REGIONAL ANALYSIS: THE SEARCH FOR A MODEL OF INTRA-NATIONAL TRADE AND FACTOR MOBILITY

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August, 1980

RESEARCH REPORT

NORTHWESTERN UNIVERSITY EVANSTON • ILLINOIS

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[2]Since the conference volume gives full titles of papers and lists all authors, in my footnotes, I will abbreviate titles and indictate only the name of the author who appears first on each paper.

Regional Analysis: The Search for a Model of Intra-National Trade and Factor Mobility

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Leon N. Moses [1,2]

The regional analysts whose papers have been presented at this conference work on complex real world problems. The influence of a change in freight rates, or of a shift in federal policy on the outputs, prices, and levels of income and employment of regions are two examples. These problems are investigated with the aid of a variety of sophisticated econometric and other techniques such as gravity formulations, central place theory and input-output analysis. On the whole, the techniques are applied with skill and ingenuity. The principal complaint expressed by the analysts is that the data with which they must work are very poor. They are right. Clearly, better results might be achieved if better data were available for such things as regional production techniques, outputs, commodity and factor prices, and interregional commodity and service flows and their transfer costs. However, are poor data the only problems regional analysts face? Are there no unresolved conceptual issues? Are there no areas where better decisions would be made as to the kinds of data that should be gathered if the right theoretical questions were asked first? I am not suggesting that the authors are uninterested in theory. What is more likely is that the research on which they have reported involves so many serious empirical problems, there was little point in their

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raising still other complications. As a reviewer I feel I should raise questions about matters of theory on which the authors have not said enough or have said nothing. I will take these issues up first and then turn to some technical matters pertaining to the use of input-output analysis.

Foreign trade specialists have a simple, elegant theory to explain the kinds and amounts of commodities nations produce, import, and export. Tastes enter the model but relative factor endowments of nations and factor intensities of production functions form the core of its logical structure. Regions specialize in the production of those goods that require large amounts of those factors with which they are relatively well endowed. Regional specialization and trade tend to reduce factor price differentials. In equilibrium all commodity and factor markets clear, and each nation is in balance of payments equilibrium. Regional analysts have generally taken the view that foreign trade theory offers little insight into the functioning of an economy in which there are few artificial barriers to the flow of goods, services, and information, but in which these flows are strongly influenced by significant transfer costs. The mobility of capital and labor within advanced nations is too great to permit use of a model that assumes fixed regional endowments and defines comparative advantage in terms of such endowments.

There are other reasons for rejecting the approach of traditional trade theory. It tends to have a commodity-resource orientation and to ignore the service industries, placing them automatically in the category of non-traded goods. Also, the foreign trade model does not pay much attention to transport costs. Basically it defines comparative advantage in production ter
Many regional analysts believe that transport costs add an important market dimension to comparative advantage. Regions have a comparative advantage in selling a given output in some areas and a comparative disadvantage in selling

the same output elsewhere. These are all valid criticisms. I also take the position that the foreign trade approach to comparative advantage cannot be adopted as an explanation for intra-national economic relationships. However, there are aspects of the theory of foreign trade that I feel raise questions about the studies with which we are here concerned. I will begin by considering these questions. I will then turn to some other theoretical matters on which economists, geographers, and others interested in regional analysis have worked, but which the conference papers ignore or appear to handle poorly.

The foreign trade model has both a monetary and a real aspect and the two are fully integrated. In equilibrium, factor markets and commodity markets clear and the value of a nation's imports equals the value of its exports. If a change disturbs the trade equilibrium, adjustment mechanisms are brought into play that reestablish it. The conference papers, as well as the broader literature on regional analysis, reveal that the real aspects of intra-national trade need much more work. The situation is perhaps even worse in the monetary area, which is where I will begin my examination.

I do not believe that any of the conference papers even considers questions of trade equilibrium and the mechanisms that assure a tendency toward such equilibrium. Balance of payments and balance of trade calculations are absent from the papers, though Ms. Polenske comments that her model permits a variety of trade balances to be calculated. Why the lack of interest in the problem? Surely the answer is not that trade disequilibria cannot exist when regions have a common currency. A common currency simply means that the regions operate under a standard that is equivalent to, but nore efficient than an old-fashioned fixed exchange-specie standard, more efficient because bank balances are transferred at less cost than bullion. Such a standard does not mean that shifts in demand cannot reduce the prices a region receives for its

exports and leave it in a position of trade imbalance.

Suppose our authors had carried out the necessary calculations and found large imbalances, what would they have done about them? Five of the eight papers fix regional trading patterns in one way or another, three by the assumption of stable trade coefficients.[3] It seems to me that the authors who make this assumption, particularly those who use fixed trade coefficients have cause for some concern. Suppose there are large trade imblalances in the year for which the trade coefficients are calculated. Should such a disequilibrium be built into the system and permitted to go on and on without correction? Since none of the eight authors takes up issues of trade disequlibria, the circumstances that produce them, and the mechanisms that correct them, I cannot provide firm answers as to how they would respond if now asked to consider these matters. However, the logical structure of the models and the empirical work done by the authors provide a basis for reasonable conjecture as to the corrective mechanisms they would adopt. It is more difficult to speculate about how they would explain the disequilibria.

Suppose a region has lost comparative advantage in certain lines and begins to experience a trade deficit. In the short run the import surplus is paid for by

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[3]Polenske, Multiregional Economic Accounts; Haveman, The Poverty Institute Model; Treyz, Massachusetts Economic Policy Analysis. The latter estimates regional purchase coefficients in a complicated, indirect way rather than from interregional commodity movements. However, I believe they serve the same function as trade coefficients. Besides, the authors indicate that a future version of the model will divide the state into seven sub-areas. Regional purchase coefficients will be used to determine intra-area activity and the usual trade coefficients will be used to determine trade with other states (p.65). The Ballard Model, A Bottom-Up Multiregional Model, does not have trade coefficients, but the coefficients on distance in the individual industry gravity models amount to the same things. The flows between areas change in volume only because exogeneous changes alter mass, but that is exactly how fixed trade coefficient models behave. The same comments pertain to the Olesen model, Multiregion. Harris, Multiregional Forecasting Model, attempts to avoid the assumption of fixed trading patterns, as does Friedlander, Freight Rates. Neither of these authors or Kresge, The Alaska Model, is concerned with trade equilibrium issues and the adjustment mechanisms that tend to establish such equilibrium.

