ESTIMATING IMPACTS OF REGIONAL POLICIES: A REVIEW OF APPLIED RESEARCH METHODS

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FOREWORD

Sharply reduced rates of population and industrial growth have been projected for many of the developed nations in the 1980s. In economies that rely primarily on market mechanisms to redirect capital and labor from surplus to deficit areas, the problems of adjustment may be slow and socially costly. In the more centralized economies, increasing difficulties in determining investment allocations and inducing sectoral redistributions of a nearly constant or diminishing labor force may arise. The socioeconomic problems that flow from such changes in labor demands and supplies form the contextual background of the Manpower Analysis Task, which is striving to develop methods for analyzing and projecting the impacts of international, national, and regional population dynamics on labor supply, demand, and productivity in the more-developed nations.

As part of the policy-oriented research conducted within the Manpower Analysis Task, this paper gives a careful, critical review of different methods that have been used to assess effects of policies focused on regional labor markets. Following an extensive review of different approaches used in this particular field of policy evaluation, the authors suggest points of departure for future research on the impacts of government intervention on labor market developments.

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ABSTRACT

This paper gives a review of research methods that have been used to estimate the impacts of regional policies. A distinction is made between microstudies and macrostudies, and the pros and cons of different approaches within each of these groups are extensively discussed. The paper concludes with some suggestions for future research in this field.

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1. INTRODUCTION

Eight years ago there was a severe complaint from the Expenditure Committee of the British House of Commons:

> There must be few areas of government expenditure in which so much is spent but so little known about the success of policy. The most our witness could say was that... the situation was better than it would have been without the incentives and controls of some sort of regional policy. Yet no one could say whether this effect was a major or a minor one. (House of Commons Expenditure Committe 1973-74:para.116)

Around the same time, Coleman wrote in a more general sense:

> ...there is no body of methods, no comprehensive methodology, for the study of the impact of public policy... (Coleman, 1975:19)

Today, the situation is quite different. In several countries a considerable amount of research has been carried out to investigate the possible impacts of regional policies. Different types of methodological approaches have been used for this purpose. As a result, quantitative estimates of several kinds of impacts of regional policy instruments have been obtained. There have even been attempts to present a more or

less comprehensive assessment of all kinds of costs and benefits, resulting from the application of complete regional policy programs.

The last decade has thus been characterized by rather active, traditional, and innovative regional policy research. After such intensive activity it now seems worthwhile to lean back for a while and assess the contributions of all these assessment activities. There have been other recent surveys in this field, but these have been less comprehensive focusing mainly on British contributions (compare Ashcroft, 1979; Marquand, 1980; and Schofield, 1979; comparative studies which cover several European countries are Ashcroft, 1980; Nicol, 1980, and Vanhove and Klaassen, 1980). Another distinction is that we want to focus on the pros and cons of the results obtained. (Other useful references discussing similar issues in different fields of policy making are Hübler, 1980 and Lund, 1976). This may enable us to identify on the one hand weak points that should be prevented in future research, and on the other hand promising approaches that could yield fruitful new applicants. Furthermore, we want to investigate if the research methodology is at all sound enough to produce reliable conclusions about the quantitative impacts of policy actions.

Since our concern will be so much with methods, the reader will not find an enumeration of all relevant contributions and a survey of their main results. Such information can be better found in the references mentioned above, while in addition the bibliography in Allen (1978) presents a good entry to relevant research in several European countries. In our discussion we shall scarcely refer to certain applications as typical examples of certain research approaches.

There are many ways in which government may affect the situation in regions, which may ask for different types of evaluation techniques. In this paper we limit ourselves to only part of these government interventions. First, we only consider those government activities aimed at directly affecting the

regional *economic* situation. This implies that we shall not deal with noneconomic regional policies (such as housing, education, and welfare) and not with national policies which may have a regionally differentiated impact. Second, activities of *national* governments are the subject of the evaluation studies reviewed here. Specific actions of local and regional governments (e.g., in land use and transport planning) are beyond the scope of this paper.

In the discussion to follow we shall first deal with some relevant notions and topics which relate mainly to policy evaluation in general. After these introductory remarks, we turn to a more specific analysis of the different methods that have been used for the estimation of impacts of regional policies.

2. SOME GENERAL NOTIONS OF POLICY EVALUATION

The purpose of policy evaluation is to acquire insight into the consequences that are attributed to certain policies. Such consequence can be intended or not intended by the designers of policy programs; which consequences are investigated depends upon the specific perspectives chosen. The impacts of regional economic policy may be evaluated from at least three separate perspectives (see also Haveman, 1976 and Schofield, 1979): first, its impact on the regional distribution of goal variables such as economic welfare, economic activities, and labor market discrepancies; second, its contribution to goal variables of national policy, like national output and full employment; and third, the financial impact on the National Treasury which is of special interest to policy makers who have to choose among alternative uses of scarce resources. In the following review it will appear that the first perspective has been the most frequently used by policy evaluators in this field. It will be clear that the selection of a certain perspective is an important first step in the evaluation procedure, since this selection determines the variables on which policy assessment will focus (the impact variables or outcome variables).

Insight into consequences, which can be attributed to certain policy instruments, requires an estimation of the situation which would have existed in the absence of these instruments: the policy-off or counterfactual situation. It can be defended that such insight can never be obtained in a reliable way and that therefore a more modest approach is required which focuses on impact estimates of marginal changes in the application of policy instruments. But even the estimation of such marginal effects is not an easy matter. It requires insight into the process that links the instrument variables with the ultimate policy-goal variables; this process is, in general, a very complicated one. It may contain an easily detectable direct relationship between instrument and impact variables, but at the same time less perceptible interrelationships via other intervening variables. Hence, not only direct, primary impacts but also indirect impacts have to be identified.

The difficulties involved in such an identification may be illustrated with an example. Suppose one is interested in the impacts of regional investment incentives on the level of employment in the different regions. What sorts of direct and indirect impacts are to be expected in this case? Partly following Ohlsson (1979) we can first mention several ways in which the employment situation in a certain region may be positively affected:

- a supported investment project may imply the creation of additional jobs--a primary and direct effect;
- the increased activity of supported plants may cause an increased regional demand for intermediate goods, with possibly a related expansion of jobs--an indirect, intra-regional effect;
- a similar effect may occur if more intermediate goods are delivered to supported plants in other regions--an indirect, interregional effect;
- the extension of employment opportunites may lead to an increase in household income and subsequently to extra induced demand for regional goods and services with possibly related employment effects--an induced household consumption effect;

- the enlarged size of economic activity in the region may form an attractive location factor for additional new activities, which creates other indirect (multiplier) effects.

However, there are also a number of possible negative impacts which may occur:

- investment subsidies may stimulate the use of advanced labor saving techniques or a concentration on capital intensive products with possibly a net loss of jobs in the supported plant--a substitution effect;
- support to certain firms may create worse perspectives for their competitors--this competition effect may cause job losses in the same region or elsewhere;
- negative competition effects may be amplified, because of reduced demands for intermediate goods and for consumption goods--indirect and induced effects, which may be intra- or interregional;
- if the supply of certain production resources is regionally limited, the expansion of regional economic activity may cause increased competition on factor markets with possibly negative impacts on regional employment.

If the still more interesting question of how investment incentives affect the discrepancies in regional labor markets is posed, additional effects have to be included relating to the supply side of the labor market. Given that it is (registered) unemployment which figures highly in the mind of policy makers yet a very effective incentive policy can be consistent with little improvement in the regional unemployment situation. Since the additional demand for labor may have a number of supply side effects such as increasing the participation rate (i.e., reducing hidden unemployment), reducing "forced" outmigration, and stimulating inmigration of workers.

This impression of the possible impacts of certain policy measure also makes it clear how important it is to indicate explicitly the *time perspective* one has in mind when estimating

effects. Some consequences of policy will be observable more or less instantly, while with respect to others there may be considerable time lags between policy stimulus and response. The picture of total effects may therefore differ substantially between a short-run and a long-run analysis.

