and utilities, business services, and administrative and auxiliary--were not statistically different from zero.

Table 5

Estimated Additional Jobs Associated With a $500,000 Increase in Economic Development Assistance Loan Guarantees for the Average State in the Average Year Over the 1974-78 Period

Industry	Initial response	Long-run response
Household, retail, and wholesale trade	9	6
Manufacturing	37	- [a]
Contract construction	-5[b]	-5
Banking, insurance, and real estate	3	3
Personal services	13	10
Total[c]	57	14
Amount of assistance per additonal job	$8,772	$35,714

[a] The dash (-) indicates the estimate was not statistically different from zero.

[b] The negative figures suggest jobs were being shifted between industries.

[c] Estimates for other industries--agriculture, mining, transportation and utilities, business services, and administrative and auxiliary--were not statistically different from zero.

Table 6

Estimated Additional Jobs Associated With a $500,000 Increase in Economic Development Assistance Direct Loans for the Average State in the Average Year Over the 1974-78 Period

Industry	Initial response	Long-run response
Household, retail, and wholesale trade	5	- [a]
Manufacturing	19	-
Contract construction	-5[b]	-7
Banking, insurance, and real estate	-	-
Personal services	-	-
Total[c]	19	-7[d]
Amount of assistance per additional job	$26,316	

[a] The dash (-) indicates the estimates were not statistically different from zero.

[b] The negative figure suggests jobs are being shifted among industries.

[c] Estimates for other industries--agriculture, mining, transportation and utilities, business services, and administrative and auxiliary--were not statistically different from zero.

[d] The model did not indicate there would be any long-run job creation.

creation objective may vary systematically between types of assistance. Also, grants may not be more cost effective because the cost to the federal government for each type of assistance is different. The federal government bears the full cost of a $500,000 grant, while the cost of a similar loan or loan guarantee is considerably smaller, as long as no default occurs. One reason why a $500,000 grant was estimated to be the most stimulative is because it entails the largest subsidy.

Tables 4 through 6 also depict how our estimates of new jobs vary by industry classification. For each type of assistance, those estimates indicate that over 50 percent of the additional jobs would have occurred in the manufacturing industry. Personal services and household, retail, and wholesale trade ranked second and third, respectively. Several industries (as shown by footnotes in the tables) did not register a statistically significant job change when analyzed by the model.

One result that may seem puzzling is the low amount of employment growth in the construction industry. Since several economic development programs funded projects that directly employed construction workers, did the model fail to pick up that industry? The model did pick up construction; however, it only estimates net new jobs. The jobs of construction workers switching from one federally funded project to another are not counted as net new jobs. Also, during this period the construction industry had the slowest average rate of growth, 0.66 of a percent in the average state, while manufacturing had the highest growth rate. Generally, the business cycle affects the construction industry more severely than other sectors. Although there were not many jobs added to the ranks of construction workers, the construction industry was nevertheless relatively sensitive to grant programs. It is not known how many jobs would have been lost were it not for the grant. The elasticity estimate of construction employment change with respect to grants was 1.87, highest among industry classifications (see table 13, appendix I).

Sensitivity tests

Once the coefficients are estimated, it is important to determine how sensitive these estimates are to changes in the model. Following an approach similar to Leamer's (1983), we dropped various demand and supply side variables to see if the coefficient estimates on the grant, loan, and loan guarantee variables changed. If these coefficients were not relatively stable, then little could be concluded about the employment effects of these programs.

Table 7 presents the reduced form estimates for the all industry total. As can be seen from the table, we exclude different sets of variables while maintaining the shift-share and policy variables. Column 1 reproduces the all industry results from table 3. Column 2 drops two demand side variables, column 3 drops three supply side variables, and so forth. The grants coefficient ranged from .0051 to .0069 and was always significant. The loan guarantee coefficient ranged from .0017 to .0029 and was always significant. The direct loan coefficient was only significant in one case (column 6), thus the estimates are inconclusive with respect to direct loans. However, the estimated coefficients for grants and loan guarantees are relatively stable.

PUBLIC WORKS VERSUS OTHER GRANTS

Although it cannot be concluded from the estimates that grants were more cost effective than other types of assistance, it is possible to compare cost effectiveness among various types of grants. The estimates in table 8 indicate that public works grants were more cost effective than business and community development grants, both in the short and long run. According to these estimates, an additional $500,000 public works grant to the average state in the average year during the 1974-78 period would be associated with 248 new jobs, relative to only 156 for other types of grants in the short run and 241 versus 160 jobs in the long run.[3] Restricted regression tests were conducted to determine whether the coefficients (both structural and reduced form) were significantly different, or whether they represented differences that could be due to chance. For finance, insurance, and real estate; manufacturing; and construction the coefficients for public works grants and other grants were significantly different, and hence, the estimated numbers of additional jobs in those industries differed.

[3]The coefficient estimates for the structural and reduced form equations for public works and other grants, upon which table 8 figures are based, appear in table 14 of appendix I.

TABLE 7

Sensitivity Tests
(Reduced-form for all industry total)
(* = significant at .05 level, dependent variable is employment change)

Variables	1	2	3	4	5	6	7	8	9
Constant	-.3475	-.3482	-.3986	-.3492	-.3506	-.3812	-.3501	-.4261	-.3488
SFTSHAR	.3392*	.3397*	.3820*	.3391*	.3409*	.3836*	.3373*	.3792*	.3384*
GRANTS	.0064*	.0064*	.0069*	.0062*	.0060*	.0051*	.0061*	.0068*	.0063*
GUARLNS	.0018*	.0018*	.0027*	.0017*	.0018*	.0029*	.0020*	.0025*	.0017*
DIRLNS	-.0001	-.00007	.0006	.0001	-.00004	.0010*		.0003	-.00006
TARGET	-.0005	-.0005	-.0005		-.0005	-.0008		-.0006	-.0005
OTHRFED	19.96	21.16	8.31	19.72	13.17	-4.58		4.77	20.80
RELATTR	.0014*	.0014*	.0015*	.0014*	.0014*		.0014*	.0013*	.0014*
GOVEMP	-.0008*	.0008*		.0008*	.0009*		.0008*	.0008*	.0008*
METRO	-.0001	-.00002		-.00001	-.00002	-.00006		-.00005	-.00002
UNEMP	-.0052*	-.0052*		-.0052*	-.0052*		.53*		-.0052*
PCTSUN	-.0001	-.00001	-.00001	-.00003	.00004	.0001		-.00001	-.00003
DUMNV	.0276*	.0258*	.0281*	.0277*			.0280*	.0257*	.0282*
DUMWY	.0135	.0139	.0210*	.0136		.0178		.0170	.0141
BUSCOND	-.2948		-.6305	-.3084	-.5683	-1.08		-.5823	
HSRENT	.0001		.00004	.00002	.00002	.00006		.00003	.00002

Table 8

Estimated Additional Jobs Associated With a $500,000 Increase in Grant Disbursements for the Average State in the Average Year Over the 1974-78 Period, by Type of Grant

Industry	Public Work Grants[a] Initial response	Public Work Grants[a] Long-run equilibrium response	Other Grants[b] Initial response	Other Grants[b] Long-run equilibrium response
Household, retail, and wholesale trade	33	30 (18-43)[c]	18	19 (7-30)
Manufacturing	170	172 (116-227)	74	75 (26-123)
Contract construction	5	- [d]	17	19 (12-25)
Banking, insurance, and real estate	15	14 (9-19)	-	-
Personal services	25	25 (2-48)	47	47 (23-70)
Total[e]	248	241 (145-337)	156	160 (68-248)
Amount of assistance per additional job	$2,016	$2,075 ($1,484-$3,448)	$3,205	$3,125 ($2,016-$7,353)

[a] The public works programs in our analysis were primarily from EDA. (See table 19.)

[b] Includes community development, business development, and block grants, but does not include technical assistance, planning grants, and demonstration projects.

[c] The numbers in parentheses represent the 95-percent confidence interval around the point estimates derived from the model.

[d] The dash (-) indicates the estimate was not statistically different from zero.

[e] Estimates for other industries--agriculture, mining transportation and utilities, business services, and administrative and auxiliary--were not statistically different from zero.

The estimated cost per additional job, in the short run, would have been $2,016 for public works grants compared with cost of $3,205 for other types of grants. Thus, our results indicate that, during this time period, public works grants were more cost effective than other types of grants.

Table 8 presents point estimates of the number of additional jobs associated with a $500,000 increase in grant disbursements in the short-run. Point estimates are limited in that they do not provide information about the precision of the estimate--i.e., about the magnitude of the error due to sampling. Interval estimates, on the other hand, provide such information. Table 8 also provides 95-percent confidence interval estimates, in parentheses, for the long run. From these figures it can be seen that the confidence interval about the long run cost of public works assistance per job is much tighter, and hence, more precise than the interval around the long run "other grants" assistance cost per job.