drawing down assets, but how is the situation cured in the long run? I believe that the authors would rely virtually completely on labor migration to restore equilibrium. It would reduce aggregate regional income and therefore imports along with other consumption. The papers exhibit a very strong tendency to treat all disequilibria as labor market phenomena and to cure them by migration rather than factor price adjustments.[4] With one possible exception, the authors do not deal with migration costs and their effects on the mobility of labor.[5] Essentially they treat labor as if it were as mobile as capital. They then

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[4]I include the Treyz and Ballard models in this characterization even though they make some effort to have regional wage rates respond to unemployment. I do this because if a wage rate change is to offer an effective cure for an unemployment or a trade deficit situation, it must bring about both cost of production and price changes. The Treyz model has something of a procedure for doing the former, but Massachusetts cost changes are not then related to area price changes. The latter are made to depend solely on changes in the U.S. Consumer Price Index (p.27). The Ballard model, wage determination procedures, which are less satisfactory than those of Treyz because they completely ignore the influence of labor productivity, (p.16) do not have wage rates influence costs of production. As in Treyz, regional prices change as the U.S. CPI changes (p.16).

[5]The Treyez model is the exception, though I must say that its procedures for determining Massachusetts labor force and population seem very odd to me. The labor force equation has log of Massachusetts population 18 and over times the full employment labor force for the U.S. divided by the U.S. population over 18 as one of its explanatory variables. I am at a loss to understand the theory behind this variable and the authors offer no explanation. The dependant variable in the second equation is log of Massachusetts population divided by the U.S. population. The explanatory variables are the log of the ratio of non-agricultural wage and salary employment in Massachusetts to the corresponding U.S. variable for the present quarter and for each of the two preceeding quarters. Again, the theory behind the choice of the exploratory variable is not explained: nor is there any explanation of why relative wage rates play no role in determining the size of the State's labor force (pp.24,25,26). All of the above aside, it is nevertheless true that the Treyz model has a two quarter lag structure for labor force and population determination. A two quarter lag is not much but at least it implies labor is not perfectly mobile.

take a second step and proceed as if land, the immobile factor, does not exist.[6] Even if capital and labor are perfectly mobile, why can't the price of land fall in the deficit region? Such a change would improve its competitive position in other lines. The expansion would establish a tendency toward trade equilibrium and reduce the need for migration. With one certain and one uncertain exception, the authors cannot use changes in factor prices, costs, and commodity prices as their equilibrating mechanism. In one way or another the models listed in footnote three above freeze trading patterns. Changes in relative factor prices, even a fall in the price of space, with wage and interest rates constant, would alter production costs and trade patterns.[7] The Friedlander model does not fix trading patterns but her econometric procedures require that each region face perfectly elastic supply functions of labor and capital.

The two possible exceptions noted above are Harris and Kresge. Harris does not assume fixed trading patterns. The logic of his model does not preclude endogenously generated changes in factor prices, but the actual model does not incorporate such changes. I am less certain about how to judge the Kresge model. It does not make explicit use of the assumption of fixed trade coefficients. However, it does designate certain industries as being traditionally export and others as import oriented. Of course, Alaska is likely to continue to export forestry and fishery products and import manufactured and agricultural products,

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[6]Harris does at least mention land but its price is fixed. This is an issue to which I will turn later.

[7]The Ballard and Treyz models allow local labor conditions to influence local wage rates and at the same time assume unchanged trade patterns. The Olesen paper is in a somewhat different position. The paper is based on a large study: Multiregion: A Simulation-Forecasting Model of BEA Economic Area Population and Employment, Oak Ridge National Laboratory. The authors of this work claim their model equilibrates labor markets by wage rate adjustments as well as migration (p.53). However, I find no evidence of the former either in the full work or in the Olesen conference paper. The authors are therefore contradictory in intent but not in actual practice.

but will it do so to the same degree in the future as in the past? Perhaps I am being precious but I suggest that the Kresge model really does assume stable trade coefficients. All but a small (fixed?) percentage of Alaska's resource based outputs are assumed to go out of the state; all but a small (fixed?) fraction of Alaska's requirements of agricultural and manufacturing goods come from the lower forty-eight. It may be that the implied trade coefficients for the above industries are zero's and one's.

I have been examining the implications of a trade deficit situation in terms of an older region that has lost comparative advantage in certain lines. Let me now consider the case of a relatively undeveloped region that is busy exploiting new opportunities. Capital and labor are flowing in and capacity is being expanded. A trade deficit exists but it simply reflects growth and the inflow of long term investment cpaital and labor. I submit that the Harris paper, which has serious difficulties of its own which I will raise later, comes closest to being able to cope with this kind of factor augmentation situation in a reasonable way. It does so because it deals explicitly with location rents and generates investment on the basis of such rents.[8]

What would the authors whose models use the assumption of fixed trade coefficients do about investment and the factor augmentation case? Polenske

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[8]The Friedlander and Treyz models appear to have the necessary ingredients for a calculation of regional profits. The former has prices, transport charges, and cost functions. The Treyz model has prices, even though they are strictly based on the CPI, and cost functions. Nevertheless, neither of the two models can be used to generate estimates of regional profits. I do not wish to go into all of the serious data problems that lead me to this judgement but will instead concentrate on one theoretical issue at this time. Both authors have cost functions that are essentially defined in terms of labor and capital. Energy costs are included but they surely do not account for all intermediate inputs costs. Since such input costs do vary regionally for the same industries, the author's cost functions are incomplete and cannot be used to calculate profits.

has already made it clear that one of the reasons she wishes to build dynamic input-output models is that she wishes to avoid the assumption of fixed trading patterns. However, neither hers nor the other input-output models employ optimization techniques. They do not generate location rents. The dynamic model she proposes is of the fixed accelerator variety in which investment depends only on rates of change in output. That kind of model could easily generate grossly incorrect patterns of investment by industry and region. By way of explanation, consider the following realistic case. Final demands change in such a way that the output of some industry must expand greatly. A certain region has a great deal of unused capacity to produce the good. Its costs are high and the industry has gradually been moving to the kind of developing area mentioned above. A surge in demand causes output to increase in the declining area, perhaps to the point where it is operating at full capacity. Profits may even appear, but no new investment takes place there because entrepreneurs realize that the basic long term disadvantage of the region has not been eliminated. However, non-optimization type fixed accelerator models will automatically call for capacity expansions in the region. Such models are bad enough at the national level. They are a disaster at the regional level because firms can obviously choose where to invest.