The complexity that can be expected in the search for policy impacts makes it very understandable that most evaluators decided to limit themselves to the analysis of certain impacts on specific variables. Such analysis can of course only result in a partial assessment of policy impacts. With respect to the point of time at which a certain policy is implemented, such assessments have been undertaken:

- either before implementation of the policy--that is, *ex ante* evaluation of possible impacts of a program, assuming, among other things, that there are no changes in uncontrolled exogenous variables and that ideas on the working of the economic system are correct;
- or during or after the program implementation--that is, *ex post* evaluation, which may be a summative evaluation of impacts when the program is completely finished or a formative evaluation when the program is being implemented in order to produce feedback for better implementation (see also Poister, 1978).

Reliable *ex ante* evaluation requires good insight into the working of the economic system, including the role played by government intervention. Thus *ex post* evaluations are frequently a necessary first step for gaining insight to make an *ex ante* assessment possible. Most of our discussion will therefore relate to problems arising in *ex post* evaluation.

A special problem in such an *ex post* evaluation arises from the measurement of the relevant variables. It is especially cumbersome to obtain meaningful operational indicators of the intensity of use of the various policy instruments. The kind of difficulties one frequently encounters may be illustrated with some examples. Some studies have attempted to estimate impacts of total

government expenditures for regions. Yet, reliable estimates of the spatial allocation of the national budget are in most cases not available, while the availability of such figures still would not reveal where impacts could be expected because of all kinds of spatial spillovers (compare Vernez, 1980). Another example relates to economic infrastructure. Public investment in such infrastructure has been used in many countries as one of the most important instruments of regional economic policy. The spatial dispersion of government activity in this field is not revealed by public infrastructural programs for assisted areas, since all types of infrastructural investments are also undertaken in other regions, not under the heading of "regional policy". An indication of policy intensity thus requires a very careful examination of different parts of the national budget. A third example relates to another very important policy instrument: investment incentives. Such incentives have been used in many different forms (compare Allen et al., 1979, for an informative discussion). Some of these incentives are, however, not known at all, e.g., when they form part of package deals with large firms. (In the Netherlands special prices for the supply of energy have been treated as top secret in the past.) For other incentives their intensity is difficult to determine since this frequently depends very much on certain characteristics of the firm or the project. This is especially the case with incentives which are not automatically but discretionarily awarded (e.g., the Selective Financial Assistance in Britain).

It is not only the measurement of the instrument variables that may be cumbersome, however. Also the information on the impact variables may be rather poor in practice. Complete and reliable information on variables like regional investment and employment is frequently not available (e.g., many studies estimate employment by counting the numbers of persons employed, without any adjustment for the fact that average working time is different among regions and time periods). Official figures on the incidence of unemployment often reveal only part of the existing job shortages, because of the existence of hidden unemployment.

From the previous discussion, it can be concluded that before any quantitative *ex post* assessment of policy is made, a careful execution of a number of important preparatory steps is required. These include:

1. Selection of the policy instruments and the variables on which impacts will be assessed.
2. A careful qualitative analysis of the working of the instruments, such as the conditions for their application, the *a priori* expected incidence at the micro level, etc.
3. The development of a theoretical-qualitative framework which indicates the crucial linkages between instrument and impact variables, including direct and indirect effects within a certain time perspective.
4. Selection of those impacts that are expected to be revealed by the chosen research method, and an indication of the effects that are excluded from the assessment and their possible importance.
5. Selection of quantitative indicators for the variables included in the analysis, and an *a priori* evaluation of possible effects caused by the incomplete mapping between variables and indicators.
6. Collection of the required data, with again a careful analysis of their shortcomings and possible consequences of these for the ultimate results.

In the remainder of this paper we shall restrict ourselves to a further discussion of the fourth step: the choice of the research method and its implications for the results. In the next section we shall first make some general remarks on this point, which may provide a framework against which the attractiveness of alternative research designs discussed later in this paper can be judged.

3. RESEARCH DESIGN AND THE VALIDITY OF THE RESULTS

The choice of a certain research design has important implications for the validity of the results to be obtained. First, any reliable evaluation study will attempt to make it plausible that an observed association between instrument and impact variables is indeed a causal one, and not that some factor other than the policy instrument has caused the observed impact. This is commonly referred to as the *internal validity* of the results. Second, in some cases one would like to use the results for deriving recommendations for policy making in different settings. In this case it has to be certain that the research findings can indeed be extended to other settings, and that they are not completely specific for the investigated case. This is called the *external validity* of the results. Both types of validity are in practice threatened in many ways, and the choice of an appropriate research design will be governed by the desire to minimize certain "threats to validity." Following Poister (1978) we can identify the most common threats to validity, that arise in many different evaluation studies.

The internal validity of the results may be jeopardized by the following difficulties:

1. The impact variables may also be influenced by other nonpolicy variables and by changes that occur in the behavior of the units to which policy is directed and that are independent of the policy taken.
2. Random components in the measurement of instruments and outcomes and in the behavior of the micro units have to be separated from the more systematic relationships one is interested in.
3. If impacts are measured by investigating outcomes for target and comparison groups, one is not always sure that the groups are fully equivalent in terms of all the factors that might have influenced the final outcomes. For example: attrition rates and patterns of maturation may differ; repeated testing of target groups may affect the measurement of their reaction.

These difficulties all relate to the problem of how to obtain a reliable approximation of the policy-off situation, with which the policy-on situation may be compared.

The external validity of the results may be threatened by the following difficulties:

1. The specific situation in which a program is implemented, like the point in time, the location in space, and the newness of the program, frequently makes the outcomes not very well transferable to other situations.
2. In many cases a mixture of policy instruments is implemented at once and produces the observed outcomes, while it may be difficult to find other situations in which precisely the same package can be implemented.
3. The evaluation results may be affected by some kind of reaction or reponse of the units which have been tested or observed, which may not exist in other settings. (For example, respondents may have certain perceptions of the likely consequences of alternative outcomes of an evaluation.)
4. The cases that are subject to evaluation will not always be representative for other cases to which the same instruments could be applied. For example, volunteer participants in a new experiment may be more motivated than participants in repeated applications of the program.

The points illustrate what kind of problems have to be solved to derive reliable statements in *ex ante* policy evaluation studies, where transferability of results from past experiences to new situations is an important condition.

To circumvent all threats to validity is simply impossible in social sciences, where a complete understanding of all relevant processes is utopic. The choice of a certain research methodology is therefore directed towards the elimination of certain of these difficulties. A first possible research strategy is the setting up of a *controlled experiment*, which allows the researcher to have

full control over the different influences on the outcomes. This may create good possibilities to avoid most of the threats to internal validity. However, such controlled experiments are seldom realizable in the field of regional policy. A second strategy is formed by *quasiexperimental* research. In this strategy, the researcher can manipulate the data collection procedures, so as to separate the impact of relevant situational (nonpolicy) variables from that of the policy instruments, using some kind of statistical technique. However, in using this approach, one is generally not sure that real causal effects have been identified: the internal validity of the results is often dubious. A third strategy is followed in studies which do not worry very much about separate policy and nonpolicy forces. Using a completely *nonexperimental design* the investigation is restricted to the simple observation of outcome variables, e.g., before and after policy implementation or among different noncomparable groups. This strategy can hardly be considered a serious attempt at policy evaluation, since the validity of the results will be threatened in all possible ways.

The remainder of this paper will be devoted to a discussion of experimental, nonexperimental, and quasiexperimental evaluation research for regional policies. We shall subdivide the studies according to the level of aggregation of the utilized data. A first group is formed by *microstudies*, in which data collected for microunits are the basis for the impact assessment. A second group consists of *macrostudies*, which utilize aggregated data to discern policy impacts.

4. MICROSTUDIES OF REGIONAL POLICY IMPACTS

Microstudies directly investigate the behavior of units likely to be affected by a policy program. The data may be collected through direct observation, questionnaires, and interviews. Several statistical techniques can be used to derive quantitative impact estimates.

In principle, *controlled experimentation* is possible at the micro level. Using completely equivalent treatment and control

groups, one attempts to isolate the impact of treatment on the outcomes of the treated group, as compared with the behavior of the control groups. Measurement of the impacts is based on the observation of outcome variables for the treatment and control groups, before and after the policy implementation. To obtain equivalent control groups, matched samples are frequently used, in which pairs of individuals with similar characteristics for relevant independent variables are selected. As stated before, this research strategy is in principle attractive to isolate causal relations between instruments and outcomes, although it does not provide much insight into the precise channels through which such effects occur. Because of this advantage, controlled experiments have been used in many areas of policy evaluation, although with the exception of regional economic policy. Applications can be found for welfare policies, educational policies, and energy policies. An interesting application is a large scale experimental analysis of several impacts of housing policies in the USA, undertaken by the RAND Corporation. This experiment covers a rather long time period (10 years) so that long term effects can also be investigated (compare Rasmussen, 1980, for more details).