EMPLOYMENT EFFECTS ON A REGIONAL BASIS

The model's estimates are based on various assumptions. One implicit assumption is that states respond to economic stimulus in the same manner. To reduce the restrictiveness of this assumption, we ran the model on a regional basis. Using cluster analysis, we separated the states into three groups or regions based on employment growth: high, medium, and low. (Table 9 shows the groups.) In general, we found evidence that grants were more cost effective in the low employment growth region.

As with the analyses described previously, structural and reduced form coefficients were derived for three subsets of states that exhibited similar employment growth patterns (low, medium, or high). These coefficients appear in tables 15, 16, and 17 in appendix I. The coefficients suggest that in the long run, unemployment rates are not related to employment growth rates in high employment growth states, while they are inversely related to employment growth rates for low- and medium-growth rate states.

Overall, there is a consistency in the coefficient estimates for the three groups of states. The responses to economic development assistance are plausible; the differences are merely ones of magnitude.

Table 9

Regional Breakdown of States Grouped by Employment Growth from 1974 through 1978[a]

Low employment growth	Medium employment growth	High employment growth
Connecticut	Alabama	Arizona
Delaware	Arkansas	California
District of Columbia	Georgia	Colorado
Florida	Hawaii	Idaho
Illinois	Iowa	Kansas
Indiana	Kentucky	Louisiana
Maryland	Maine	Minnesota
Massachusetts	Michigan	Montana
New Jersey	Mississippi	New Hampshire
New York	Missouri	New Mexico
North Carolina	Nebraska	North Dakota
Ohio	Oklahoma	Oregon
Pennsylvania	South Carolina	South Dakota
Rhode Island	Tennessee	Texas
West Virginia	Virginia	Utah
	Wisconsin	Vermont
		Washington

[a] Alaska, Nevada, and Wyoming were excluded from the groups because their growth patterns were significantly different from the average for any of the three categories. These groups were obtained using hierarchical cluster analysis.

Table 10 presents estimates of the number of additional jobs associated with an additional $500,000 grant. The table shows both the initial response and the long-run equilibrium response for the three categories of low, medium, and high employment growth states. A $500,000 increase in grants for the average state in the low employment growth region initially would be associated with 407 additional jobs during the average year over the 1974-78 period. In the long run, this would correspond to 332 jobs. On the other hand, in the high employment growth region a $500,000 increase in grants initially would be associated with 137 additional jobs for the average state. This difference between regions was found to be statistically significant based on a procedure, as outlined by Fisher (1970), that tests the equality of a subset of coefficients between regressions.

In terms of cost per job, our results indicate that grants were most cost effective in the low employment growth region, costing $1,229 per additional job compared with $3,676 per job in the high employment growth region in the short run. The cost gap is even larger in the long run. Thus, it appears that an addition in grants would have been most cost effective for the region that lagged behind the nation in employment growth.

ECONOMIC DEVELOPMENT ASSISTANCE AND GENERAL ECONOMIC GROWTH

Based on estimates from our model, it appears that, during the mid-1970's, economic development assistance did create new jobs. In 1978 the grant programs we analyzed obligated almost $4.6 billion for economic development. However, relative to the size of the economy, this amount represented only 0.2 percent of the gross national product.

We controlled for each industry's growth due to general growth in the economy and found that economy-wide growth was associated with a relatively large number of jobs. Table 11 presents the estimates of the additional jobs for the average state associated with a 1-percent increase in grants, loan guarantees, direct loans, and general industry growth. A 1-percent increase in general industry growth would have been associated with an increase of 5,391 new jobs.

In contrast, a 1-percent increase in economic development grants would have been associated with 235 new jobs. But notice that 235 jobs is 4.4 percent of 5,391 jobs. So economic development grants, which in 1978 represented only 0.2 percent of the gross national product appear to have been a relatively efficient way to create additional jobs, when efficiency is measured by their size relative to the total economy.

Our results also indicate that the manufacturing industry would have benefited most from increased economic growth, followed by personal services and the household, retail, and wholesale trade industry. This result should not be surprising since these industries represented 66.6 percent of total industry employment in 1978.

Table 10

Estimated Additional Jobs Associated With a $500,000 Increase in Grant Disbursements for the Average State in the Average Year Over the 1974-78 Period, By Employment Growth Category

Industry	Low Employment Growth		Medium Employment Growth		High Employment Growth	
	Initial response	Long-run equilibrium response	Initial response	Long-run equilibrium response	Initial response	Long-run equilibrium response
Household, retail, and wholesale trade	29	28	33	43	18	20
Manufacturing	278	206	124	133	109	110
Contract construction	15	14	15	13	- [a]	-
Banking, insurance, and real estate	19	15	9	9	10	10
Personal services	66	59	38	50	-	-
Total[b]	407	332	219	248	137	140
Amount of assistance per additional job	$ 1,229	$1,553	$2,283	$2,016	$3,650	$3,571

[a] The dash (-) indicates the estimates were not statistically different from zero.

[b] Estimates for other industries--agriculture, mining, transportation and utilities, business services, and administrative and auxiliary--were not statistically different from zero.

Table 11

Estimated Additional Jobs Associated With a 1-Percent Increase in Economic Development Assistance and General Industry Growth for the Average State in the Average Year Over the 1974-78 Period

Industry	Economic Development assistance			General industry growth
	Grants	Loan guarantees	Direct loans	Shift-share factor
Household, retail, and wholesale trade	31	13	7	1,286
Manufacturing	151	53	28	2,293
Contract construction	9	-8[a]	-7	156
Banking, insurance, and real estate	10	5	- ([b])	278
Personal services	34	18	-	1,378
Total[c]	235	81	28	5,391

[a] The negative signs suggest jobs are being shifted among industries.

[b] The dash (-) indicates the estimates were not statistically different from zero.

[c] Estimates for other industries--agriculture, minning, transportation and utilities, busines services, and administrative and auxiliary--were not statistically different from zero.

COMPARISON WITH OTHER STUDIES

It is difficult to compare the estimates presented here with other studies. Most other studies estimated a different cost figure--average cost. Our estimates are marginal cost, i.e., the change in employment with respect to a small increment in grants or loan guarantees, holding all other factors constant. In general the estimates presented here are lower than many studies, which may imply that marginal costs are below average costs. In other words, it may be that these grant programs, in effect, were subject to economies of scale, and were producing in the decreasing segment of their average cost curve.

Table 12 presents various cost per job estimates, noting the basis on which the estimates were made. We really cannot compare, for example, the Congressional Budget Office (CBO) (1975) public works cost per job estimate of $12,500 to 15,625 with our public works cost per job estimate of $1,484 to 3,448 because the former is based on average cost while the latter is based on marginal cost. Both studies represent attempts to estimate program effects. However, the CBO estimate attempts to capture the full effect of the program, while our study focuses on the effect of a marginal change in the application the these grants.

Cost per job estimates have a wide variance for several reasons. Two basic reasons, which were discussed previously, are the use of different methodologies and the use of different definitions of cost. Because the estimates vary so widely it is important to be careful in making any policy application based on the estimates. However, the methodology we employed can be useful in making relative comparisons across programs. As table 12 illustrates, grants for public works appear to be relatively more cost effective at the margin than other types of grants.

Table 12

Selected Alternative Cost per Job Estimates

Type of program	Source	Cost per job
Highway	Bezdek & Hannon (1974)	$12,346[a]
Educational facilities	Bezdek & Hannon (1974)	11,765[a]
Water and waste treatment	Bezdek & Hannon (1974)	12,195[a]
Accelerated Public Works	CBO (1975)	12,500-15,625[b]
Public Works	Barrows & Bromley (1975)	34,483[c]
Sewer plant	Vernez et al. (1977)	9,259[a]
Flood protection	Vernez et al. (1977)	5,464[a]
Public Works	Shaikh & Salinas (1979)	3,710-4,277
Public Works	OMB (1979)	18,735-23,107[a]
EDA programs	Martin, Kiker, and Graham (1980)	3,819[d]
Public Works	This study	1,484-3,448[e]
Other grants	This study	2,016-7,353[e]

[a] Average cost based on gross employment estimate (direct, indirect, and induced).
[b] Average cost based on net employment estimate (direct, indirect, induced).
[c] Marginal cost based on direct gross employment estimate only.
[d] Average cost based on employment growth estimate over the 1962-74 period.
[e] Marginal cost based on employment growth (direct, indirect, and induced) as estimated in an econometric model rather than from input-output multipliers.