Above we dealt with trade equlibrium and the monetary aspects of trade. Matters pertaining to factor prices and endowments did come up, but I would now like to deal with them and other real aspects of trade in more detail. Again I begin with a question. What is the theory of comparative advantage with which the authors of the conference papers are working? Nowhere in the papers do I find a clear statement of what comparative advantage means in an intra-national context. It would have been interesting to ask each of the authors to provide a brief statement of the theory of trade that guided their empirical work. It is too late for that now. However, the assumptions made,

implicitly as well as explicitly, imply a certain theory of regional specialization and trade. I will present an overall characterization of the theory with which I believe the authors are working and then note ways in which it does not properly represent aspects of individual papers. I will then indicate the changes in the theory that I believe should be made if we are to explain trends in regional specialization and trade within developed nations.

The authors assume that capital is perfectly mobile. They do not state that labor adjusts instantaneously to regional differences in employment pressure, unemployment rates, and wage rates. However, they ignore migration costs and give every indication they believe labor is highly mobile even in the short run and perfectly mobile in other periods. Nowhere do the authors say anything about consumers' utility functions or how consumer satisfaction depends on location.

Concerning the latter I will make two assumptions that are common in both urban economics and foreign trade studies: preference functions are identical, and the satisfaction a consumer derives from any bundle of goods does not depend on location. The last condition really means that all areas are identical in their social and physical amenities, including climate. Not all of the authors state that they are working with the perfectly competitive model but I do not believe they would object to my attributing this assumption to them.

The papers do not deal with innovations and the diffusion of technical knowledge. However, since labor and capital are treated as perfectly mobile it seems reasonable to conclude that the authors reason as if technical information is a ubiquity, at least in the long run. This implies regionally identical production functions. Treyz assumes they are identical even in the short run, and I will shortly argue that this assumption is also implied in

the work of all of the other authors except Friedlander. Finally, on production functions, all of the authors assume that they are homogeneous of degree one, including those for transportation. Some of the authors make general references to and comment on the importance of scale and agglomeration economies. However, again except for Friedlander, all assume Leontief-style intermediate input coefficients for all industries.[9] Treyz uses such coefficients for intermediate inputs. He allows substitution between primary factors, labor, capital and energy, but the overall production function for each industry is assumed to exhibit constant returns to scale.[10] The papers do almost nothing with land, land rents, and the quality of resources and climate. In order to see where the main assumptions made by most of the authors lead us, I would like to begin by assuming a nation that is made up of regions that differ in size but not in the quality of their land.

The assumptions concerning mobility of capital and labor mean that rates of return on investment and real wage rates must be identical in the long run. If land is of identical quality everywhere and all production functions are identical and homogeneous of degree one, the equalization of the above two factor prices means that rent, the price of the immobile factor, is also equalized, which then means that cost functions are everywhere the same. If all industries

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[9]Friedlander rejects the fixed coefficient approach. Capitol, labor, and energy enter her cost-production functions in a way that permits substitution. Her approach to cost estimation does not rule out scale economies but she notes that "....the data are probably disposed to indicate that production is subject to constant returns to scale."(p.20).

[10]Both Treyz and Friedlander accord energy a special status in their cost function analyses but do not explain why they do so.

are competitive, only normal profits will be earned in the long run and commodity prices, which will be equal to costs, will also be identical everywhere.

The model with which I believe the authors are working, with the possible exception of Kresge, yields a long run solution of economic isolation. There is no regional specialization or trade. The regions are scalar versions of one another. A region that has twice as much land as another will also have twice as much labor and twice as much capital. It will produce and consume twice as much of everything but at identical prices. Trade is the result of a temporary factor immobility. Perhaps labor is not instantaneously and perfectly mobile, or the diffusion of technical knowledge involves both cost and time. Such short run immobilities and adjustment costs produce trade as a temporary equilibrating mechanism as the system works towards long run isolation.

I think that there will be some regional analysts who will not only accept my characterization of the trade theory implied in the above assumptions, but who also believe that empirical investigations reveal the system really operates that way. That is, they believe that regional specialization and trade become less important over time unless there are new shocks to the system, like major innovations. These analysts often judge the extent of regional specialization and the importance of trade from time series data on employment. Using a two or three digit industry classification they find such things as a convergence toward the national average in percent of regional employment accounted for by industries. Such convergence is taken to mean that regions have also become more alike over time in output mix, which is to say that they have less reason to trade with one another. I do not wish to

dwell on the theoretical and empirical weaknesses of such studies. A significant part of the trade that takes place in any year is probably the result of disequilibria that are in the process of being slowly worked out. The shift of an industry from regions that have specialized in it in the past to other regions often takes decades. In the process, time series data on regional employment or output will surely show that the industry is becoming more geographically dispersed. However, that does not mean that the industry has become footloose and that in equilibrium each region will be self-sufficient in it. The numbers may simply be telling us that the shift is not yet complete.

To repeat, many locational adjustments take a long time. As a result, a good deal of the trade that takes in any year probably reflects disequilibria. Therefore, I am not absolutely shocked by Olesen's finding that inertia, as represented by past employment, is the most powerful determinant of present BEA employment in manufacturing as well as service industries.[11] However, is it possible that the slow pace of change Olesen observes is not wholly the result of inertia and adjustment costs? Perhaps there are the fundamental regional characteristics and advantages that neither he nor the other authors are able to reveal, again with the possible exception of Kresge, because of the analytical techniques they have chosen to adopt. Perhaps there are very significant scale and agglomerative economies which assure that certain industries will only be found in a few regions in the long run, though they may move from some areas to others. It is to these and some related matters that I now wish to turn but I would like first to summarize my overall position. The trade that takes place at any time is of two kinds.

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[11] pp.21 and 27.