Apart from the difficulty of transfering results to other situations, there are at least two problems with this method which may have prevented a wider use for regional policy evaluation:

1. Application of this method will reveal only those impacts that are experienced by the target group during the period of experimentation. Impacts which occur later, or which are felt by other groups, will not be observed. Furthermore, if certain impacts were not expected *a priori*, the measurement method may fail to register them appropriately.

2. In many situations it will be impossible to find adequate control groups. This is particularly true for most regional policy instruments. First, participation in most programs is voluntary so that participants and nonparticipants constitute incomparable groups by

definition. Second, the rules for participation contain restrictions on the geographical location, implying that control groups have to be sought in the same region, which will again be impossible. It has to be noted however, that this difficulty might be weakened if the policy evaluation were to have more influence on the policy implementation, so that some discrimination in application could be attained in order to obtain control groups.

Since the second drawback could be avoided in certain policy programs, and since the first drawback may be less serious if we look for very specific impacts, the use of controlled experiments seems still to be an attractive strategy for the evaluation of certain policy instruments (i.e., those which can be implemented so that the evaluation has some control on the stimulus).

Another, and more frequent use of microstudies for policy evaluation is in a *non- or quasiexperimental* setting. A non-experimental microstudy would use questionnaires or interviews to obtain information on the development of the impact variables at the microlevel after the implementation of a certain policy, without any serious attempt to control for nonpolicy influences. This approach may be quite valid, if the collection of essential information depends upon such surveys. For example, in Bartels and Wijma (1980) a survey among relocated government offices was used to assess some of the direct impacts of relocation on the regions of destination. In Poolman and Wever (1978) it could be concluded from a survey of the assisted firms in a certain region that employment in these firms had developed less favorably than employment in the nonassisted firms, in a given time period. Neither of these examples makes a reliable estimation of direct and indirect policy impacts possible, but still both yield useful information for further evaluation attempts. In a quasiexperimental approach, however, more information is collected so that it may be attempted to identify separate influences on the impact variables. This can be operationalized by means of an *ex post* survey, with sufficient variation in the data to detect different

influences; a panel study in which the same units are surveyed before and after policy intervention; or a retrospective survey in which questions are asked about the situation before and after the policy intervention.

Instrument variables may enter in two different ways in such surveys. The first possibility is that they are not explicitly incorporated, but act via certain intervening variables. These intervening variables are then directly influenced by the instruments and have a direct effect on the impact variable. There have been, for example, studies which used microdata to estimate the relation between personal disposable income and the amount of hours worked for members of the labor force. The income variable may be considered as an intervening variable for policy instruments such as income taxes, minimum wages, etc. Hence, the estimated relationship would allow an assessment of policy effects. (Certain drawbacks of the use of intervening variables for such purposes are discussed below under the macrostudies.) A second possibility is to incorporate the instrument variables explicitly in the questionnaire. For example, it is asked if the eligibility for a relocation subsidy has contributed to the decision to relocate.

It has to be noted that most microstudies which provide insight into possible impacts of regional policies have not been initially concerned with assessment of policy impacts. Instead they focus more generally on identifying the forces and factors behind particular decisions, in particular, the decisions of private firms and individual households to move to another location. The information obtained by such surveys makes it also possible to comment on the role of policy. But the design of the studies, and the way in which the data have been examined, frequently only allow conclusions on the *relative* influence of policy as compared with other factors. Quantitative statements on policy impacts are then not easily derived. This is clear in studies of private firms focusing on their location and investment behavior and their performance at different locations, and in studies of location behavior of individual households.

Although a quantitative assessment of policy impacts is seldom derived from these survey studies they still have provided very valuable insight which is indispensable for the analysis of policy impacts. We mention a number of useful contributions from these microstudies. (The remaining part of this section is based to an important extent on the discussion in Nicol and McKean, 1980.)

1. Microstudies yield information on the perception by individual decision makers of the importance of relevant factors influencing their decision. This at least provides some qualitative information on the possible roles of policy instruments, and also insight which may be useful for the design of macrostudies. As an example we mention studies of the location behavior of private firms. These have revealed, in countries like the Netherlands (see Bartels and van Duijn, 1981, for a summary) and West Germany (see Krist, 1980, for a summary), that the availability of building sites, adequate labor supply, and good traffic conditions have been more important location factors than the possibility of obtaining investment incentives. However in Marquand (1980) British studies are discussed which place regional incentives in the second place, behind labor availability.

2. Some of these studies have asked hypothetical questions, e.g.,"Would you have undertaken this investment project if no incentives had been available?" as an attempt to get some indication of the counterfactual situation. Despite the fact that he answers to such questions are also hypothetical, they have been used to derive quantitative assessments of possible policy impacts. Interesting examples are Calame (1980), where results are presented which have been derived from questions about wage subsidy programs in various countries; Beaumont (1979), where the impacts of migration incentives are investigated for labor migrants; Poolman and Wever (1978), where the impacts of investment incentives on the location decision of firms are considered; and Moore and Rhodes (1976a), where interviews are used to reveal various effects of a regional employment premium as perceived by senior executives in firms.

3. The results may contribute to a better understanding of the decision process, which is important for the design of macrostudies. For example, it the movement of households or firms has to be studied with aggregate data, it is important to know whether the decision process which leads to a move can be broken down into two steps (first: decision to move; second: choice of destination) or is essentially an interdependent process. Several microstudies of the location behavior of firms have concluded that a two step procedure seems to be a good approximation, in which the decision to move is not affected much by government incentives while the choice of the location may be more sensitive to policy instruments. But in countries with disincentive policies this separation seems artificial, since such policies obviously act to make investment, movement, and location decisions interdependent.

4. Microstudies contain information that may be very helpful in the measurement of the intensity of a certain policy instrument, since such measurement depends on how policy instruments enter the decision process of microunits. For example, investment and labor incentives are not only treated as a reduction in factor prices, but also as an easy contribution to profits. It is important to know to what extent such alternative uses occur. Microstudies have also suggested that firms generally do not apply discounted cash flow techniques in investment appraisal, so that there seems no reason to measure the strength of investment incentives in terms of their discounted value.

5. Certain important relationships will be better understood with microdata, such as the length of the time lag between investment and employment creation.

6. If microstudies are used to investigate comparative costs, performance, and satisfaction of firms after a move to development areas, they may yield information on the possible resource costs of regional policy.

However, there are also some serious difficulties with this survey research which may limit its usefulness for impact analyses. Some of the difficulties originate from the survey's

character, others from the particular demands stemming from the evaluation purpose.

1. A major problem can arise in relation to the representativeness of the sample. Many microstudies seem to have been designed just to collect information for a particular group of units, without much attention to the sample selection.

2. If personal interviews are used it cannot always be avoided that the interviewer has some influence on the answers of the respondents.

3. There may be several difficulties with the interpretation of the answers given by respondents. First, there may be "respondents effects", in the sense that the information given by respondents differs from their real behavior. In the case of policy questions respondents might say that policy was important if this would influence the future availability of incentives. Second, the way in which the decision was made may be such that it could not easily be incorporated in the structure of the questionnaire. Thus, for example, when asked to rank relevant factors in order of importance, the interdependency of these factors may cause the respondent to opt for one main cause when, in fact, a variety of features led to a general concensus for a particular decision. Third, the problem of *ex post* rationalization permeates survey research. Thus, a different rationale may be subsequently attributed to decisions which conceals the real motives in the decision process. Fourth, in some microstudies it is impossible to contact the persons who took the decision in question. This is especially a problem when the time between the decision and the study is such that the relevant person is no longer with the firm or the household.

4. An inherent problem of all survey research is to design a questionnaire or interview schedule in such a way as to obtain the information required without influencing the answers given by the respondents. In policy evaluation it is, for example, important to have questions in sufficient detail on the policy instruments included in the questionnaire.

5. As with the controlled experiments, this quasiexperimental research will reveal only part of the possible impacts, i.e., as far as they relate to the respondents participating in

the survey and to the time period for which the questions have been formulated. This also implies that the results will be rather specific and not easily generalizable.