SOURCES: Roger H. Bezdek and Bruce Hannon, "Energy, Manpower, and the Highway Budget," Journal of Environmental Systems, 1974; Congressional Budget Office, "Temporary Measures to Stimulate Employment," September 1975; Richard L. Barrows and Daniel W. Bromley, "Employment Impacts of the Economic Development Administration's Public Works Program," American Journal of Agricultural Economics, 57 (February 1975), pp. 46-54; Georges Vernez, Regional Cycles and Employment Effects of Public Works Investments, The Rand Corporation, R-2052-EDA, January 1977; Abdul Q. J. Shaikh and Patricia Wilson Salinas, "Evaluating Economic Development Programs: The Subjectivity of Cost Per Job Measures," a paper prepared by the authors while with the Policy Office of EDA, 1979; Office of Management and Budget, Public Works and Countercyclical Assistance, November 1979; Randolph C. Martin, B. F. Kiker, and Robert E. Graham, Jr., "The Effectiveness of Economic Development Administration Programs: Income Growth, Cost Per Job, and Human Migration," Report prepared for EDA, April 1980.

CHAPTER 5

SUMMARY AND CONCLUSIONS

The federal government in the past has attempted to increase income and create jobs through economic development assistance programs. There has been much debate over the effectiveness of such programs, with no concensus forthcoming. Past efforts to evaluate program effectiveness, mostly the case study approach, have produced widely varying results, which could not readily be compared across programs.

We began this project with the notion of developing an approach to evaluate consistently the effect of various federal economic development programs on employment. We did not evaluate these programs on objectives other than job creation. Comparisons between program results obtained through the case study approach could be misleading because agencies usually selected the projects surveyed, developed their own separate criteria of what jobs to count, and measured effects differently. Also, to the extent that case studies were based on projects not selected randomly, the results could not be generalized. For example, results obtained through the study of a small number of local public works grant projects may or may not be representative of all the projects funded by the program, much less all public works grant programs.

In reviewing the policy evaluation literature, we found that the preponderance of federal agency studies relied on the case study approach. This, however, is not the only possible approach. Econometric modeling offers another way, albeit with its own set of limitations distinct from the drawbacks of the case study approach. As with any model, the results are tentative due to the nature of statistical inference and the limitations of both the data and the current state of the art in parameter estimation techniques. Thus, one should be cautious in interpreting the results. But this approach does provide a framework in which to evaluate programs consistently and make statistical generalizations across programs.

Chapter 2 of this report discusses the relevant literature from both the case study and econometric modeling approaches. In this chapter, we discuss the general issue of how to estimate the causal links between policy and economic activity. More specific issues such as how to define a job, how to evaluate the dynamics of program effects, and how to apply a consistent methodology are also discussed.

The specific model we developed is presented in chapter 3. Basically, we followed the approach of previous researchers, in both Great Britain and the United States, who developed econometric models to estimate a relationship between policy and economic activity. The specific economic activity considered

here, following the approach of an NBER (1976) study, was adjustment in the labor market. Based on economic theory, the quantity of labor supplied and demanded is a function of both local and national factors, including measures of federal economic development assistance. After presenting the model in general, the remainder of chapter 3 discusses the specific variables in the model and the estimation techniques used.

Chapter 4 presents the results of our model. Based on estimates from this model, it appears that employment effects differed depending on industry and also on regional employment characteristics. Regardless of whether aid was provided in the form of grants, direct loans, or loan guarantees, the greatest employment effect was in the manufacturing sector of the economy. Public works grants were found to have a greater effect on employment than other grants, which included community development block grants and grants for business development. These estimates are not comparable to those derived for direct loans or loan guarantees because the cost to the federal government for each type of assistance is different.

One of the assumptions implicit in the econometric results is that states respond to economic stimulus in the same manner. By reducing this restriction and grouping states into three clusters according to whether they were characterized by low, medium, or high employment growth, we found that grants were most effective in states with low employment growth.

The model developed here demonstrates the possibility of using this approach in evaluating other federal programs. The results obtained from an econometric model do not replace the results from case studies, but add another dimension to comprehensive program evaluation. Both approaches provide different but useful information for the policymaker, and when used together could result in more informed policy decisions.

Table 13

Estimated Additional Jobs Associated with a 1 Percent Increase in Federal Assistance for the Average State in the Average Year over the 1974-78 Period[a]

Industry	Additional jobs created			Elasticity estimate			Average change in employment	Average employment
	Grants	Loan guarantees	Direct loans	Grants	Loan guarantees	Direct loans		
Household, retail, and wholesale trade	31	13	7	.2520	.1069	.0585	0.0420	289,846
Manfacturing	151	53	28	1.3840	.4853	.2606	0.0302	361,637
Contract construction[b]	9	-8	-7	1.8787	-1.5427	-1.3810	0.0066	74,452
Banking, insurance, and real estate[c]	10	5	-	.2060	.0961	-	0.0566	89,099
Personal services	34	18	-	.2416	.1316	-	0.0557	249,401
Total	235	81	28					
Amount of assistance per additional job[d]	$2,311	$8,836	$26,592					

[a] Job creation estimates in other industries -- agricultural, minning, transportation and utilities, business services, and admininstrative and auxiliary--were not statistically different from zero.
[b] The negative sign suggest jobs are being shifted among industries.
[c] The dash (-) indicates the estimates were not statistically different from zero.
[d] Attempts to calculate by hand may yield slightly different results due to rounding.

APPENDIX I

Table 14

Public works: Coefficient Estimates for the Structural and Reduced Form Equations, by Industry[a]
(* = significant at .05 level, dependent variable is employment change)

	Trade			Manufacturing			Construction		
Variables	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.9868*	-.4414	-.4074	-1.2072*	-.7068	-.5884	-.5805*	-.2740	-.2349
SFTSHAR	1.0059*	.4337*	.4113*	1.2604*	.6441*	.5710*	.4803*	.2105*	.1939*
RELWAGE	.5327	-.0807		.8448*	.1981		1.4941*	.5196*	
PWGRNTS		.0069*	.0062*		.0284*	.0287*		.0043	.0017
OTHRGRNT		.0037*	.0039		.0124*	.0125*		.0139*	.0152*
GUARLNS		.0019*	.0013*		.0058*	.0039*		.0012	.0003
DIRLNS		.0010*	.0005		.0023	.0011		.0009	.0006
TARGET		-.0006*	-.0007*		-.0010	-.0011		.0008	.0011
OTHRFED		-.0142	-.0072		-.1272	-.1198		-.0683	-.0553
RELATTR		.0009*	.0009*		.0019*	.0019*		.0008	.0008
GOVEMP	-.0001		.0004	.0007		.0013*	.0030*		.0006
METRO	-.0004*		-.0001	-.0004		.0001	.0001		.0001
UNEMP	-.0011		-.0026*	-.0194*		-.0123*	-.0018		-.0054
PCTSUN	.0003		-.0001	.0017		-.0002	-.0008		-.0001
BUSCOND		-.3796	-.2136*		-.7334	-.2244		.2330	.3893
HSRENT		.00001*	.0001		.0002*	.0001		.0001*	.0001
R^2	.71			.61			.27		

APPENDIX I

Variables	FIRE			Personal Service			Total All Industries[b]		
	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.6989*	-.3064	-.2679	-1.1087*	-.5588	-.4558	-.8179*	-.4335	-.3961
SFTSHAR	.7439*	.3257*	.2948*	1.1384*	.5423*	.4647*	.8178*	.4060*	.3687*
RELWAGE	-.0477	.0032		.5292*	.5292*		-.3706*	-.1566*	
PWGRNTS		.0099*	.0098*		.0060*	.0061*		.0083*	.0079*
OTHRGRNT		-.0007	-.0004		.0113*	.0113*		.0072*	.0076*
GUARLNS		.0023*	.0019*		.0034*	.0027*		.0026*	.0019*
DIRLNS		.0003	.0001		.0006	.0001		.0011*	.0005
TARGET		-.0012	-.0012*		-.0013	-.0013		-.0004	.0003
OTHRFED		.0112	.0190		-.0387	-.0321		-.0082	-.0096
RELATTR		.0005*	.0005*		.0004	.0006		.0010*	.0010*
GOVEMP	.0002		.0004	.0002		.0005	.0005		.0007*
METRO	-.0001		.0001	-.0001		-.0001	-.0002*		.0001
UNEMP	-.0028		-.0022	-.0061		-.0053*	-.0059*		-.0043*
PCTSUN	-.0001		-.0002	.0001		.0001	.0007*		-.0001
BUSCOND		.4767	.5111		.8553	.6952		-.1015	.1498
HSRENT		.0001*	.0001		.0001	.0001		.0001*	.0001*
R^2	.33			.32			.-		

[a] Demand equations were estimated using ridge regression with k = 0.5. Discussion on k's selection is presented in chapter 3. The R^2 was not calculated in the computer program (SAS) using ridge egression.

[b] This includes other industries not appearing in this table.