Some of it is the result of temporary disequilibria, short run factor immobilities etc. The remainder, and certainly not an unimportant part of trade, is the result of fundamental differences in regions, and of scale and agglomerative factors. The assumptions and techniques adopted by the authors are not well suited to distinguishing between the two and to revealing the influence of long run conditions. Let us begin by thinking about the price of space or resources.

Suppose the quality of land or a certain resource or climate makes a region especially suited for a particular industry. Over the years a large number of firms that produce the product have located in the area. There is a significant export trade. Other industries are also found in the region, some providing raw materials or semifabricated manufactured goods to the export industry, some providing manufactured products or services to the local population. The argument I would like to make can be framed in terms of an increase or a decrease in demand for the region's export product, but let me choose the former. Output of the industry expands and additional capacity is installed. I believe that all of the models presented at the conference except the Friedlander model would always have every other industry in the region also expand.[12] In this sense the logic of the models is that of the simple export-base multiplier.

As a result of the induced across the board expansion in all outputs, there would have to be a much greater in-migration of labor than that required to satisfy the employment requirements of the export industry. The models

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[12]The Friedlander model is not a general equilibrium model. It does not take into account the intermediate input linkage between industries and it does not deal with the outputs of those goods and services that are local.

behave in this way because they treat capital as perfectly mobile, make essentially the same assumption about labor, and do not have land-resource constraints. It is as if all regions always have unoccupied space that is equal in quality to occupied space, so that land has a zero price. The authors reason in this way even though three of the papers use BEA's as the unit of regional analysis. These are relatively small areas.

One of the papers does pay some attention to the price of space. Harris defines location rent as the sum of profit and the rental value of land per unit of output.[13] However, he indicates later that his industry location equations have been modified. "The marginal transportation costs of obtaining inputs and the labor costs have all been converted into components of location rent and combined into a single variable.[14] No mention here of land rents. Furthermore, I find no market clearing equations for land in the Harris model and no way in which changes in the level of economic activity of a region generate changes in land prices, as distinguished from location rents. I interpret the latter as returns to fixed capacity. If I am correct in this judgement of the model, then it, as well as the non-optimizing input-output models always convert an increase in demand for a region's export product into an increase in all other outputs of the region. The location rents of all industries either increase or remain unchanged because expansions in output do not generate increases in the prices of inputs or cause costs to increase because of agglomerative diseconomies.

What is more likely to increase in price than the immobile factor, land and the resources we are using it to represent? It strikes me that a sound and realistic model of regional specialization, growth,

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[13] p.5

[14] p.11

and trade would have industries that are especially well-suited to a region bid up the price of space when they expand. Other industries are forced to leave the region because their costs go up. At least some of the labor released by the declining industries moves into the expanding industry. As a result, there is less need for migration than in models in which the price of space is implicilty zero. Since the expansion of the efficient export sector is accompanied by a decline in other industries whose products must then be imported, the workings of comparative advantage also provide a tendency toward balance of trade equilibrium.[15]

Climate and the quality of resources are not completely absent from the papers. In the full study on which Olesen bases his paper, Sun Belt is entered as a dummy variable in a regression. The authors find that the variable is positive and significant and conclude that the area has a special drawing power for industry. There are no reports of investigations into the effect of Sun Belt on the costs, revenues, or profits of firms, so that the statistical result is without conceptual content at this stage. The Kresge model differs from the others because the unique resource situation of the area is emphasized. He indicates that the outputs of Alaska's resource industries are determined in an economic model in which, among other things, world prices and local supply conditions enter. I assume that supply conditions include the effects on exploitation costs of output rates, but nothing

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[15]Some studies deal with large regions. It may be argued that in such cases there is no need to be concerned with land constraints or the price of space, but this is incorrect. It is a way of trying to aviod a theoretical problem that has important empirical implications by resort to an aggregation argument. Suppose we are analyzing the Massachusetts economy. Should we ignore the influence that the price of space has on the industries that locate in the state's major urbanized areas. If that is the approach adopted, projections of the entire state's output will have serious errors. I have used Massachusetts as an example because Treyz states that future versions of his model will divide the state into seven sub-areas.

of a detailed nature on this relationship appears in the paper. Neither the Olesen or the Kresge papers has the growth of industries in a region that are especially suited to it raise land-resource prices and thereby costs of production of other industries, bringing a reduction in the region's output of them, an increase in imports, and a tendency toward balance of trade equilibrium.

I cannot prove the point by presenting empirical evidence but feel that a good deal of the trade between regions is the result of scale and agglomerative economies. Few counties, BEA's, or even states can have an integrated steel mill, an automobile assembly plant, or a petroleum refinery. Which states have them depends on resource-market location and transportation. However, even if a number of areas are equally good as a location for such industries, scale economies restrict the number that will actually have them. All others must import their products. Models that ignore scale economies cannot explain a great deal of the trade in such goods. If they also ignore land and its price, they are unable to explain why, if an area has an industry that is characterized by great scale economies, it does not have a large number of other industries and must import its requirements of them.

Several of the authors express the view that agglomerative economies play a very important role in explaining regional specialization and trade. However, they do nothing with them if they are properly interpreted as a scale phenomenon and not simply as the transport savings realized when firms that use each other's outputs as inputs, locate near one another. The latter, associative economies, do not depend on scale. They would exist even if all transportation activities involved zero scale economies because transport costs increase with length of haul. I would like to consider agglomerative

economies before taking up transportation.

Olesen claims that agglomerative economies are fundamental to the approach adopted in his study. "....we accepted the notion of central place theory that space may be partitioned into a hierarchy of trading areas that cause regional economies to be interdependent".[16] Aggalomerative economies go beyond service industries and trade in services but let us accept Olesen's more restricted view of them. This is an area where regional analysts have done a great deal of theoretical and empirical work, much more than has been done by foreign trade specialists. The latter tend to have a commodity-resource orientation and to ignore trade in services.