6. It may be difficult to isolate effects of individual factors from the information given by the respondents, although statistical techniques could be used to solve this problem to some extent.

These difficulties with survey research may make a macro approach more attractive in some circumstances. But also macrostudies are not without their inherent problems, as will be demonstrated below.

5. MACROSTUDIES WITHOUT AN EXPLICIT MODEL

Macrostudies use aggregate data to reveal policy impacts, and hence only a non- or quasiexperimental approach can be followed. In this section we discuss studies that can be called nonexperimental since they do not attempt to control for the influence of situational variables. In the next two sections, studies are discussed in which more serious attempts have been undertaken to separate the policy and nonpolicy impacts, by means of the formulation of some macro model which is tested by using certain statistical techniques. The hypothesized model may be a simple one-equation model (Section 6) or a more comprehensive multi-equation model (Section 7).

The nonexperimental macrostudies are very simple attempts to reveal possible policy impacts. The following types of studies can be considered as belonging to this category.

1. Studies which employ a *simple monitoring* of relevant outcomes after policy implementation for the affected regions alone, or compared with the situation elsewhere. Policy makers frequently use figures on employment estimates by individual firms when applying for location aids, participants in certain policy programs, and goal variables like unemployment and migration to suggest a possible impact of their policies. Of

course, such assessments have a very shaky basis. This approach is more defendable if certain consequences of comprehensive policy programs have to be described, e.g., the building of new towns (see Tuppen, 1979 for an example), land colonization schemes (e.g., Bahrin, 1979), etc.

2. Studies which undertake an *international comparison* of experiences with certain policy instruments, by investigating their intensity of use and some outcome variables in different countries. An interesting example is a study which investigates the use and possible impacts of restrictive policy instruments in four European countries (Wettmann, et al., 1979). This study demonstrates that an international comparison may yield insight in the working of policy instruments and in the causes of a variation of their impacts among countries.

3. Studies which derive by *simple time series analysis* a trend for the policy-off observations, and use this to approximate the counterfactual situation in the policy-on period. The gap between this counterfactual and the actual situation is considered as a policy impact. Crucial to this approach is the clear ability to distinguish between periods of no (or passive) application of certain instruments and periods of active policy. But if such periods are relatively short, serious difficulties arise when interpreting them as periods in which a small (passive phase) or a large (active phase) policy impact can be expected, because the existence of time lags may cause the effects to occur in nonpolicy periods (compare also Dessant and Smart, 1977). In addition, one may have reservation in extrapolating a trend from a short policy-off period over a long policy-on period (as is done in Rees and Miall, 1979). If these problems do not exist, and if it can be shown that a distinct gap between the actual and counterfactual situation arose around the time of policy change, some *a priori* support for the contention that the gap is associated with policy is provided. The addition of a new policy instrument to an existing package would be an example where impacts could be revealed in this way (see Moore and Rhodes, 1976a).

There are several ways in which this approach has been operationalized. A first way is to take a simple average policy-off value as representing the counterfactual situation. It should be noted that, where this approach has been used, the estimates thereby derived have been used simply as a check on the results of other, more sophisticated, approaches. (Examples can be found in Marquand, 1980, for investment trends; in Moore and Rhodes, 1976b, and Ashcroft and Taylor, 1977 and 1979, for trends in the movement of firms to development areas, in MacKay, 1979, for the movement of firms, associated employment, and industrial building; and in Martin and Graham, 1980, for trends in personal income.) A second way of implementing this approach is to use the trend in the regional share of some related variable, to estimate the counterfactual values of the impact variable. This idea is further developed in Begg, et al. (1976) where the trend in the ratio of actual to "standardized" investment is extrapolated from the policy-off period, with standardized investment defined as the investment which would have occured had the regional share of investment by industry equaled the regional share in employment. A third way is to use a regression model to fit a trend line through policy-off observations and to project this to give the counterfactual situations. In Recker (1977) a secular and cyclical time trend is estimated to assess policy impacts on employment and investment in German regions. In Frost (1975) a regional employment trend is specified by relating employment by industry in a certain region to employment in other regions, for the policy-off period.

4. Studies which focus on comparisons across regions to estimate the counterfactual situation. Also in this case regional shares for other variables have been used to established expected nonpolicy values for the impact variables, compare, e.g., Hart (1971).

The basic characteristic of all these studies can be described as "measurement without explanation." Since no detailed attention is given to the question to what extent other independent variables may have affected the impact variables, the reliability of the results obtained by such studies is doubtful. More

sophisticated approaches to the separation of policy and nonpolicy influences are therefore required.

6. MACROSTUDIES WITH A ONE-EQUATION MODEL

Most macrostudies of regional policy impacts have incorporated ideas about the working of the regional economic system, to formualte a one-equation model which can be used to estimate the impacts. Such a model can then be considered as a kind of simple, reduced form model, representing a much more complicated structural model which is then, however, not specified. In such a reduced form model the policy instruments may or may not be explicitly included among the independent variables. This distinction is used to put the relevant studies in two different groups, to be discussed in 6.1 and 6.2 respectively. Drawbacks of the single equation modles will be discussed in 6.3.

6.1 No Explicit Role for the Policy Instruments

Studies in this group have in common that attention is directed towards modeling the impact of situational variables and treating the policy impacts mainly as a residual. This kind of approach has been primarily justified by the acknowledgment of the serious difficulties associated with deriving aggregate measures of policy strength and with incorporating these as separate independent variables in statistical analyses (see 6.2).

A first approach which requires explicit discussion is the adoption of a deterministic model to account for possible effects of the regional economic structure on the overall regional development. In a time series context, national sectoral growth rates are applied to the regional structure in a certain base year, to define the expected counterfactual situation, i.e.:

$$\hat{E}_{rt} = \sum_i E_{iro} \frac{E_{it}}{E_{io}} \qquad (1)$$

where

$\hat{E}_{rt}$ = the expected value of variable E in region r, year t

E_{iro} = the value of variable E in sector i in region r in base year o

E_{it}, E_{io} = the national values of E in sector i at times t and o

The divergence between actual and counterfactual regional development, $E_{rt} - \hat{E}_{rt}$, will contain effects of a change in economic structure between base and terminal years, and region specific components in sectoral growth. Both types of influences may have been partly caused by influences of policy. A change in the divergence $E_{rt} - \hat{E}_{rt}$ may in certain circumstances be interpreted as an indication of policy impacts, i.e., if only regional policy, among all the factors that could have influenced performance, could have operated in a manner (in terms of timing and direction) compatible with the observed change.

Support for the contention that the above procedure identifies the policy effect requires that, in the policy-off period, the divergence is close to zero, i.e., $E_{rt} - \hat{E}_{rt} \simeq 0$, and that it increases around the time when policy moved into its active phase, thereby providing *a priori* support that the emergence of the gap between the actual and counterfactual situation is attributable to policy. If the actual and adjusted series generally do not closely correspond in the policy-off period, the procedure adopted is that of fitting a trend line to the divergence in the policy-off period, e.g., $E_{rt} - \hat{E}_{rt} = f(t)$ or $E_{rt}/\hat{E}_{rt} = g(t)$, which is then projected into the policy-on period and added to the expected series $\hat{E}_{rt}$ to provide an adapted hypothetical policy-off situation, $\hat{\hat{E}}_{rt} = \hat{E}_{rt} + f(t)$ or $\hat{\hat{E}}_{rt} = \hat{E}_{rt}\, g(t)$, with $\hat{E}_{rt}$ defined in (1). This modification rests on the assumption that the unspecified forces operating in the policy-off period continue to act in the same

direction and with the same amplitude as in the policy-on period. Another complication arises from the presence of a possible cyclical component in yearly observations, which may make the detection of structural policy impacts difficult. To eliminate such cyclical influences, the terminal year would have to be chosen so as to be comparable with the base year in terms of business-cycle phase.