APPENDIX I

Table 15

Low Employment Growth States:
Coefficient Estimates for the Structural and Reduced Form Equations, by Industry[a]
(* = significant at .05 level, dependent variable is employment change)

	Trade			Manufacturing			Construction		
Variables	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.6127*	-.3269	-.3000	-.7836*	-.7999	-.4911	-.9108	-.2849	-.3908
SFTSHAR	.6776*	.3384*	.3430*	1.0562*	.7746*	.6376*	.4760	.2236*	.2055*
RELWAGE	-.5453	.8230*		-.2670*	.8140		.4734	.4293*	
GRANTS		.0064*	.0056*		.0433*	.0321*		.0145*	.0134*
GUARLNS		-.0042*	-.0017		-.0047	-.0053		-.0099	-.0079
DIRLNS		.0047	.0021		.0203	.0021		-.0095	-.0093
TARGET		-.0014*	-.0008		-.0005	.0004		.0009	.0033
OTHRFED		-69.90	-73.49		99.50	-110.10		-48.20	-182.2
RELATTR		-.0008	.0009		-.0003	-.0006		.0021	.0012
GOVEMP	-.0010*		-.00004	-.0008		.0006	.0040		.0006
METRO	-.0003		-.00005	.0002		.00001	.0008		.0005
UNEMP	-.0079*		-.0053*	-.0351*		-.0210*	.0024		-.0097*
PCTSUN	.0011		.0001	.0002		-.0006	.0027		.0028*
BUSCOND		-1.708*	-1.158		-7.793*	-4.553		-.3565	.1696
HSRENT		.00004	.0001		-.0001	.00002		.0001	.00003
R^2	.74			.66			.16		

APPENDIX I

Variables	FIRE			Personal Service			Total All Industries[b]		
	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.3131	-.3383	-.2182	-.5952*	-.5499	-.3277	-.7114*	-.5018	-.4339
SFTSHAR	.3671*	.3483*	.2481*	.6793*	.5285*	.3437*	.7218*	.4804*	.4051*
RELWAGE	.1278	.0605		-1.582*	-.9023		-.8963	.3469	
GRANTS		.0116*	.0095*		.0158*	.0140*		.0131*	.0101*
GUARLNS		-.0015	-.0006		-.0013	-.0009		-.0045	-.0030
DIRLNS		-.0004	-.0020		.0049	-.0058		-.0015	-.0019
TARGET		.0003	.0006		.0020	.0033*		.0007	.0011
OTHRFED		-75.17	-114.69		-48.22	-101.59		-70.44	-119.37
RELATTR		.0005	.0004		.0009	.0005		.0006	.0003
GOVEMP	-.0005		.0001	.0008		.00004			.0005
METRO	-.0005		.0002	-.0004		.0002	-.00003		.0003*
UNEMP	-.0116*		-.0059*	-.0139*		-.0106*	.0003		-.0076*
PCTSUN	.0010		.0001	.0003		.0001	-.0120*		.0005
BUSCOND		-.3681	.4117*		-3.488*	-.3212	.0010	-1.552	-.7931
HSRENT		.0001	.0001		.0001	.0002*		.0001	.0001
R^2	.43			.56			.77		

[a] Demand equations were estimated using ridge regression with k = 0.5. Discussion on k's selection is presented in chapter 3. The R^2 was not calculated in the computer program (SAS) using ridge regression.

[b] This includes other industries not appearing in this table.

APPENDIX I

Table 16

Medium Employment Growth States:
Coefficient Estimates for the Structural and Reduced Form Equations, by Industry[a]
(* = significant at .05 level, dependent variable is employment change)

	Trade			Manufacturing			Construction		
Variables	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.9170*	-.5367	-.5313	-.9487*	-.8234	-.6338	-.6213*	-.3627	-.2407
SFTSHAR	.9810*	.5332*	.5564*	1.2429*	.8200*	.7087*	.6341*	.3399*	.3144*
RELWAGE	-.8780	-.9584*		.2386	-.1983		-.7925	-.3824	
GRANTS		.0068*	.0088*		.0175*	.0187*		.0016*	.0102*
GUARLNS		.0012	.0015		.0026	.0028		-.0017	-.0006
DIRLNS		.0012	.0011		.0044	.0044		-.0004	-.0002
TARGET		-.0009	-.0001		-.0077*	-.0061*		.0033	.0041
OTHRFED		11.16	19.51		-161.03	-110.79		-57.33	-43.89
RELATTR		-.00003	.0002		-.0019	-.0005		-.0006	-.0002
GOVEMP	-.0012		-.0001	-.0029		-.0008	-.0021		.0000
METRO	-.0001		.0004	.0002		.0004	-.0002		.0001
UNEMP	-.0015		-.0033*	-.0186*		-.0135*	.0024		-.0051
PCTSUN	.0003		-.0003	.0004		.0001	.0013		-.0011
BUSCOND		1.3349	1.958		7.2351	8.306*		.3178	-.2862
HSRENT		.00004	.00003		.00003	-.00002		.00002	-.00003
R^2	.86			.68			.65		

Variables	FIRE			Personal Service			Total All Industries[b]		
	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.6206*	-.3030	-.2202	-.8726*	-.5810	-.4100	-.8946*	-.5024	-.4933
SFTSHAR	.7578*	.3222*	.3001*	.9866*	.6009*	.4890*	.9454*	.5112*	.5193*
RELWAGE	-.4428	.0081		1.196	1.030		-.4841	-1.168*	
GRANTS		.0067*	.0069*		.0094*	.0125*		.0072*	.0075*
GUARLNS		.0015	.0015		.0024	.00001		-.0012	.0007
DIRLNS		-.0001	.0003		.0004	.0010		.0010	.0009
TARGET		-.0028	-.0027*		-.0023	-.0014		.0006	.0008
OTHRFED		49.79	59.74		9.786	15.24		-30.86	.196
RELATTR		-.0003	-.0002		-.0015	-.0013		.0004	.0004
GOVEMP	-.0001		-.0001	.0001		.0007			-.00004
METRO	-.0003		.00001	-.0002		.00003	-.0012		.0001
UNEMP	.0001		-.0013	-.0072		-.0072*	.00001		-.0052*
PCTSUN	-.0014		-.0007	.0011		-.0006	-.0030		-.0003
BUSCOND		2.598*	2.100		.8880	1.556	.0006	.4559	1.067
HSRENT		.00006	.0001		-.00002	-.0001		.00001	.00002
R^2	.45			.42			.82		

[a] Demand equations were estimated using ridge regression with k = 0.5. Discussion on k's selection is presented in chapter 3. The R^2 was not calculated in the computer program (SAS) using ridge regression.

[b] This includes other industries not appearing in this table.

APPENDIX I

Table 17

High Employment Growth States:
Coefficient Estimates for the Structural and Reduced Form Equations, by Industry[a]
(* = significant at .05 level, dependent variable is employment change)

	Trade			Manufacturing			Construction		
Variables	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-1.2447*	-.5935	-.6211	-1.4075*	-.8923	-.7412	-.3580*	-.2602	-.2147
SFTSHAR	1.2620*	.6111*	.6439*	1.3808*	.8199*	.7446*	.4137*	.2921*	.2645*
RELWAGE	.6340	-.5478*		.5197	.2747		.1127	-.2277	
GRANTS		.0037*	.0040*		.0248*	.0251*		-.0048	-.0032
GUARLNS		.0006	.0001		.0029	.0013		-.0006	-.0014
DIRLNS		-.0004	-.0006		-.0014	-.0013		-.0008	-.0005
TARGET		-.0009	-.0008		-.0017	-.0023		.0020	.0023
OTHRFED		-63.54	-35.05		-80.83	41.69		-209.05	-150.24
RELATTR		.0007	.0007		.0026	.0039*		.0001	.0004
GOVEMP	.0005		.0003	.0032		.0010	.0009		.0002
METRO	.0001		.00003	.00001		.0002	.0005		.0003
UNEMP	.0033		-.0008	-.0111		-.0119	-.0055		-.0061
PCTSUN	-.0010*		-.0004	.0002		-.0012	-.0013		-.0007
BUSCOND		-.8760	-.5483		-1.193	-1.110		-.4534	-.2739
HSRENT		.0001	.0001		.0003	.0004		-.00003	.0001
R^2	.80			.57			.50		

APPENDIX I

		FIRE		Personal Service			Total All Industries[b]		
Variables	Supply	Demand	Reduced form	Supply	Demand	Reduced form	Supply	Demand	Reduced form
Constant	-.8051*	-.3765	-.3750	-1.3124*	-.7632	-.6625	-.9791*	-.5282	-.4882
SFTSHAR	.8469*	.3939*	.3818*	1.3668*	.7763*	.7178*	.9923*	.5217*	.4870*
RELWAGE	-.0022	.0121		.3807	.0495		.5160	-.1144	
GRANTS		.0070*	.0074*		.0023	.0020		.0064*	.0063*
GUARLNS		.0031*	.0024		.0045	.0034		.0016	.0009
DIRLNS		-.0008	-.0008		-.0008	-.0015		.0001	-.0005
TARGET		-.0030*	-.0029*		-.0025	-.0025		.0011	-.0010
OTHRFED		-80.61	8.179		-172.01	-128.2		-52.66	4.70
RELATTR		-.0009	.0006		.0008	.0013		.0009	.0009
GOVEMP	.0011		.0007	-.0003		-.0001			.0006
METRO	.0002		.0002	-.00002		-.0001	.0009		.0001
UNEMP	-.0010		-.0013	-.0017		-.0043	.00004		-.0036*
PCTSUN	-.0008		-.0006	-.0004		-.00004	-.0017		-.0004
BUSCOND		.0017	-.0303		.9424	.8812	.0004	-.0989	.0045
HSRENT		.0001	.0002		.00004	.00004		.0001	.0002*
R^2	.31			.23			.78		

[a] Demand equations were estimated using ridge regression with k = 0.5. Discussion on k's selection is presented in chapter 3. The R^2 was not calculated in the computer program (SAS) using ridge regression.