I suspect that trade in services is very important and probably provides a great deal of the export income with which many large metropolitan areas pay for their imports of manufactured and agricultural goods. Unfortunately, the authors, including Olesen, do nothing with central place theory and such trade. After stating that these things are fundamental to the approach adopted in his study, all that Olesen does is perform time series studies of regional service industry employment shares. These lead him to two conclusions: inertia is the most important determinant of BEA employment in these industries; areas are becoming more alike in such employment. If they are becoming more alike in such employment, are they also becoming more alike in service outputs? If so, does this mean that trade in services is declining and the hierarchy of central places gradually vanishing? If so, does this mean that places like New York will have to attract large numbers of manufacturing firms in the future if they are to achieve trade and employment equilibrium and avoid large scale out-migration of labor over time? These are the kinds of questions that do not appear in the Olesen

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[16] p. 4

paper, but the same is true of the other papers. With the exception of the Treyz paper, they all ignore trade in services. Treyz has a table that shows the percent of Massachusetts employment dependent on exports by industry.[17] The service sectors appear in this table, but the figures are probably the result of simple input-output calculations. I am therefore sceptical as to how much they tell us about trade in services. I would now like to consider transport costs and what I have called the associative economies.

Transportation costs and the quality and nature of transportation networks have long been viewed by regional, as opposed to foreign trade, analysts as important determinants of the volume, commodity content, and direction of trade. Yet, with the exception of Harris, the authors of the conference papers are quite inadequate in their analyses of the ways in which transportation operates to influence regional specialization and trade. In the Harris model comparative advantage does have a spatial as well as a production dimension. He recognizes and tries to deal with the fact that a change in even one transport rate can alter all transport flows, outputs, and location rents and result in changes in the geographic patterns of all industries. He tries to take into account the associative effects of transportation. Thus, a movement into a region by an industry is seen as possibly influencing all prices in the region, and location rents, as well as all outputs and trade patterns.[18] I am not saying that the Harris treatment of transportation is without problems, but that it is conceptually superior to the other papers in this regard.

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[17] p. 16.

[18] There is a problem concerning price changes that I will take up later.

Transport costs and rates are of course somewhere in the background of the fixed trade coefficient models but that is as much as can be said about their role in these models. Suppose the analysts who use this approach know that great relative changes had taken place in commodity and regional transportation costs since their trade coefficients were calculated, what would they do with the information? They could guess about the effects on the trade coefficients or wait for new interregional data to appear and then calculate new coefficients. Anything of a conceptual nature would mean abandoning the model.

I fail to find in the Alaska and Massachusetts models an analysis of the effects of transport costs on regional specialization and trade. I suppose this difficulty is inherent in top-down, single region models. It is not possible to deal with transportation meaningfully when shipments take place from a single area to the rest of the nation. Nevertheless, in the Alaska study I think it would have been especially interesting and worthwhile to try and determine the effect of transportation costs on the benefits the state derives from trade with the lower 48.

There are no transportation networks or costs in the Ballard model, only undifferentiated distance. Its relationship to comparative advantage and the associative economies is so remote as to be trivial. Interregional transport costs and rates change over time relative to each other and relative to regional factor prices and production costs. These things play a vital role in determining the changes that take place in the commodities regions produce, in the areas from which they acquire their imports and to which they ship their exports. Gravity models are quite useless for analyzing such things since distance does not change. The Olesen model, which also tries to explain interregional relationships through separate gravity models for each manufactur-

ing industry is a bit superior to the Ballard model because it uses Interstate System truck travel times between areas rather that distance, but it also teaches us nothing about how transportation helps to determine the lines of activity in which regions have advantages relative to one another.

The Friedlander model deals specifically with the effects of freight rates on regional production and employment. This is the paper in which one should find a thorough analysis of the influence of transport costs on regional comparative advantage and on what I have been calling the associative economies, but that is not the case. Friedlander develops time series data for home and delivered prices, by region and industry. Home prices are defined as regional value added divided by regional shipments. Delivered prices are obtained by adding transport costs to home prices. The model's transport costs come from time series data for truck and rail revenues and shipments. The cost of transporting a commodity from a region r to a region s is a weighted average of rail and truck revenues per ton-mile between the two. A cross-section time series model is then run in which home and delivered prices of a commodity group are used to explain interregional shipments. Friedlander reports getting good statistical results. The estimated equations are then used to predict the effects of a change in freight rates on a region's shipments, on its output, which is defined as the sum of shipments, and on employment.

The Friedlander model does not formally incorporate the influence of associative economies because it does not have intermediate inputs and transport costs on such inputs. It is the essence of these economies that an expansion in the output of a commodity in a region tends to reduce the prices home firms pay for it. An industry does not normally expand in a region unless it has some combination of a production and transport cost advantage. A fall in the home price of one commodity group reduces costs of production of other industries. Earlier I argued that induced changes in land rents do exert an opposite effect,

but the Friedlander model does not have land and rents in its production and cost functions. The model proceeds as if all commodities are strictly for final consumption and produced exclusively with non-transported primary factors of production, labor, capital, and energy. Since there are no linkages between industries, the model must understate the total effects on all industries in a region of changes in freight rates. The model is in trouble if the Friedlander commodities serve as intermediate inputs as well as final goods. Each home price is then the sum of per unit capital, labor, and intermediate input costs. However, the latter depend on home prices and transport costs. I believe that Friedlander assumes production only requires non-transported primary factors, and also assumes that factors are available to each region in unlimited quantities at constant factor prices, because she wishes to avoid problems of simultaneity. Input-output models are designed to catch the effects of industry interdependence but Friedlander rejects the assumption of fixed technical coefficients. I accept her reasons for doing so and will shortly explain why I share her views. Nevertheless, the fact remains that her model is incomplete and does not estimate the full effects on shipments, outputs, and employment of changes in freight rates.

As stated earlier, the Friedlander equations use home and delivered prices as explanatory variables. Her regions are large, which means that intra-regional per unit transport charges are large, but they are not used in the empirical work because the model then underestimates intra-regional shipments. Instead, dummy variables are entered for such shipments. This is a troublesome point. Given the size of the regions used in the study, it would not be surprising if two-thirds of all shipments are intra-regional. Yet, this means that a catch-all variable rather than the economic variables of the model are explaining most of the shipments. It may

be that the substitution of dummy variables for intra-regional transport costs also helps the model in its explanation of interregional shipments because it tends to eliminate cases in which a region ships to another region even when its true home price, which includes intra-regional transport costs, exceeds delivered price. Given the level of regional and commodity aggregation with which Friedlander must work, there are probably a significant number of cases in which regions ship large amounts to each other despite the fact that each of their delivered prices exceeds the other's home price.