This deterministic decomposition approach sometimes referred to as modified shift-share analysis, has found wide application, mainly in British studies. Since Moore and Rhodes used this method to analyze regional employment in their seminal article published in 1973, it has been used in some form by several other researchers. Applications to employment data can be found in MacKay (1976 and 1979), Moore and Rhodes (1973, 1974, and 1976a), Moore, Rhodes, and Tyler (1977), Keeble (1980), and Ohlsson (1980). Investment data have been investigated in Ashcroft (1979), Begg, et al. (1976), Blake (1976), Moore and Rhodes (1973 and 1974), and Rees and Miall (1979), production data in Ohlsson (1980), the movement of industrial firms in MacKay (1979).

The absence of any explicit attention to stochastic elements in this deterministic approach has brought some authors to propose an alternative, second approach. This is a stochastic standardization approach, which allows for the possibility to perform statistical tests on the significance of estimated impacts. The statistical tool is analysis of variance, which has been used in Buck and Atkins (1976a). Their model is

$$g_{ir} E_{iro} = \sum_i a_i D_i E_{iro} + \sum_r b_r D_r E_{iro} + u_{ir} E_{iro} \qquad (2)$$

where

g_{ir} = growth of employment in industry i, region r, in a certain time period

E_{iro} = weight of industry i in region r in base year o

D_i = dummy variable with value 1 for industry i, and 0 in other cases

D_r = dummy variable with value 1 for region r and 0 elsewhere

u_{ir} = error term

The regional component for region r can be calculated as $b_r - \sum_r E_r b_r$, which may be considered as an indication for a policy impact. The advantages ascribed by Buck and Atkins to this approach are the possibility of performing statistical tests and the feature that the policy effect now excludes possible stochastic disturbances. However, the approach has also some important drawbacks. First, it implies that only a general industry-wide regional effect of policy will be identified as a policy impact, while any nonsystematic differential growth--which may have a policy causation--is allocated to the residual term. Second, a change in economic structure caused by policy will not be captured in the impact estimate.

Users of both standardization approaches have been motivated by the desire to use a simple calculation technique, which may reveal most of the direct and indirect effects of policy, as far as these effects relate to the sectors being investigated. There are, however, some problems related to these approaches which have to be kept in mind when interpreting their results.

1. Estimation of the counterfactual situation is done in a rather simplified way, by concentrating on one possible independent force, i.e., the effect of differences in industrial structure. Of course, there are many other independent factors which may be of equal or more importance (see below). Besides, the use of the same standardization techniques in other contexts has demonstrated that completely different interpretations can be given to the results.

2. The deterministic approach excludes the possibility that regional policy may also effect the national aggregates. If such an effect indeed exists (see Moore and Rhodes, 1975, for the underlying theoretical arguments as to how policy can influence national aggregates, and Rees and Miall, 1979, for some evidence) the counterfactual situation is inaccurately established.

3. Since policy instruments play no explicit role in the analysis, indications of the reliability with which quantitative policy impacts are estimated cannot be derived from this kind of work.

4. Application of these methods to small regions is not possible, since the use of national trends to obtain the expected series $\hat{E}_{rt}$ does not make much sense (compare Dessant and Smart, 1977).

5. There are some other drawbacks which are commonly associated with such simple standardization techniques (compare Richardson, 1978, and Schofield, 1979).

A third approach has concentrated on avoiding the drawback mentioned under 1 above, by incorporating several independent variables in a regression analysis, while the policy impact is still estimated on the basis of the residuals. For example:

$$Y_{rt} = \sum_i a_i X_{irt} + u_{rt} \qquad (3)$$

where

Y_{rt} = the dependent, impact variable

X_{irt} = the i^{th} independent, nonpolicy variable

u_{rt} = the residual

An equation such as (3) has been estimated cross sectionally for data on industrial employment growth in Dutch regions, in Vanhove (1962) (see also Vanhove and Klaassen, 1980) and in Van Duijn (1975), where the regional values of the unexplained residuals are interpreted as indicative for the size of policy impacts. Some problems with this approach are apparent:

1. There is no reason to assume that nonpolicy variables have no influence on the size of the residuals. This is especially relevant since the studies mentioned above incorporated a very small number of independent variables with, as a result, a rather low level of overall association in terms of R^2.
2. If policy instruments, which can *a priori* be expected to directly influence the dependent variable and which are likely to be correlated with some of the independent variables, are excluded, biased estimates of the regression coefficients, and consequently of the policy impacts, are obtained.
3. The average value of regional residuals is by definition zero in a cross-section estimation. This implies that positive residuals in some regions are offset by negative ones in other regions, and that a national effect of regional policies can therefore not be detected. It also implies that the absolute value of the residuals cannot be used to obtain a quantitative estimate of the policy impact; only a ranking of the residuals by size may reflect the degree of policy success.

To solve the problems related to these three types of macro-studies, an explicit incorporation of policy instruments in the model could provide a better alternative.

6.2 Explicit Incorporation of Policy Instruments

Models that incorporate both policy instruments and nonpolicy variables attempt to present a more complete description of the working of the economic system than the type of model discussed in

6.1. Such a description can in general be obtained in two different ways. The first way is to formulate some specific behavioral and/or technical hypotheses which are believed to be relevant for the part of the system being investigated and to derive testable relationships for the impact variable from these hypotheses. The second way is to use some *ad hoc* reasoning, based on intuition and evidence from other empirical studies (e.g., microstudies), in the selection of variables and the specification of the precise functional relation. The studies to be reviewed below belong mainly to the *ad hoc* type. The preference for such an approach, rather than strict theoretical reasoning in a very specific framework, is very understandable in this context:

- the conditions under which most economic theories would be applicable are in general difficult to find in the real world;
- there exist no comprehensive theories which reserve a specific role for the type of policy interventions investigated in the evaluation studies;
- if possibly relevant theories are formulated for the microlevel, application at the macrolevel is not straightforward, because of severe aggregation problems.

Ad hoc single equation models have been formulated for different types of impact variables (e.g., the deviation series $E_{rt} - \hat{E}_{rt}$ discussed above, industrial moves to assisted areas, regional investment and employment growth), and have been estimated with cross-section or time-series data, or sometimes a combination of both. The best way to make a subdivision is to consider the specific way in which the instrument variables enter the analysis.

First, there have been a small number of studies which have preferred to use *a composite index* to represent policy influences, e.g., a simple dummy variable to represent assisted area status or policy-on years (Bartels and Roosma, 1979; and Erfeld, 1979), or a weighted average of the strength of different instruments (Spanger and Treuner, 1975; and Vanhove and Klaassen, 1980). Of

course, the latter approach introduces much arbitrariness in the specification of the weights. The preference for such a composite index is motivated by observations like the following:

- if the number of instruments that may in principle affect the impact variable is very large, it may be intractable to separate indicators for each of them;
- if regional policy works essentially as a package of instruments which reinforce each other, an analysis of the separate influence of individual instruments makes little sense.

The acceptability of using this composite index approach to represent regional policy depends on the extent to which the dummy variable incorporates only the availability or non-availability of regional incentives and, accordingly, on the comprehensiveness of the specification of the nonpolicy component of the model. To the extent that other systematic differences between nonassisted and assisted areas, or between policy-off and policy-on periods, are not explicitly included in the model, these will be picked up by the dummy variable which will then inaccurately reflect the influence of policy. Furthermore, a 0/1 dummy variable allows no distinction to be made within the assisted areas or within the policy-on period in terms of the strength of policy. One can of course attempt to achieve a higher degree of differentiation by using values other than 0 or 1, i.e., by scoring or points system (Bartels and Roosma, 1979). Again, while this may be better than the simple black/white distinction made by a 0/1 dummy variable, it is still highly arbitrary. For example, to put one value at 0.5 and another at 1.0 implies that the latter should have an impulse twice as strong as the former - an extremely subjective and *ad hoc* approach, particularly when little justification is given to support the exact magnitude of these differences.

A second type of study has incorporated policy influences via another *intervening variable*. Particularly in the case of investment incentives this approach has been applied. Since one of the effects of such incentives is to lower the cost of capital one may define:

$$c_r = (1 - p_r)\, c \qquad (4)$$

where

c_r = the user cost of capital in region r

c = the national user cost of capital

p_r = the cost reducing impact of incentives

and subsequently incorporate variable c_r in some regional investment model (compare Erfeld, 1979, and Graziani, 1973). There are other examples which demonstrate how the use of intervening variables may shed some light on possible policy effects. Buck and Atkins (1976b) estimated elasticities of factor substitution for different sectors, to derive *ex ante* estimates of possible substitution effects caused by incentives for the use of a specific production factor. Treyz, et al. (1980) derive a reduced-form equation which can be used to estimate employment effects of changes in capital and labor costs, caused by changes in tax rates. (The relation is thought to capture effects of factor intermediate input substitution and of spatial relocation.)