[b] This includes other industries not appearing in this table.

APPENDIX I

DATA SOURCES

e, RELWAGE, and SFTSHAR -- U.S. Bureau of the Census, County Business Patterns (various years).

RELATTR -- The source for the numerator was U.S. Bureau of the Census, County Business Patterns (various years). These figures were deflated using a distance matrix supplied by the U.S. Bureau of Economic Analysis.

GRANTS, GUARLNS, DIRLNS, AND TARGET -- We collected the numerator from agency computer files. The source for the denominator was U.S. Department of Commerce, Survey of Current Business (various years).

OTHRFED -- The source of the numerator was Community Services Administration, Geographical Distribution of Federal Funds (various years). The source of the denominator was U.S. Department of Commerce, Survey of Current Business (various years).

GOVEMP -- The source of the numerator was U.S. Bureau of the Census, Public Employment, Series GE, No. 1 (various years). The source of the denominator was U.S. Bureau of the Census, Statistical Abstract of the United States: 1980.

METRO, UNEMP -- U.S. Bureau of the Census, Statistical Abstract of the United States: 1980.

PCTSUN -- U.S. Bureau of the Census, County and City Data Book, 1977.

DUMNV, DUMWY -- 0, 1 dummy variables.

BUSCOND -- The source of the numerator was U.S. Bureau of the Census, State Government Tax Collections, Series GF, No. 1 (various years). The source for the denominator was U.S. Department of Commerce, Survey of Current Business (various years).

HSRENT -- The numerator was constructed from data in U.S Bureau of the Census and Department of Housing and Urban Development Annual Housing Survey, Series H-170 (various years), (Part B, Housing Characteristics for Selected Metropolitan Areas); and CPI component data on residential rent supplied by the Bureau of Labor Statistics. The source for the denominator was U.S. Bureau of the Census, County Business Patterns (various years).

PROGRAMS SELECTED

Our review encompassed economic development programs in the following departments, agencies, and commissions:

Farmers Home Administration (FmHA)
Rural Electrification Administration (REA)
Regional Planning Commissions (RPC)
Economic Development Administration (EDA)
Office of Minority Business Enterprise (OMBE)
Small Business Administration (SBA)
Community Services Administration (CSA)
Department of Housing and Urban Development (HUD)

The major criterion for selecting programs was the presence of a legislative objective to create jobs. All the programs contained this objective, with one exception. Programs in the Rural Electrification Administration were included, even though they did not meet the criterion, because it was deemed that they were extremely important in fostering rural economic development. Several programs had other legislative objectives besides job creation, but we did not evaluate the programs on their other objectives.

Table 18

Total Obligations and Disbursements for Economic Development Programs Between FY 1969-78 Used in Our Survey[a] (in billions of constant 1972 $)

	Disbursements	Obligations
Grants		
Public works	$ 4.3	$ 8.9
Community development	.1	9.2
Business development	.1	.1
Block	7.6	8.0
Technical assistance, planning, and demonstration projects	.5	1.3
Direct loans		
Community development	10.7	17.4
Business development	2.1	2.9
Loan guarantees		
Community development	0[b]	.8
Business development	2.2	12.7
Total	$27.6	$61.3

[a]Some agencies were not able to provide us with data (particularly on disbursements) going back to 1969.

[b]No disbursement occurs on a loan guarantee unless there is a default.

Table 19

Economic Development Grant, Loan, and Loan Guarantee Programs Between FY 1969-78 Used in Our Survey[a]
(in millions of constant 1972 $)

GRANTS

PUBLIC WORKS

Agency	Program name	Disbursements	Obligations
ARC	Appalachian Regional Development	$ 0.65	$ 1.02
ARC	Appalachian Development Highway System	-[b]	1,453.78
ARC	Appalachian Special Transportation Related Planning, Research and Demonstration Program	-	2.29
EDA	Local Public Works Program	2,466.11	4,319.22
EDA	Grants and Loans for Public Works and Development Facilities	1,270.02	1,297.42
EDA	Public Works Impact Projects	154.86	207.98
FmHA	Water and Waste Disposal Systems for Rural Communities	402.90	861.73
HUD	Basic Water and Sewer Facilities Grants	-	562.05
HUD	Neighborhood Facilities Grant	-	165.29
HUD	New Communities Supplementary Grants	-	49.24

COMMUNITY DEVELOPMENT

Agency	Program name	Disbursements	Obligations
ARC	Appalachian Supplements to Federal Grant-in-Aid (Community Development)	-	$ 105.42
ARC	Appalachian Housing Assistance		8.17
ARC	Appalachian Local Development District Assistance	-	22.76
ARC	Appalachian Mine Area Restoration	-	1.96

[a]Some agencies were not able to provide us with data going back to 1969.

[b]A dash (-) indicates the agency could not provide annual disbursement data on a county basis.

Agency	Program name	Disbursements	Obligations
ARC	Appalachian Vocational and Other Education Facilities and Operations	-	$ 74.72
CSA	Community Action	-	237.01
CSA	Community Economic Development	-	160.09
HUD	New Communities Supplementary Grants	-	4.79
HUD	Model Cities Supplementary Grants	-	2,237.75
HUD	Urban Renewal Projects	-	1,794.06
HUD	Urban Renewal Demonstration Grants	$ 98.60	4,505.78
RC	Four Corners Supplements to Federal Grant-in-Aid	21.03	26.43

BUSINESS DEVELOPMENT

Agency	Program name	Disbursements	Obligations
ARC	Appalachian Regional Development	$ 2.52	$ 4.46
FmHA	Industrial Development Grant	26.94	37.72
HUD	Urban Development Action Grants	38.02	66.61
OMBE	Minority Business Development--Management and Technical Assistance	-	23.42

BLOCK GRANTS

Agency	Program name	Disbursements	Obligations
HUD	Community Development Block Grants, Large Cities	$6,888.52	$ 7,181.85
HUD	Community Development Block Grants, Small Cities	750.71	785.55
HUD	Community Development Block Grants, Indian Set Asides	5.27	6.01

TECHNICAL ASSISTANCE, PLANNING GRANTS, DEMONSTRATION PROJECTS, AND OTHER

Agency	Program name	Disbursements	Obligations
ARC	Appalachian Housing Technical Assistance	-	$ 2.96
ARC	Appalachian State Research, Technical Assistance, and Demonstration Projects	-	33.45
ARC	Other ARC Programs	-	107.36
EDA	Support for Planning Organizations	$ 55.62	55.89
EDA	Technical Assistance	120.06	132.92
EDA	State and Local Economic Development Planning	40.98	47.71
EDA	District Operational Asst.	2.34	2.78
EDA	Other EDA programs	245.87	427.65
FmHA	Comprehensive Areawide Water and Sewer Planning Grants	-	4.52
FmHA	Area Development Assistance Planning Grants	-	3.32
HUD	Comprehensive Planning Asst., Section 701	-	482.59
HUD	Model Cities Supplementary Grants	-	0.53
RC	Four Corners Regional Economic Development	-	0.34

DIRECT LOANS

COMMUNITY DEVELOPMENT

Agency	Program name	Disbursements	Obligations
EDA	Other programs	$ 64.01	$ 75.20
EDA	Grants and Loans for Public Works and Development Facilities	99.63	53.59
FmHA	Resource Conservation and Development Loans	6.93	7.62
FmHA	Water and Waste Disposal Systems for Rural Communities	1,787.81	2,538.62
FmHA	Community Facilities Loans	445.40	658.54
HUD	Public Facilities Loans	-	113.91
HUD	Section 312 Rehabilitation Loans	-	378.03
REA	Rural Electrification Loans	6,981.90	11,191.32
REA	Rural Telephone Loans	899.28	1,418.94
REA	Rural Telephone Bank Loans	518.29	919.56

BUSINESS DEVELOPMENT

Agency	Program name	Disbursements	Obligations
EDA	Business Development Asst.	$ 316.64	$ 308.03
EDA	Trade Adjustment Assistance	25.50	42.89
FmHA	Business and Industrial Loans	28.37	32.34
SBA	Displaced Business Loans	225.86	247.85
SBA	Economic Opportunity Loans	442.25	488.54
SBA	Small Business Investment Companies	-	539.51
SBA	Section 7(a) Small Business Loans	747.44	824.27
SBA	State and Local Development Company Loans	303.19	353.33
SBA	Base Closing Economic Injury Loans	19.73	21.31
SBA	Economic Dislocation Loans	9.47	9.82

LOAN GUARANTEES

COMMUNITY DEVELOPMENT

Agency	Program name	Disbursements[c]	Obligations
FmHA	Resource Conservation and Development Loans	-	$ 2.98
FmHA	Water and Waste Disposal Systems for Rural Communities	-	566.81
HUD	New Communities Loan Guarantees	-	279.61

BUSINESS DEVELOPMENT

Agency	Program name	Disbursements[c]	Obligations
EDA	Business Development Asst.	-	$ 214.73
EDA	Trade Adjustment Assistance	-	21.70
FmHA	Business and Industrial Loans	$ 765.51	1,405.58
SBA	Displaced Business Loans	0.36	1.81
SBA	Economic Opportunity Loans	75.50	272.06
SBA	Section 7(a) Small Business Loans	1,340.82	10,676.72
SBA	State and Local Development Company Loans	21.43	128.40
SBA	Base Closing Economic Injury Loans	0.08	0.32

[c]Disbursements for loan guarantees represent a default.