Let us bear in mind that the Friedlander model does not deal with freight rates but with indices in which shipments by the modes are the weights. The indicies are then treated as if they are the rates of a group of firms that offer a homogeneous transportation service in the carriage of a single homogeneous product. The effects on shipments and outputs of a change in "the rate" charged by these firms is then studied. Unfortunately, an index number is not a measure of the relevant cost. P_{irv}, the delivered price of "commodity" i in region v when shipped from region r is the home price in i plus the freight charge of the marginal transportation mode, if more than one actually makes such deliveries. Delivered price is not the sum of home price and an average of the charges by the modes. Finally, the empirical results may also be misleading because interregional differences in transport charges and the changes in transport charges over time that enter her estimations are probably more the result of differences in and changes in the commodity composition of shipments within a group than they are the result of real changes in transport rates. I would now like to turn to issues related to the use of input-output and other techniques.

Part Two: Quantitative Techniques and Comparative Advantage

This part of my paper deals with the techniques that the authors of the conference papers have used to represent, to analyze and to project the way in which comparative advantage functions in an open economy. Since input-output techniques form the logical core of all but one of the papers, their relationships to the above matters will receive special emphasis. I begin with national input-output models because their outputs become data inputs or serve as control totals in many regional models. The national model also serves as a useful pedagogical device for introducing some very serious problems that regional studies encounter because they use input-output techniques.

More than forty years have passed since W. W. Leontief's first input-output publication. A great deal has been written over these years about the stability of the technical coefficients, the a_{ij}'s. It has been argued that it is safe to assume they are quite stable because technological change takes place slowly. Empirical work has been done and the coefficients compared over time. Some authors have, it seems to me foolishly, even compared coefficients for different countries. Such empirical work and the entire argument about technological change and the stability issue seems largely irrelevant to me. It loses sight of the fact that Leontief really made two assumptions. He assumed that production functions were characterized by fixity of physical coefficients. Then, because it was impossible to obtain data for physical coefficients, he assumed they could be represented by value coefficients. The logic and empirical usefullness of input-output models was supposedly unaffected by the shift. I do not accept either of these conclusions. If production functions really do entail fixed physical coefficients, the value coefficients must change with every change in relative prices because each value coefficient is the product of an underlying physical coefficient and a relative price. Thus,

a_{ij} is obtained by dividing the value of the flow of industry i to j by the value of the latter's output:

$$a_{ij} = \frac{x_{ij}}{X_j}, \text{ but}$$

$$x_{ij} = q_{ij} \tilde{X}_j P_i$$

where q_{ij} is the underlying physical coefficient, $\tilde{X}_j$ is the physical output of the jth. industry and P_i is the price of industry i output. The value of industry j's output is:

$$X_j = P_j \tilde{X}_j .$$

The value coefficient is then:

$$a_{ij} = \frac{q_{ij} \tilde{X}_j P_i}{\tilde{X}_j P_j}$$

$$= q_{ij} \frac{P_i}{P_j} .$$

As stated, each value coefficient is the product of an underlying physical coefficient and a relative price.

Even if production functions can be meaningfully approximated by fixed physical input coefficients and even if technology changes slowly, there is no reason to believe that relative prices cannot change rapidly, even erratically. They may do so, for example, because products have different primary factor intensities and the supply functions of primary factors have different elasticities. In this case each relative shift in final demands will cause changes in factor and commodity prices, with the result that all a_{ij}'s will change. It is not reasonable to answer this criticism with arguments that imply a single primary factor of production or which admit the existence of more than one primary factor but assume that each has a constant supply price. The latter argument must be cast in long run real terms since nominal wages, profits, and rents are known to change quite dramatically relative to one another in the short run.

However, this means that input-output is not appropriate for short run analysis because of changes in relative factor prices, and not appropriate for long run analysis because that is the period in which technological change takes place.

The problems caused by changing relative prices are especially serious because input-output tables are expensive to construct and are used for years before being replaced. If the a_{ij}'s vary greatly over time because of changes in relative prices, then the outputs, employment, and incomes obtained using old tables are full of errors. Input-output specialists sometimes try and avoid the price issue by claiming that their projections are in real terms, the prices of some base period. Such a position does not seem supportable to me. Thus, suppose an input-output table had been constructed for 1974 and it is known that no technological change has taken place since then. We wish to use our 1974 table in 1980 to forecast outputs etc. in 1981. Should we ignore the very significant changes in relative prices that have taken place since '74 and estimate final demands in terms of the relative prices of that year? Surely not, for the final demands and resulting outputs would then have serious errors. If the influence of changes in relative prices on final demands is taken into account we have a logical difficulty. The analysis involves two sets of relative prices, one for the final demands and another for the value coefficients and output calculations.

The use of unchanged a_{ij}'s can also introduce systematic errors in output estimates. In this regard, assume that P_i has risen relative to all other prices. Use of the hypothetical 1974 table means that coefficients in the ith row are smaller, and those in the ith column are larger than the true coefficients. These errors are of course spread out all over the inverse matrix. The errors they can produce in estimated outputs can be both large and systematic. Up to now I have accepted the Leontief assumption that production functions are characterized by fixity of physical input

coefficients and examined some of the difficulties that follow when a_{ij}'s are treated as if they are physical coefficients that are unaffected by changes in relative prices. I would now like to raise another problem.

Suppose empirical work reveals the a_{ij}'s are relatively stable during a period in which relative prices are changing substantially, This should cause concern rather than bring relief. It probably means that the basic assumption of input-output analysis is incorrect and that production functions admit of significant input substitution. I have one last point to make on the difficulties associated with the use of input-output at the national level before taking up the implications of these same difficulties in regional studies: use of value coefficients can change the basic logic of both static and dynamic input-output models. I use the national price model as an example because Ms. Polenske urges the use of the dual or price version of her interregional model for the analysis of regional price changes.