The use of intervening variables to estimate policy impacts has at least the following drawbacks. First, it constrains the mechanism via which policy has its effects. To the extent that investment incentives influence investment by other routes (e.g., via a liquidity effect or a lowering of product prices) or are simply used as windfall profits, the effect of policy will

be inappropriately defined. Second, it assumes that firms (in the case of factor incentives) equally perceive, evaluate, and react to all variables that affect the intervening variable (compare Lund, 1976). Third, if the incentives differ for different types of projects it may not be easy to obtain a simple aggregate measure of the policy strength [like p_r in (4)].

The third, and most important group of studies is characterized by *direct incorporation of separate policy instruments* in the model. The measurement of the intensity of individual instruments has either been done by dummy variables (Ashcroft and Taylor, 1979; Erfeld, 1979; MacKay, 1976; Shaffer, 1979) or by more detailed indicators. Since we discussed the use of dummy variables above, we now concentrate on the more detailed indicators for the policy strength. Such indicators have been calculated either on a "volume" basis, or on a strength or intensity basis.

"Volume" measures express in some way the size of the program implementation and use, for example, total government expenditures and property taxes per capita (Shaffer, 1979), total received assistance for the development of public facilities (Martin and Graham, 1980), the size of infrastructural projects (Sant, 1975), the size of cumulated social capital stock in regions (Mera, 1975), the expenditures on regional incentives (Bölting, 1976; Erfeld, 1979; and Sant, 1975), the number of refusals for a development control policy (e.g., Bowers and Gunawardena, 1978), the relative number of applicants for regional labor market programs (Schmid, 1979), etc.

The use of such "volume" measures is however dubious for policy instruments whose intensity of use depends on the voluntary participation of the relevant decision units. In this case the "volume" measure will already incorporate the policy impact to some extent. Besides, there will easily be a chance of simultaneity bias since the level of the dependent variable may codetermine the volume measure. Finally, such volume measures may depend on the state of the economy such as in the case of investment incentives which are in general applied

more often in times of high econmic growth. This dependency makes the identification of an autonomous policy impact problematic.

The alternative to "volume" measures consists of measures of the intensity or strength of application of an instrument, which do not depend on the use that is made of the instrument. For example, the strength of incentive policies is measured by estimating their financial significance in reducing factor costs, as compared with average factor costs, and the strength of disincentive policies is measured in refusal rates, i.e., the ratio of refusals to applications. In many studies this kind of measurement has in fact been used; compare, e.g., Ashcroft and Taylor, 1977 and 1979; Bowers and Gunawardena, 1978, and Moore and Rhodes, 1973 and 1976b.

Also this approach to measurement has, however, some inherent problems. First, there are several regional policy instruments whose intensity cannot be easily approximated with a simple number, like soft loans, investment allowances, or free depreciation. During certain periods such instrument have been quite important in regional economic policy. Besides, other instruments frequently include important elements which cannot be easily quantified, such as *quid pro quo* deals and verbal steering in incentive and disincentive policies. Second, the need to obtain an aggregate indicator may require specific assumptions which hide important variations in the intensity of individual applications and in the ways the instruments may enter the decision process. Such variations may arise from the award conditions, different rates of incentives for different projects, limitations of the coverage of the scheme, the tax treatment of received aid, the discounting practices of a firm, etc. (A more extensive discussion of such variations can be found in Allen, et al., 1979; Melliss and Richardson, 1976; and Ohlsson, 1980.) Also the cumulation of incentives for a given project may lead to a total award that can be markedly different from the sum of the individual awards (see Allen, et al., 1979). It will be clear that the required assumptions to obtain an

aggregate strength measure are frequently rather far-reaching and not easily testable (compare Moore and Rhodes, 1973, for assumptions used to derive an aggregate incentive indicator).

A common problem in all studies which directly incorporate the separate policy instruments is caused by the fact that a relatively complete picture of policy and nonpolicy influences implies that a large number of independent variables will have to be incorporated in the anlaysis. But frequently the number of observations is very limited, and so the researcher has to make a selection from the possible independent variables. In most British evaluation studies this selection has been very limited, incorporating just one or two nonpolicy variables (especially the estimation of the influence of the "pressure of aggregate demand," measured in different ways, on industrial moves and regional development), and a very limited number of policy variables (excluding, for example, infrastructural investments and the availability of government advance factories). A notable exception among British studies is Keeble (1976 and 1980) where more nonpolicy factors have been included. Also studies for some other countries have made more serious attempts to incorporate a wider set of nonpolicy influences, e.g., Bartels and Roosma (1979), Martin and Graham (1980), Schmid (1979), and Shaffer (1979).

In cases where the nonpolicy situation is represented poorly, an overestimation of the policy impact may easily result, because of possible multicollinearity between included policy and excluded nonpolicy variables. In some studies, where multicollinearity contributed to nonsignificant parameter estimates, the collinear nonpolicy variables have been ommitted, with the possible consequence that the impact of policy variables becomes significant.

Typical structural developments of the recent past which have hardly been included in the policy studies of regional economic development include:

- Major changes in the economic structure, with a severe decline of employment first in agriculture and mining, and later (since the mid sixties) in manufacturing, and a fast increase of employment in the service sector;
- The move of people and jobs from the large conurbations to less urbanized areas, a move that has become important since the midsixties. According to Keeble (1980) recent regional employment trends in Great Britain can be better explained by the rurality of regions than by their assisted area status (compare also Fothergill and Gudgeon, 1978).

Since these structural developments have gained in importance precisely in a period of more active regional policy making, a minimal requirement is that their impacts on regional development be separated from the policy impacts.

6.3 Drawbacks of Single Equation Models

All approaches discussed in 6.1 and 6.2 employ a single equation framework. This restriction may imply a number of drawbacks:

1. In several cases it can be expected that the estimated coefficients will possess some simultaneity bias. This may be the case for independent nonpolicy variables, which can be expected to be influenced by the dependent variable. (Some employment studies use regional unemployment as an independent location factor, but this variable is clearly codetermined by the employment variable.) This may also be the case for the instrument variables, whose intensity may depend on the value of the dependent impact variable. (In cross-section studies a relatively poor regional performance of a variable like employment may explain the fact that certain policy instruments are applied in such regions.)

2. Several policy instruments are intended to influence more than one aspect of the regional or national economy, but such multiple objectives cannot be accounted for in a single equation framework.

3. Like other approaches discussed so far, this one implies that only a partial assessment of policy impacts is possible. Indirect effects on other regions, or on other variables, are not detected by means of a single-equation model. This drawback may be partly solved by applying some *ad hoc* procedures to the derived results, e.g., using multipliers from other sources to calculate indirect employment effects. Compare Marquand (1980), Moore and Rhodes (1976a) and Ohlsson (1980).

These drawbacks lead us to the following research method, which utilizies a multi-equation framework.

7. MACROSTUDIES WITH A MULTI-EQUATION MODEL

In multi-equation models several dependent variables are related to relevant independent variables, while sometimes simultaneous influences are also accounted for. Such models have been constructed for single regions, or for several regions at once (a multi-regional model). If interregional linkages are represented in the model (an interregional model) interregional policy impacts could in principle be estimated. In a top-down multi- or interregional model (i.e., a model that distributes national totals derived from a national model to the different regions) only distributional impacts of policies can be analyzed, while in a bottom-up approach (where national totals are calculated by aggregating regional values, which have been determined in the multiregional model) also regional policy impacts on national aggregates can be estimated in principle. Most operational multiregional models are some mixture of a top-down and bottom-up approach, with some variables determined primarily at the regional level (population, labor force, local services) and others first at the national level.

A relevant distinction in the present context of policy evaluation is between econometric and noneconometric models.

7.1 Econometric Models

In econometric models the parameters are estimated by statistical techniques, using a sufficient amount of data so that statistical inferences can be made about the reliability of the results. Although a considerable number of regional econometric models have been constructed in the recent past, few of them seem to have been designed with the principal aim of making impact assessments possible.