AGENCY COMMENTS

As chapter 1 pointed out, the purpose of this study is to provide the technical reader with a more comprehensive discussion of our analysis than was presented in testimony and the nontechnical report. We initially sent a draft of the nontechnical report to the following agencies: the Council of Economic Advisers; the Office of Management and Budget; the Departments of Housing and Urban Development, Commerce (EDA), Agriculture; and the Small Business Administration. After receiving technical questions, we sent the same agencies, with one exception, a draft of this study. We had already discussed technical aspects of the study with the Council of Economic Advisers and had responded to its technical concerns by analyzing both the short run and the long run effects and by presenting the estimates for grants, loans, and loan guarantees in separate tables. The SBA had no comments on the technical paper.

Many of the agency comments deal with technical limitations in econometric modeling. We have attempted to address specific comments, including suggested re-estimations of the model, by revising the text of the study. The sections of the text where revisions have been made are noted subsequently when we address each agency's specific comments. However, we also address their comments in terms of several broad issues; namely, model specification, data selection, and interpretation of results.

Model specification

A model can be misspecified by omitting relevant variables, by including incorrect variables, or by using the incorrect functional form. However, there is no easy way to know, a priori, whether a model is misspecified. The model we present is consistent with previous labor market models presented in the regional economics literature and has benefited from peer review by government and academic economists. All of the models in the previous literature contain the economic variables of wage rate, income, and unemployment. They also include a set of socioeconomic variables, which depend on the specific data available for the analysis, and a set of program variables. We have included the wage rate, income, and unemployment, along with a shift-share variable, which was not considered in the labor market models that we reviewed. Also, our set of program variables is broader than all the other studies. Our set of socioeconomic variables is based on those used in earlier studies, with a few minor differences. For example, the NBER study (1976) includes migration and median years schooling. As variables intended to capture location decisions, we included percent sunshine, business conditions, and house rents, but we did not include migration because of known data limitations. Although we included a variable to account for the effects of schooling in initial regression runs, this variable was dropped because it was

always insignificant. In another study, Barrows and Bromley (1975) included industry employment growth. We did not include this as an independent variable because we captured this effect through the regional analysis that segmented the sample into high-, medium-, and low-employment growth regions.

Besides constructing a model that reflected the previous literature, we conducted various sensitivity tests of our results. One test was to include and exclude various demand and supply side factors to see if the program effects were altered. This test is similar to what Leamer (1983) suggested and is discussed in chapter 3. The basic results indicate that the coefficients on grants and loan guarantees are not substantially altered.

We also tested for heteroscedasticity using Glejser's (1969) approach, estimated more than one functional form, and introduced interaction terms in the early rounds of model specification. We did not find problems of heteroscedasticity, nor did we find that different functional forms or interaction terms increased explanatory power. What we found when we did these early regression runs was that our major estimation problem, shown by the condition index, was collinearity. Conceptually important variables --the shift-share variable and the wage rate variable--were collinear. Also, the program variables exhibited varying amounts of collinearity. Thus, we attempted to resolve the collinearity problem through the use of ridge regression, as suggested by Belsley, Kuh, and Welsch (1980). The sensitivity tests that we present illustrate a reasonable stability in the coefficient estimates. Certainly more can be done with respect to model specification, but this will always be the case in any modeling effort.

Data selection

A second broad issue raised by the agency comments deals with data selection. One key question is how to define the set of programs representing federal economic development efforts over this period. One agency has suggested that our set was not wide enough because there were other programs that created jobs. Another agency has suggested that our set was too wide, and program "A" should not be compared with program "B" because the objectives of these programs differed.

We selected this set of 56 programs by first looking at how others defined the set of economic development programs. For example, our set is larger than the programs included in the federal budget's functional category of "community and area development."

Our main criterion was whether the legislation listed job creation as an objective of these programs. With the exception of Rural Electrification Administration programs, job creation was a legislated objective in each program we included. However, job creation may not have been the only objective, so we evaluated these programs on only one of perhaps several program objectives. The text in chapter 1 has been revised to clarify this point and the section on economic development assistance in chapter 3 along with appendix III, has been expanded to describe more fully our process of program selection.

Another issue related to data focused on our level of aggregation. We estimated the model after aggregating all the variables to the state level. It has been suggested at various times that we disaggregate to standard metropolitan statistical areas or counties. Although benefits from disaggregation can be offset by less reliable data, which is basically why we used aggregate data, we view this suggestion as an area for further research. We found regional differences when we analyzed the state-level data by employment growth, and further results may be obtained by analyzing a more disaggregated set of observations.

Interpretation of results

Another broad issue reflected in various agency comments focused on how to interpret the results. Some of the comments offered additional ways to interpret the regression results. We have revised the text where appropriate to account for other plausible interpretations.

On another level, the agency comments expressed both support for developing this method of analysis and skepticism about this model's applicability for policy purposes. Perhaps the draft was not explicit in conveying to policymakers the limitations of any econometric model. We have revised our discussion of the model's limitations. Here we simply want to reemphasize that the model analyzes employment effects during the 1974-78 period. This model cannot be used for forecasting nor can the estimated effects be assumed to occur in the current economic climate. The model does illustrate that it is possible to develop a uniform methodology to analyze the employment effects of federal programs. More current data would be needed to analyze programs in the 1980's.

APPENDIX V

UNITED STATES DEPARTMENT OF COMMERCE
Economic Development Administration
Washington, D C 20230

NOV 8 1983

Mr. Arthur J. Corazzini
Acting Director
Program Analysis Division
U.S. General Accounting Office
Washington, D.C. 20548

Dear Mr. Corazzini:

Thank you for your letter to Secretary Baldrige regarding technical documentation supporting your report, "The Effectiveness of Federal Economic Development Programs."

We have reviewed the documentation and discussed it with Charles Vehorn of your staff. In general, we support the use of econometric modeling in program evaluation applications. We do believe, however, that GAO's model needs considerable work before use in policy applications.

Our specific objections and recommendations have been relayed in direct discussions with GAO staff and in earlier written comments. Our principle objection is that the model is too aggregative. The model should be constructed on county rather than state data and economic development assistance should be less broadly defined. The model would also benefit from a longer time series.

The Economic Development Administration continues to be interested in the GAO model and how it is used. If any further revisions are made, we would like to have the opportunity to again review the report.

Sincerely,

Carlos C. Campbell
Assistant Secretary
for Economic Development

[GAO COMMENT: We agree with the comment that the next step in this exercise would be to disaggregate the model to a level of observation below the state-level. Perhaps we would be able to pick up differential effects, as suggested when the state-level data were analyzed in terms of high-, medium-, and low-employment growth. However, the data are less reliable at the county level, as a result, any benefit from disaggregation could be offset by increased errors in measurement.

We also agree that the model would benefit from a longer time series, but 1978 was the most recent year that consistent data could be provided from all the agencies we reviewed.

In terms of policy applications, we have already noted that the model analyzes a particular set of programs during the 1974-78 period. We have cautioned the reader that the model was not designed to make forecasts and the results could be different under a different set of economic conditions.]

APPENDIX VI

DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT
WASHINGTON D C 20410

OFFICE OF THE ASSISTANT SECRETARY
FOR POLICY DEVELOPMENT AND RESEARCH

November 10, 1983

IN REPLY REFER TO

Mr. Arthur J. Corazzini
Acting Director
Program Analysis Division
U.S. General Accounting Office
Washington, DC 20548

Dear Mr. Corazzini:

Secretary Pierce has asked me to respond to your letter of September 28, 1983, which requested comments on the General Accounting Office's technical documentation supporting the draft letter report "The Effectiveness of Federal Economic Development Programs." The technical documentation does clarify some of the issues we raised in our May 6, 1983 comments on the draft letter report. However, on the basis of the technical documentation provided, we have the following comments on model specification and interpretation of results:

Model Specification Issues

1. The model results showing that loan guarantees are more effective at producing jobs than direct loans are puzzling until Tables C-1 and C-2 in the technical paper are examined. This result is clearly a case of omitted variables bias. The loan guarantee obligations are predominantly for programs directed toward business, while the direct loan programs are primarily community development programs, many of them rural. Thus, the observed differences are not due to the assistance mechanism, but instead reflect different program beneficiaries and purposes. These differences also explain the fact that the disparity between short-term and long-term effects is much greater for direct loans, which fund discrete projects rather than business undertakings.