Suppose for some base period we have actual physical outputs of all industries, underlying physical input coefficients, all factor payments and all goods prices. As an amusement we might run an input-output model of price determination:

$$P_i - q_{1i}P_1 - q_{2i}P_2 - \cdots - q_{ni}P_n = \Pi_i \ ,$$

when the Π_i's are the final factor payments per unit of output. If the Π's and q_{ij}'s, the physical input coefficients, are correct the model will yield the already known base period prices. Now suppose, to wile away still more time, we use base period data to compile value coefficients and solve the price model again:

$$P_i - a_{1i}P_1 - a_{2i}P_2 - \cdots - a_{ni}P_n = \Pi_i \ .$$

Since each value coefficient is the product of an underlying physical input coefficient and a relative price, this system of equations amounts to:

$$P_i - (q_{1i}\frac{P_1}{P_i})P_1 - (q_{2i}\frac{P_2}{P_i})P_2 - \ldots - (q_{ni}\frac{P_n}{P_i})P_n = \Pi_i .$$

But what we now have is a system of similtaneous quadratic rather than linear equations. It is unlikely to yield the prices with which we started and which were used to compute the base period value coefficients. In other words, a value coefficient model of prices is not internally consistent. What happens when a value coefficient model is used to determine prices in some future period? The forecast prices are then an average of base period and true forecast period prices. If the Π_i's have risen, the value coefficient model will understate the increases in prices and also distort them relative to one another. I would now like to turn to some of the implications of the above difficulties in regional models.

Regional input-output models are basically of two kinds. The purely interregional are one type, the top down single or multi-region models another. One of the ways in which they differ is in their treatment of national outputs. Such outputs are a product of the operation of a strict interregional model. They are determined in a separate national input-output calculation and provide data inputs in the top-down models. If the claims that I have made about the instability of value coefficients in periods of changing relative prices are correct, the outputs yielded by outdated input-output tables are a very weak foundation on which to build a top-down regional model, but this is only the first in a series of problems.

Regional models currently use national coefficients. Analysts do try and take into account product-mix differences between regions by aggregation. That is, regional models often employ a less detailed industrial classification than the one employed in the national table. Sub-sets of the national coefficients are aggregated by a method that employs regional weights. Employ-

ment figures are frequently used because they are available for the more detailed industrial classification. The resulting, aggregated a_{ij}'s are called regional technical coefficients. They differ from the coefficients of a comparably aggregated national table but only because of differences in weights. They do not differ from the national value coefficients in a theoretically meaningful way. The use of such value coefficients involves the implicit assumption that every region has the same underlying physical input coefficients, the same relative commodity and factor prices, and hence the same relative factor endowments. Earlier I suggested that the theory of intra-national trade implied by many regional studies is one in which there is no comparative advantage, no regional specialization and no trade in long run equilibrium. The use of regionally identical a_{ij}'s, and I ignore the appearance of differences brought about by an aggregation sleight-of-hand, implies that the system is already in such isolation equilibrium.

Much more serious is the fact that it is not possible to explain current patterns of regional specialization and trade with models that assume regionally identical costs of production. If transport costs increase with distance, the only trade that can logically be explained by such models is that due to short run capacity limitations. A region imports a good because it does not have enough capacity to satisfy home requirements; a good is exported because another region has encountered a capacity constraint. That is what is implied in the use of national value coefficients for each region. It may be claimed that my criticisms of regional input-output studies are unfair because regional analysts would like to use regionally differentiated coefficients but they are not available. To this I respond by raising a question and offering an answer. Namely, how would the situation change if regional input-output analysts had their wish and regionally differentiated value coefficients were available: would they be

that better off? I believe that many analysts, including some who have presented their work at this conference and others who have commented on the papers, will find that they are not at all pleased by what such coefficients imply.

My argument on regionally differentiated a_{ij}'s can be explained equally well in either of two contexts, that of a formal spatial competition model of the Samuelson-Takayama variety, or a real world situation. Regions are treated as points in the former, the product is homogeneous and the industry perfectly competitive. In equilibrium, price differences between regions are less than or equal to transport costs. All consumers in a region pay one delivered price, all producers in a region receive one net price. There is no crosshauling, and regions do not both export and import the good. Empirical studies deal with regions that are often quite large, and with industries that include many different products. In such a real world situation, commodity flows suggest a great deal of crosshauling. Purchasers of a "good" pay different prices for an input either because they have different sources of supply or because they are really buying different goods. The same is true of the net prices received by the firms in a region that sell a "good." I choose to develop the argument for what is essentially the real world situation. However, in order to eliminate some very cumbersome notation I will assume that each industry produces a single homogeneous product. Therefore my presentation emphasizes the regional and leaves out the additional difficulties that grow out of industrial aggregation.

Consider the value coefficient a_{ijv}, the dollars worth of input i required to produce a dollars worth of output j in region v. To calculate this coefficient we need the value of all purchases of i by j- type firms

in v from all regions. Let us assume that only two regions, r and s, made such shipments to v in the period for which we are deriving the coefficients. We also need the value of output of the j^{th} industry in region v. It is the sum of the regional shipments. Again assume that there are only two regions to which the product is shipped, m and v itself. The value coefficient is:

$$a_{ijv} = \frac{x_{ijrv}}{X_{jvm}} + \frac{x_{ijsv}}{X_{jvv}} .$$

These flows are obviously the products of physical flows and regionally differentiated prices and freight rates:

$$a_{ijv} = \frac{\tilde{x}_{ijrv} (P_{ir} + t_{ivv}) + \tilde{x}_{ijsv} (P_{is} + t_{isv})}{\tilde{X}_{jvm} (P_{jm} - t_{jvm}) + \tilde{X}_{jvv} (P_{jv} - t_{jvv})}$$

Above, the $\tilde{x}$'s are physical interindustry flows, $\tilde{x}_{ijrv}$ being the physical flow of the i^{th} output from the r^{th} region to the j^{th} industry in the v^{th} region. The P's are prices. P_{ir} is the price of industry i output in region r. The t's are per unit transport charges. t_{irv} is the cost of shipping a unit of industry i output from region r to region v. The sum of a regional price such as P_{ir} and the associated transport charge, t_{irv}, is of course delivered price. $\tilde{X}_{jvm}$ in the denominator is that part of the total physical output of industry j output in region v that is shipped to region m. $\tilde{X}_{jvv}$ is the remainder of the output, in our case intra-regional shipments. The regional prices and transport charges in the denominator yield net prices. We must still introduce the underlying physical coefficient, but a preliminary comment on the stability of regional value coefficients seems in order at this point.