Examples of multiregional models especially designed for policy evaluation are cross sectionally estimated simple models for Austria (Berentsen, 1978) and for the Netherlands (Folmer, 1980; and Folmer and Oosterhaven, 1981). These studies all adopt a multiple objective framework, but are also characterized by a poor specification of interrelationship between variables and regions. That is, interregional linkages are absent; Berentsen incorporates only policy instruments and impact variables, but no situational variables; in Folmer (1980) no instrument variables are explicitly used. An interesting feature of the two Dutch studies is that they utilize an estimation technique which explicitly allows for measurement errors and unobservable variables. An additional interesting characteristic of Folmer and Oosterhaven (1981) is the specification of instrument variables as endogenous variables in the model.

Other econometric models for regions have generally paid most attention to the specification of relationships between dependent and situational variables, with less concern about the role of policy intervention in the economic system. Therefore, only very particular policy impact analyzes can be made with most of these models. Examples of models with the capacity for impact assessments can be found in the review papers of Bolton (1980) and Glickman (1980a) (see also Adams and Glickman, 1980; Glickman, 1980b; and van Delft et al., 1977). An especially ambitious

model is the 51 regions bottom-up model developed at the U.S. Department of Commerce (compare Ballard and Wendling, 1980) which has been used for several types of *ex ante* policy impact analysis (compare Ballard et al. 1980).

7.2 Noneconometric Models

There exists also a group of models, consisting of relationships which have not been quantified by means of conventional statistical techniques. Instead point estimates are used for all or some of the coefficients, or a mixture of different estimation models is used for quantification of the relations. Such models have generally been designed to make rather detailed simulations of economic systems possible, a purpose which has made a full econometric estimation of the model impossible. Input-output models can be considered as first examples of this group, since they have generally been based on point estimates of input-output coefficients and other relevant coefficients. Regional input-output models have the capacity of estimating also some indirect effects of certain exogenous changes, through interindustry linkages, induced consumption (if consumption is endogenous), or induced investment (if investment is endogenous), while in interregional input-output models, which incorporate interregional interindustry linkages, also some interregional effects can be traced. Since input-output models are essentially demand driven, a requirement for policy impact assessment is that the policies can be translated into changes in the final demand categories, e.g., in government expenditures. A large number of studies have exploited this feature for both *ex post* and *ex ante* estimation of policy impacts, mainly of changes in government expenditures in regions. (Recent examples are Oakland, 1979, and Oosterhaven, 1981; in Moore and Rhodes, 1976a, input-output models have been used to calculate impacts of labor subsidies via their effect on prices of final demand categories.)

A second group is formed by the so-called microsimulation models. Such models have been designed to simulate *ex ante* effects of policy changes on data bases containing observations

on disaggregated components of one or more major sectors of the economy. The disaggregated feature makes them especially attractive for the analysis of distributional impacts (i.e., on certain groups of people) of policy changes. The advantages which are generally ascribed to microsimulation models include: available detailed information about the initial state of the microunits can be fully used; regulations relating to microunits can be given a literal representation; available understanding about the behavior of microunits can be used; assumptions about microunits can be introduced at the microlevel; outputs can be aggregated as desired (see Orcutt, et al. 1980); they can give answers to questions at the microlevel; and the use of disaggregated relations may be more accurate than that of aggregated relations because the latter conceal distributional assumptions of some kind (Arrow, 1980).

In most applications of microsimulation models the behavior of households is analyzed in the most detailed way (compare several contributions in Haveman and Hollenbeck, 1980), while enterprises and government are treated at a more general level. (For an attempt to analyze the behavior of firms in a more detailed way see however Eliasson, 1980.) Treatment of some variables at an aggregated level requires the addition of a macroblock to the detailed microblock.

Microsimulation models have been used for the *ex ante* assessment of spatial impacts of certain national policies; compare some of the contributions in Haveman and Hollenbeck (1980) and also Danziger, et al. (1980).

7.3 Some Weak Points in Multi-equation Models

Although the use of a multi-equation framework offers in principle an attractive point of departure to obtain quantitative estimates of policy impacts, most operational models have their weaknesses, which restrict the reliability of their results. These weak points relate mainly to the specification of the models.

First, many models are to an important extent demand driven, and do not allow the possibility of capacity constraints, which in reality may be very important. (In Oakland, 1979, it is demonstrated how the incorporation of such constraints affects the impact estimates.) Second, interregional linkages are mostly incorporated in a rather poor way, e.g., by means of crude interaction variabls which are assumed to represent such linkages. (The basic problem is, of course, the absence of statistics on interregional trade.) Third, as was noted already, the policy component of the models is in general very poorly specified, with few serious attempts to incorporate the most relevant policy instruments. Fourth, most models have used a very traditional theoretical framework with few attempts to adjust "national" theories to the particularities of the regional economy. (For example, a disequilibrium approach seems to be very obvious in many cases, but it has hardly been used in regional model building.)

There are still some other problems, which we prefer to discuss in a separate section, however, since they apply also to the single equation studies.

8. WEAK POINTS IN THE MACRO APPROACH

Several researchers have used some sort of macro approach to obtain quantitative estimates of direct and indirect policy impacts. Especially British researchers have shown much enthusiasm for the single-equation approach. Their applications have even produced a controversy about what the exact figure of employment impacts of regional economic policy has been (for a summary of different quantitative estimates, see Ashcroft, 1979). It is, however, difficult to take such discussions seriously, since there are several weak points (apart from those already mentioned above) in the macro approach, which make such exact interpretations of the results a bit ridiculous. In this section we want to discuss some obvious problems which arise in most macrostudies.

A first problem is caused by the quasiexperimental nature of the research. Of crucial importance in such research is the specification of the policy-off situation which requires that all

relevant nonpolicy factors have been included. But the problem common to all social science research is that one never knows for sure whether all of the relevant variables are included (see also Cook and Scioli, 1975). Therefore, there exists much room for personal preferences, which will affect the results. More specifically, in many studies of regional employment and industrial movement, it is often difficult for the reader to find the rationale behind the choice of explanatory variables. Studies on industrial movement, for example, are not based on any explicit theory of the firm or on previous results from microstudies. (Microstudies would suggest the importance of internal aspects of the firm and the availability of sites and premises for location, but both factors are absent in most macrostudies.)

A second general problem relates to the choice of proxies and measurements for the variables. We mentioned already the difficulties which arise with the measurement of instrument and impact variables. But also the measurement of the nonpolicy independent variables may have an important influence on the derived results. In Britain, for example, there has been a controversy about which pressure of demand proxy to use in movement models. Since this variable was the only nonpolicy variable in such studies as Bowers and Gunawardena (1979), and Moore and Rhodes (1976b), the quantitative results appeared to be very sensitive to the particular proxy used. Similar problems have arisen with the proxies for nonpolicy location factors. For example, how do we measure the attractiveness of the regional labor market? Should this be measured by the relative regional unemployment rate, by the deviation of the regional from the national unemployment rate, or by means of a more comprehensive indicator which also accounts for influences on labor supply from intersectoral shifts in employment, natural population growth, and autonomous interregional migration?

A third problem, which arises especially in employment relations, is caused by the complicated time lags which may exist between the implementation of an instrument and the dating

of the impact. Such lags may be considerable, for instruments like investment incentives and infrastructure provision. Some kind of distributed lag structure could perhaps capture this characteristic in the model, but the present evaluation studies do not utilize such a structure. The problem becomes still more complicated if it can reasonably be assumed that the lag distribution varies both cyclically and secularly over time (compare Lund, 1976). It is hardly possible to translate this into an operational model, and it is not very clear how the results may be affected by the resulting misspecification of the model.

A fourth problem is related to the assumption that different explanatory variables are independent of each other, and that they have a proportional impact on the dependent variable. This assumption is very common in these evaluation studies. In many cases, however, it is clear on *a priori* grounds that such an assumption is unrealistic. For example, it is unlikely that the pursuance of regional policy objectives and the state of the regional and/or national economy will be independent (compare also Nicol, 1979); or that the effects of incentives and disincentives are largely independent of each other, or that investment incentives will have a proportional impact on regional investment. As long as such possibilities are not taken into account in the model specification, the results may be completely misinterpreted.