2. Treating unemployment as an exogenous variable results in some confusing conclusions about causality. For example, the negative relationship between unemployment and manufacturing employment reported on page 4-10 of the technical report is more likely due to the fact that unemployment is high where manufacturing employment has fallen, than that manufacturing firms avoid areas of high unemployment. Persistent differentials in regional unemployment rates indicate a resistance of individuals to relocate to find employment. This is as important as unemployment discouraging immigration.

Another example is the discussion on page 4-21 that unemployment is inversely related to change in employment in slow and medium growth States, and insignificant in high growth States. The two extreme causes of unemployment are a loss of jobs and a growth in population more rapid than that in jobs. The first situation indicates distress, and this is what is occurring in the older Northern States. The second situation does not necessarily indicate distress if employment is growing rapidly, and population is outpacing employment only because people are attracted to the area. The second situation describes the West, which has had historically high unemployment even though its economy has generally been healthy. In both cases, the causality does not run from unemployment to employment growth, but from employment growth to unemployment rate. The high employment growth rate does not eliminate unemployment because it only encourages inmigration.

3. Government employment is a component of the demand for employment. Why, then, does it appear only in the supply equation?

4. The BUSCOND variable reflects both the State corporate tax rate and the number and size of corporations in the State. Thus high revenues can reflect both high tax rates and a large corporate sector. This combination probably explains why the BUSCOND value is insignificant. It is a poor proxy for what it is intended to measure.

Interpretation of Results

1. The statement on page 4 of the letter report that "because the model estimates the effect of these programs on the average State for the average year during the 1974-1978 period, we cannot estimate the specific effect of the programs on a specific State" is not correct. The regression procedure fits a regression line. Estimates for an individual State can be produced by substituting the values for the State into the fitted equation. Admittedly, the confidence interval is smaller for the mean values, but estimates can still be produced for any State in the sample.

2. Footnote 6 on page 6 says that the reason fewer jobs are produced in the long run than in the short run is that the increased demand pushes up wages which subsequently reduces demand. Since the increase in demand is projected to be at most 216 jobs in a State, this explanation sounds unlikely. A more likely explanation for why long-term jobs are less than short-term jobs is that a number of the jobs are temporary and have no lasting effect on total employment.

   A test of whether these programs push up wages is found in the reduced form equation which predicts wages. These results are not presented in the paper. Finally, the technical paper finds on page 4-8 that employment is not very sensitive to relative wages. This further weakens the assertion that long run employment is lower due to higher wages.

3. An example of how the model results are governed by the sample used is seen in the conclusion that public works produce more jobs than other types of grants. Examination of Table C-1 in the technical paper shows that the bulk of grant disbursements of "other" grants are block grants (Community Development Block Grants). The funds from this program were rarely used for economic development projects during the study period. It is not surprising that few jobs were created relative to public works projects. Public works projects may indeed produce more jobs than other economic development projects, but this is not a valid test of that hypothesis.

   Furthermore, most of the public works grants are EDA programs. The greater effectiveness of the projects in producing jobs may have something to do with the administrative capabilities and goals of EDA. The model doesn't address these issues.

4. Page 2-5 of the technical paper implies that the model will use distributed lags to measure total person years of employment. However, person years of employment is not addressed when the lags are discussed on page 3-19. Can the estimated lag structure be used to say anything about the degree to which permanent jobs are created?

In conclusion, we feel that the combination of the sample used and omitted variables bias results in conclusions about program effects that are inappropriate. At a minimum, both the letter and technical report should always be distributed together to minimize the misuse of the conclusions.

Sincerely yours,

Benjamin F. Bobo
Acting General Deputy
Assistant Secretary

[GAO COMMENT:

Model specification

1. All of these programs, except those of the Rural Electrification Administration, include job creation as one of their legislative objectives, but the relative importance of the job creation objective may well vary from program to program. The section on economic development assistance in chapter 3 and appendix III have been expanded to clarify our process for selecting programs. Also, the text has been revised to clarify that these programs have more than one purpose.

2. The examples given suggest that two-way causation could be possible. In large-scale regional models that attempt to present a comprehensive description of regional behavior this simultaneity would be modeled. But, as we have said before, the model presented here is a truncated version of these larger models and was not designed to present a comprehensive picture of regional behavior. We have used the unemployment variable here as a general proxy for conditions in the labor market. As such, it is intended to capture such factors as business confidence.

3. Even if government employment were in both equations, it would not change the reduced form estimates of long-run effects.

4. We tried to capture the effective tax rate, rather than the nominal tax rate. Thus, the proxy for business conditions was corporate tax revenue divided by a tax base, here personal income because consistent data on the actual corporate tax base are difficult to obtain. It may not be the best proxy, but we could not find a better one.

Interpretation of results

1. We agree with the comment. The draft has been revised.

2. We have revised that footnote in the letter report to reflect both explanations. However, the projection is not at most 216 jobs. The estimate is a marginal increase for a $500,000 increase in grants.

   The appropriate reduced form wage equation to perform the suggested test is the all industry total regression because this equation captures the broad labor market for an area. In this equation the coefficient on grants is positive.

3. All of the programs, except those in the Rural Electrification Administration, include job creation as one of their objectives. However, it may be that certain programs, during

this period, placed more emphasis on job creation than other programs. The section on economic development assistance in chapter 3 and appendix III have been expanded to clarify our process for selecting programs. Also, we were specifically asked to make this comparison.

4. The text has been clarified to point out that distributed lags are used with respect to the assistance variables.]

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF MANAGEMENT AND BUDGET
WASHINGTON, D C 20503

NOV 0 1 1983

Mr. Arthur Corazzini
Acting Director, Program Analysis Division
U.S. General Accounting Office
Washington, D.C. 20548

Dear Mr. Corazzini:

Thank you for another opportunity to comment on your draft report, "The Effectiveness of Federal Economic Development Programs (PAD-83-42)." We have reviewed the revised document, your responses to our and other agencies' comments on the previous draft, and the technical documentation that you provided. Although some of our questions have been answered, a number still remain and others have arisen.

From a technical standpoint, the model contains some improvements over other regional econometric models (e.g., BEA's NRIES). These improvements, however, make but a small adjustment to an already overly simplified way of approximating complex interactions among national and regional economic systems (which you have further simplified to save computer time, and deal with data and other limitations). Attempting to measure the relative effects on local areas of 56 Federal economic development programs -- which represent a small share of those local economies -- with a technique that still needs considerable refinement to accurately describe the effects of more sizeable economic variables is misleading. Although we are supportive of efforts to improve econometric models for their use in policy analysis, we are cognizant of the difficulties that can arise from using econometrics as proof, especially in cases such as this one. These difficulties are well illustrated in the American Economic Review article, "Let's Take the Con Out of Econometrics," cited in HUD Assistant Secretary Bobo's comments on your first draft.

We also continue to be concerned that although the findings in your report differ dramatically from previous work on this topic, the report does not attempt to:

(1) interpret the model's findings,

(2) contrast them directly with those of other studies, and

(3) show how sensitive the findings are to changes in the specification of the model's equations or the inclusion or omission of certain variables.

These problems put the report's findings into question.

Finally, although some of the model's limitations are noted in your draft letter to Congressman Oberstar, the presentation of the numerical findings for 1974-78 is misleading because it does not take them into account. Specifically, the cost of a job should reflect its cost to society -- the resources that would otherwise have been used by private or other Federal activities that also employ labor. All tabulations of the cost of a job should explicitly take these costs into account.

In summary, these concerns, and those from my letter of May 6, which still have not been satisfactorily addressed (specifically questions 1-3, 6-10), again lead me to suggest that corrections be made before the draft report, "The Effectiveness of Federal Economic Development Programs," is released.

Sincerely,

Joseph R. Wright, Jr.
Deputy Director

[GAO COMMENT: OMB expressed both support and skepticism of econometric modeling for policy purposes. We have pointed out that our model has limited policy application because it does not make forecasts, and the results are only applicable for the 1974-78 period. However, we believe that the limitations to econometric modeling are less severe than those of the case study approach.

1. We believe that the results were interpreted adequately in chapter 4.

2. Although we discussed the previous literature in terms of methodology, we did not discuss the results of previous studies in the draft reviewed by OMB. We have revised the text by including in chapter 4 a section that contrasts our results with previous research.