Earlier it was observed that national value coefficients are undoubtedly unstable because they depend on relative prices. As we can now see, the situation is much worse for regional value coefficients. Each national coefficient depends on only one relative price. Each regional value coefficient depends on the prices of all regions that sell a given input in a region and the different transport charges on those shipments. As we can see, a_{ijv} also depends on the prices that prevail for product j in those regions in which v's producers sell it and on the costs of transport to those regions. Is it reasonable to assume stability for coefficients whose values depend not only on P_i/P_j, as in the national case, but this price relative expressed in terms of every region? And, if that were not enough, they depend on all transport charges as well.

At this point it should be clear that regional analysts who argue the need for regional "technical" coefficients while also rejecting fixed trade coefficient models are involved in a contradiction. They cannot have stable regional "technical" coefficients without assuming the stability of trade coefficients. Thus, consider x_{ijrv} and x_{ijsv}, the two value of input flows that enter into the calculation of a_{ijv}. To see what they entail let us introduce the underlying physical coefficient, q_{ijv}.[19] If this coefficient is multiplied by physical output we have the total physical quantity of input i used by industry j in region v:

$$\tilde{x}_{ijv} = q_{ijv} \tilde{X}_{jv}$$

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[19] The situation is essentially unchanged if v is left off and the assumption thereby made of regionally identical physical coefficients. Regional costs and commodity prices can still differ because primary factor endowments and therefore prices differ.

The physical flow $\tilde{x}_{ijv}$ is converted into a value flow by multiplying by price. The difficulty is that region v receives shipments from two areas, r and s, which have different prices for i and different transport rates.

$$x_{ijv} = \tilde{x}_{ijrv} (P_i + t_{irv}) + \tilde{x}_{ijsv} (P_i + t_{isv}).$$

Obviously we have an index number problem with the physical flows, the weights. If the weights change relative to one another, x_{ijv} will change even if the total physical flow is unchanged. Unchanged weights means fixed trade coefficients. In other words, the value flow x_{ijv} can be represented as follows:

$$x_{ijv} = q_{ijv}\, b_{irv}\, \tilde{\tilde{x}}_{jv} (P_{ir} + t_{irv})$$

$$+ q_{ijv}\, b_{isv}\, \tilde{\tilde{x}}_{jv} (P_{is} + t_{isv}).$$

The two b's are the trade coefficients. For example:

$$b_{irv} = \frac{\tilde{x}_{ijrv}}{\tilde{x}_{ijrv} + \tilde{x}_{ijsv}}.$$

Analysts may continue to urge the need for regional technical coefficients. However, they should recognize that in doing so they are accepting the validity of the fixed trade coefficient model. I do not believe that trade flows are stable, though they can appear to be so when calculated for large regions and an industrial classification that is highly aggregated. Let us now consider what all of the above means for the papers presented at the conference.

Three of the papers, those by Ballard, Treyz, and Kresge, allow regional wage rates to adjust to labor market conditions. Olesen claims that his model allows local labor supply and demand conditions to influence wage rates. I think models should generate changes in the prices of land and resources as well as labor as regions grow and decline. Changes in relative factor prices

between regions is one of the most important ways in which shifts in regional comparative advantage are expressed over time. They also help establish a tendency toward trade equilibrium, which reduces the need for labor migration. However, change in the structure of regional wage rates means changes in prices and trade flows. If the models did employ regionally differentiated "technical" coefficients, the coefficients would be unstable. In this regard, the Polenske output model--and I will turn shortly to her price model--and the Haveman model are at least logically consistent. They employ fixed trade coefficients but make no mention of the influence of output changes on factor prices. In effect they assume that every region has a perfectly elastic supply of every factor.

On its surface, the Harris model is more appealing theoretically than the other models because it employs optimization techniques rather than fixed trading patterns. However, it also has serious difficulties and its surface appeal is somewhat deceptive. Harris uses location rents to predict and explain changes in the locational pattern of industry. He correctly observes that ".... if there is a movement of output to more favorable locations in any one industry, the prices of all commodities and inputs will change causing location rents and profits to change for all industries in all regions".[20] He is correct but it should also be noted that such regional price changes mean that intermediate input costs, that is the so-called technical coefficients, have changed. Harris makes no effort to adjust the coefficients. What will he do if regional "technical" coefficients ever become available? He rejects fixed trading patterns in favor of an optimization technique that determines trade flows. However, the regional "technical" coefficients used to determine regional outputs will actually entail fixed trading patterns, and fixed regional prices and transport costs. To achieve logical consistency the Harris model must have a way of altering the "technical" coefficients each time one of its

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[20] p.8

recursive solutions indicates a change in prices. It will be difficult to devise a technique for doing this since each change in intermediate costs may alter all location rents. Because it is recursive, Harris' model could easily wander aimlessly back and forth, telling an industry to leave a region in one solution and sending it back in the next.

I would now like to comment on the Polenske price model. As stated above, the output model implicitly assumes that all supply schedules, factor as well as commodity, are perfectly elastic. It also assumes away capacity constraints in both production and transportation. We may not like these assumptions but they are the ones with which we must live if we are going to treat regional "technical" coefficients as stable. The Polenske output is at least internally consistent. The same cannot be said of the price model. A set of trade coefficients, which depend for their stability on relative regional prices and transport charges, and a set of "technical" coefficients which also assume unchanged relative prices are used to determine changes in the prices of all goods in all regions that will follow some exogeneous change. Earlier I showed that the logic of a national price model based on value coefficients is deeply flawed. The Polenske interregional price model faces even more serious logical problems. I suggest that it be abondoned.

In summary, I am skeptical about the theory of interregional trade and comparative advantage on which the models presented at this conference appear to be based. It is obvious that I am troubled by the use of input-output techniques at the national level and much troubled by their use in regional models. Perhaps my attitudes resemble those of a politician of some renown in Chicago. When confronted by demands for a reduction in patronage jobs, the introduction of sealed bids on contracts etc., he commented that" Chicago ain't ready for reform". On the other hand it may really be that the state of

theoretical understanding and data are such that meaningful regional analyses of the general equilibrium variety cannot now be performed. There are many worthwhile things that can be done at the partial equilibrium level while progress is made on the theory of intra-national economic relations, and decisions are made as to the best quantitative techniques for implementing that theory and the data needed for such implementation.