A fifth problem which is inherent to regression analysis is the required assumption of constant coefficients over relatively long periods of time (in time series analysis) or over a relatively large set of different regions (in cross-section analysis). In many applications this assumption seems to be hardly defendable, however. Nonpolicy variables may have an impact which differs in time (e.g., the effect of regional labor supply as a location factor is codetermined by the general labor market situation) or over regions. The same may be true for the impact of instrument variables. In Shaffer (1979) it was demonstrated that the results of a cross-section analysis differed already for different subperiods within a time span of 10 years. In Suyker (1980) it was

shown how, among other things, the impact of housing supply on net regional migration changes over time, while Suyker (1979) presents evidence for a time dependent impact coefficient for investment incentives in a regional industrial employment model (by making the impact dependent on the national growth of industrial employment).

A sixth problem which is typical for the evaluation studies is that the presented information makes it rather difficult to adequately judge the precise statistical properties of the estimations. They present very few test statistics for judging to what extent the statistical assumptions underlying the estimation are indeed met and hardly any sensitivity tests (to investigate the effects of small changes in the data or removal of outlayers for example). The problem is particularly severe with the large scale econometric regional models. Since they are so large, it is simply impossible to present all relevant statistical information. But the consequence is that it is also impossible for an outsider to make any reliable judgement about the quality of such models (see Arrow, 1980, for similar complaints).

A final problem with present studies relates to the uncertainty of the impact estimates. Adopting a stochastic framework, as has been done in most studies, implies that one can never tell with complete certainty what will be the impact of a certain explanatory variable. The only assessment that can be made is that under a large number of assumptions an interval can be specified in which with certain reliability the impact estimate may be located. It is rather curious to note that none of the evaluation studies demonstrates full awareness of the stochastic nature of the results obtained. All put strong reliance on the interpretation of point estimates, and no attempts are made to present interval estimates. This drawback is, however, not particular to the studies reviewed in this paper; it also applies to most other evaluation studies in economics (see Arrow, 1980, for additional comments). Yet, the drawback may be rather simply solved especially in linear models (whereas for nonlinear multi-equation models, where formulas for

asymptotic variances of impact multipliers are not known, stochastic simulation could be used to perform sensitivity analysis, compare Fair, 1980). Unreliability of coefficient estimates is, however, not the only source of uncertainty in the impact estimates. For example, one knows for sure that the model will not completely represent the real world (especially in cases where important changes in policy are investigated, which may be expected to affect also the structural relationships themselves), and in *ex ante* analyzes the values of other exogeneous variables will be unknown. All these sources of uncertainty require careful sensitivity studies to investigate how they might affect the results.

9. HEROIC ATTEMPTS TOWARDS MORE COMPREHENSIVE COST-BENEFIT EVALUATION

The foregoing paragraphs have reviewed studies which could be characterized as partial assessments of policy impacts. None of these studies intended to derive a more or less complete picture of different advantages and disadvantages of certain policies from a certain point of view. Given the many difficulties one has already to solve for making reliable partial impact assessments, it is obvious that attempts to assess the costs and benefits in a more comprehensive way still require a larger number of often heroic assumptions. Results of such studies are consequently easily disputed. It is not our intention to present extensive criticism here; instead we shall briefly mention some studies which contain interesting approaches.

A first type of cost-benefit analysis may be undertaken from the point of view of *the Treasury*, to investigate if the policy implies on the whole a real cost for the state. Such an analysis has to estimate different kinds of costs and benefits for the Treasury in financial terms. Benefits may include payments on recoverable items (like loans), increases in tax payments from assisted firms and newly employed workers, increases in national insurance contributions, savings in unemployment and other social security payments, savings in infrastructure because of a more

favorable population distribution. Costs may include non-recoverable outlay items (grants), increases in infrastructural expenditures for firms or households, the adminstrative costs of policy making, and possible negative effects (more unemployment benefits, less tax payments) caused by possible negative indirect effects of the policy. In addition it has been argued that also consequences of additional national economic policy, to maintain aggregate demand in prosperous regions, have to be added (Moore and Rhodes, 1974, 1975, and 1977; see however, Schofield, 1979, for criticism). It seems that this kind of cost-benefit analysis will yield the most reliable results when made for very specific policy instruments, instead of complete policy packages. (Examples of such applications are Schmid, 1979, where an evaluation of regional wage-cost subsidies in Germany is presented, and Calame, 1980, which contains results of evaluation studies for wage subsidies in a number of countries.)

A second type of more comprehensive evaluation attempts to quantify advantages and disadvantages for *the national economy*. It is not *a priori* clear on what variable such assessment would have to concentrate; apart from economic variables such as unemployment and national income, other valid indicators would be the degree of overall congestion and the well being of people derived from their places of living. If we restrict ourselves to the economic impacts, we can indicate some of the effects on the national economy. Possible advantages of regional policy could include the use of resources which would otherwise have remained idle (especially labor), a decrease in inflation, effects of national policies implemented to affect the situation of prosperous regions. [In the Moore and Rhodes (1975) approach the assumption about the national policy strategy determines which part of created jobs can be considered as net creation instead of simple diversion from the prosperous regions.] Possible disadvantages could include the costs of moving firms to other locations, higher production costs in assisted areas, productivity losses from more labor intensive production, lost output resulting from disincentive instruments, extra costs caused by additional infrastructure, etc. Serious work to explore the complicated questions

which arise in such evaluation has been done by Blake (1973) but especially by Moore and Rhodes (1974, 1975, and 1977). (For a critical discussion of their assumptions compare Ashcroft, 1979; Marquand, 1980; and Schofield, 1979.)

A third type of comprehensive evaluation is concerned with *the opportunity costs* of the policy strategy actually used. The basic question is: What would have been the implications of alternative uses of the expenditures devoted to regional assistance, e.g., on the use of national resources? To answer this question one needs an assumption concerning what kind of restrictions on the national economy could have been removed by means of the released financial means and some kind of model of the national economy which makes *ex ante* assessment of alternative policies possible. A good discussion of the difficulties which have to be solved with this approach is given in Marquand (1980).

This brief discussion of the more ambitious comprehensive evaluation studies demonstrates that a complete assessment of costs and benefits of one or alternative policy strategies can be considered as being beyond the capability of objective research. There are so many disputable assumptions to be made, that unequivocal conclusions are impossible.

10. HOW TO PROCEED

A reader impressed by all the difficulties associated with quantitative assessments of policy impacts could be inclined to conclude that a serious scientific approach to this particular problem is not very well practicable; and that therefore, this field of study is not a very fruitful one for further development. However, the role of government in economic life has become so important, that it is simply impossible to deny its impacts on economic development. Policy makers themselves have already created an increased demand for studies that assess impacts of different policy actions. (This is, for example, demonstrated by the trend towards urban impact analysis in the USA, see Glickman, 1980b.) Therefore, it makes sense to conclude this discussion with a brief answer to the question: How do we proceed?

An obvious first desire for future research on this topic, is that the possible drawbacks related to the particular methodology adopted are minimized as much as possible, and that careful sensitivity analyzes would be made of the possible effects of remaining deficiencies. With respect to the attractiveness of the different methods, it is our opinion that valuable results could be obtained from the experimental approach to microstudies which could contribute to a better understanding of the working of very specific programs (see also Marquand, 1980, for pleas into this direction), and from the further development of multi-equation models, into a direction which would provide insight into effects of instrument variables on the ultimate goal variables of regional policy (such as unemployment, activity rates, interregional migration, and even self sustained economic growth of regions). Such models could also be helpful as tools for *ex ante* impact analysis, which is another area where more future research would be welcome.

It has already been demonstrated (Marquand, 1980; and Moore and Rhodes, 1976a and 1977) that the reliability of impact estimates can be much improved, when results of micro- and macro-studies are compared, to detect to what extent these point in the same direction. *Ex post* evaluation studies should adopt this approach more frequently.

Finally, there are two specific topics which seem to require more attention in future evaluation studies. The first is the role of infrastructural investments in regional economic development. This role has hardly been investigated empirically up until now (compare also Nijkamp and Sigar, 1980). The second is the role of long-term trends in economic life in regional economic development. The prosperous phase in the long cycle has been characterized by a relatively fast expansion of business activity, looking also for new locations in peripheral areas, and by the increase in available financial resources to intensify regional economic policy. The intriguing question of which of these two factors has contributed most of all to the occasionally observed improvement in the performance of the development areas has not yet been sufficiently answered.

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