3. We have revised the text by including in chapter 4 a section on sensitivity testing.

4. We have pointed out that neither the case study approach nor this type of econometric model can deal adequately with societal costs, i.e., the fiscal substitution that occurs when resources are freed up from one government program and allowed to flow into either the private sector or other government programs. As we pointed out, no one that we are aware of has satisfactorily addressed this issue. We are aware of a study by OMB (1979) that used published estimates of the degree of substitution of federal for state and local moneys in project construction activities to reduce gross job estimates. Unfortunately, these displacement rate estimates are not very satisfactory because they are so imprecise ranging from 20 to 80 percent.

5. Finally, we believe that we addressed satisfactorily all the questions OMB raised about the letter report. We tried to find out what OBM specifically meant by saying its concerns were not satisfactorily addressed. However, OMB told us several times that it was unable to meet with us, but would contact us when it could meet.]

United States Department of Agriculture
Economic Research Service
Washington, D C 20250

October 26, 1983

Mr. Arthur J. Corazzini
Acting Director, Program Analysis
United States General Accounting Office
Washington, D.C. 20548

Dear Mr. Corazzini:

We have reviewed a copy of the GAO draft report entitled "The Effectiveness of Federal Economic Development Programs" (PAD-83-42) and offer the following comments. Input was received from the Farmers Home Administration (FmHA), Rural Electrification Administration (REA), Economic Research Service (ERS), and Office of Budget and Program Analysis (OBPA) in developing these comments.

The GAO draft report appears to represent only a first step in satisfying Congressman Oberstar's request to devise a common methodology suitable for evaluating various Federal development programs. Due to the overly simple nature of the GAO econometric model, we believe that the report should be considered only as preliminary and that it would be inappropriate to draw any policy implications from it. A number of specific criticisms are discussed below.

(1) The findings of the report are based on an econometric model. Few, if any, statistical results of the model are presented. The report provides only an outline of the model. From this outline, it would appear that there are serious shortcomings. The GAO model, with a single reduced form equation estimated for each of 10 industry groupings, is not capable of picking up differential job creation effects of Federal programs. For example, the level of Federal funding is included as a variable explaining the demand for labor. However, such funding is also related to the supply of labor, particularly when funding is allocated on the basis of an area's unemployment or population growth. Failure to provide adequately for the relationship between Federal funding and labor supply and demand is likely to have resulted in significantly biased statistical estimates.

Even with the disaggregation of Federal programs by such categories as grants, loans, or loan guarantees, GAO in effect "mixes apples and oranges" due to widely varying program goals (job creation, production of electricity, road building, etc.) and requirements (matching funds, specified use of funds, or block grants). For example, GAO has given insufficient consideration to the broad range of objectives among the various kinds of Federal programs included in the analysis. If the effectiveness of loans and grants in job creation are to be compared, then programs having similar job creation objectives should have been selected.

EDA public works grants are often targeted to specific local areas, with the primary objective of promoting local job creation. On the other hand, the programs of REA and FmHA (with the exception of the FmHA Business and Industry loan guarantee program) have as their major objective improving the quality of life in rural areas by providing the basic human amenities and essential public services, and alleviating health and sanitary hazards. Job creation is likely a resulting benefit, but it is not a stated objective. One would expect that public works programs, by their very nature, would be more effective in job creation than programs in which job creation is not the major objective.

In addition, some of the data used in the GAO model appear to be inaccurate, misleading or improperly footnoted. For example, FmHA grants for water and waste disposal systems for rural communities were improperly categorized under the public works grants section. These should have been listed under the community development grants section. Also, FmHA grants, or for that matter other Federal grants listed in the survey, often cannot or do not cover 100 percent of total project costs. Many times FmHA water and waste disposal loan funds are used with FmHA water and waste disposal grants to make up the full project cost. We wonder whether the survey took this loan-grant mix into account and how the number of jobs created was split between this combination.

(2) In spite of a claim to the contrary in the GAO response to OMB's critique of the March, 1983 draft, the GAO model does not begin to grapple with the crucial substitution issue; namely, what would have happened if specific Federal programs did not occur? The model lacks equations showing the explicit transfer of resources between sectors of the economy which would be necessary for an increase in Federal programs. Ignoring these transfers leads to the erroneous implication that unemployment can be eliminated through a sufficiently large increase in Federal programs.

(3) The model does not account for State and local economic development assistance, in particular, industrial revenue bond (IRB) sales. Total annual issues of IRB's grew from $1.3 billion in 1975 to $7.1 billion in 1979 (see: U.S. Congress, "Small Issue Industrial Revenue Bonds," Congressional Budget Office, Washington, D.C., April, 1981). Thus, State economic development programs could have been a significant factor in explaining "government-induced" employment growth.

(4) The logic of the shift-share framework on which the model is based allows one to use State variation in Federal programs to help explain State variation in employment growth, but it is not clear that anything can be said about the number of jobs created nationally or in particular states as a result of Federal programs. Inappropriate interpretation of the results may explain the apparent finding of extremely low costs for additional jobs.

(5) The report does not provide confidence intervals; point estimates are not sufficient for drawing meaningful policy implications from the statistical results. The report also fails to explain how the long-run responses were obtained.

(6) Corporate profits vary a great deal over the business cycle. Personal income, on the other hand, is much more stable and in any event, is not the base for corporate taxes. Therefore, the ratio of State income tax revenue to personal income is a poor measure of State tax rates (which are readily available!) and cannot be used as a proxy for State business conditions.

(7) Justification is lacking or inadequate for many of the report's results.

-- Why is the estimated cost per job, in particular for jobs due to grants, so low? Taken at face value, the implied cost to eliminate all unemployment in the United States is amazingly small ($20-30 billion).

-- Why do guaranteed loans create more jobs than do direct loans of equal cost? This demonstrates again the dangers of aggregating programs having different purposes and of not accounting for all program costs (such as the opportunity cost of resources transferred to Federal programs).

-- It is implausible that local programs reduce contract construction employment by shifting workers to other industries. This finding should have suggested a need to modify the model. At the least, use of a one-tail test of statistical significance would have produced a conclusion that the construction industry did not respond significantly to Federal loan programs.

In conclusion, we believe that due to its incompleteness, the GAO model does not satisfy the goal of providing a common methodology with which different Federal development programs can be analyzed. Tremendous variation of programs within the categories of grants, loans, and guaranteed loans, plus complex interactions between the Federal, State, and private sectors of the U.S. economy make it impossible to adequately address this issue with a single equation model.

JOHN E. LEE, JR.
for Administrator

[GAO COMMENT: We delivered copies of a draft revised letter report and technical study to the Department of Agriculture's Inspector General. Unfortunately, Agriculture's Economic Research Service (ERS) wrote these comments without having received and reviewed the technical study. After we pointed out that we wanted comments on the study, ERS agreed to give us informal comments. ERS believed that the study answered some but not all of its concerns. Accordingly, ERS decided that it would not revise its initial comments. However, we have attempted to deal with ERS' informal comments by clarifying the text. Here, we are responding to the seven issues raised in ERS' comment. In many instances, our response simply is to point out where the study discusses ERS' concern.

1. Statistical results are presented in the technical document. Even if federal funding were in both equations, it would not change the reduced-form estimates of long-run effects. The sections on economic development assistance in chapter 3 and appendix III discuss our process of program selection. While we selected the categories, the agencies provided us with the data, and we deferred to their judgment in placing their programs into our categories. Also, the econometric model takes the loan-grant mix into account by including separate variables for grants, loans, and loan guarantees.

2. The technical study has been revised to make it clear that we do not address the substitution issue (see chs. 1 and 2). Also, we agree that our marginal estimates do not imply that unemployment could be eliminated through large increases in federal spending (see ch. 4).

3. ERS is correct. Additional analysis may show state IRB's to also have a significant effect; however, we did not attempt to analyze IRB's.

4. We have not discussed job creation nationally or in a particular state. We reported the effect on the average state.

5. This technical document provides the additional statistical information and explains the difficulty in drawing policy conclusions.

6. We tried to capture the effective tax rate, rather than the nominal tax rate. Thus, the proxy for business conditions was corporate tax revenue divided by a tax base, here personal income because consistent data on the actual corporate tax base are difficult to obtain. It may not be the best proxy, but we could not find a better one.

7. The cost per job estimate is a marginal cost based on a small increase in grants. Our study cannot be used to estimate the cost per job for a substantial $20 to 30 billion expansion in the size of these grant programs. We agree that such an expansion is unlikely to eliminate unemployment.

   We have said on p. 9 of the letter report and p. 24 of this study that it is inappropriate to conclude that one type of federal assistance is more cost-effective than another because the cost for each type is different.

   It is also possible that the relative importance of the job creation objective varies systematically between the loan and loan guarantee programs.

   We believe that the statement on contract construction is not implausible. Rosen (1979) argued that construction workers are flexible and have skills that they transfer to other industries.]

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