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Regional science: The state of the art

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A review of P. Nijkamp and E.S. Mills, eds. Handbook of Regional and Urban Economics, Volume 1: Regional Economics, (North-Holland, Amsterdam, 1986) pp. xxii + 702, ISBN 0-444-87969-2.

1. Conceptual issues

1.1. Introduction

The study of spatial phenomena is labeled by many terms. Three of them are regional science, regional economies, and urban economics. The progression tends to be from operational modeling to economic theorizing. Regional science is a multidisciplinary tool box used to address macro issues such as the determination of output and employment across regions. Regional economics focuses on the economics within a given region, while urban economics casts the analysis in a microeconomic framework with land-consuming households. However, the progression from regional science to urban economics could also be considered to be a progression from the most general to the most specific. In economics, agents have fixed locations; the only role of location is as a device to differentiate commodities. General equilibrium analysis can be used to explain the supply of and demand for commodities, differentiated by location, along with the resulting trade flows. The location of consumers is endogenized in urban economics, conditional on firm locations. In other branches of the regional

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science literature, consumers have fixed locations while firms are free to locate. Regional science has the potential to explain simultaneously the supply of and demand for commodities as well as the locations of all agents by region, called 'location–allocation' problems.

In the *Handbook of Regional Economics*, editors Nijkamp and Mills (1986, p. 17) proclaim that the discipline is mature. They provide the following arguments.

> In fact, it was not until the mid fifties that regional economics came into being as an accepted analytical framework for studying the implications of geographical location–allocation problems. Since then, regional economics (and later, urban economics) has made formidable progress in achieving a further understanding of the structure and evolution of spatial economic systems. (. . .) As such, it is also able to generate a unifying approach to problems emerging at the cross-roads of economics and geography (Nijkamp and Mills, 1986, p. 1).

It goes on. ". . . regional economics many claim to have provided a real contribution to economic theorizing and analyzing . . ." (Nijkamp and Mills, 1986, p. 2). Unified sciences with accepted analytical frameworks lend themselves to consolidation in handbooks. The main questions can be identified, the framework is laid out, the tools of analysis are given, and the powerful theorems are derivable and applicable. The claim that regional science has acquired the status of a mature discipline is noteworthy, as it was conceived only a few decades ago. In fact, this time span is so short that the development of regional science might be classified as a scientific revolution in the sense of Kuhn (1970).

In this paper we attempt to assess the achievements of the field. Does the state of the art, as consolidated in the *Handbook*, constitute a unified framework for location–allocation problems? We speak of the state of the art of regional science rather than regional economics, since the former is a wider framework that accommodates interregional allocation problems. However, our assessment will be from the point of view of economic theory.

Our review of regional science is limited to Volume 1 of the *Handbook*. Volume 2 (Urban Economics) is deliberately excluded, at least in this review, in order to focus on location–allocation problems. This dissection is somewhat arbitrary. The Beckmann and Thisse paper in Volume 1 might be considered to be urban economics, while the Henderson paper in Volume 2 might be considered to be regional economics.

1.2. Relationship to economics

In principle, the scope of the field extends that of economics. Economics is confined to the problem of allocation of factor inputs and commodities across households and firms in a static or dynamic context. *Some* location problems, those featuring fixed locations of agents but not commodities, can be subsumed by economics, using the device of differentiating commodities not only by their physical qualities but also by their locations. This procedure, called locating, differentiates Kansas from Missouri wheats like steel from textiles. Regional scientists correctly argue that the economist's method of locating is trivial and mechanical, without accounting for indigenous spatial features. In the context of production theory, for example, input–output analysis runs into some serious problems with this approach. The non-substitution assumption may be reasonable for steel and textiles as well as economies with a single factor of production (Samuelson, 1951), but by subjecting input–output analysis to the locating device, one would implicitly assume that Kansas and Missouri wheats are complements instead of substitutes. Moreover, wheat flow would be 'explained' by fixed coefficients for Kansas and Missouri wheats, without any economics of allocation such as transport cost minimization. Regional science, on the other hand, attempts to explain the location of *both* economic agents and commodities, along with the allocation of the latter over production and consumption activities. By fixing the location of agents, the field of economics is subsumed. While economics trivializes geographical space into an index set, regional science not only preserves the richness of geography, but also the usual economic detail. In this sense, the scope of regional science is wider. Economists face a rather limited job by comparison. We may conclude that if the accomplishments of regional scientists have reached the same maturity as those of economists, as suggested by Nijkamp and Mills, the achievement is even more impressive.

Economics is the study of agents (firms and households) and commodities (factor inputs, goods and services). Firms have production possibilities which are described by a subset of the commodity space. Preference relationships on the latter describe the tastes of households. Households also have initial endowments of commodities. Prices and quantities are endogenous. How about geographical space? In the *Handbook*, it is a set of finitely or infinitely many points (representing locations) with a metric (representing distances between them). The main division between regional and urban economics, as exemplified by the division of material between the two *Handbooks* (Nijkamp and Mills, 1986, 1987), seems to be the modeling of geographical space. Urban economics involves the use of quantifiable, differentiated land, while regional science requires no geographical detail other than an index set of locations and possibly a matrix of distances between locations with a list of total land endowments.

Each firm or household has one location, either fixed exogenously or to be determined endogenously. When both firms and households have fixed locations, all that remains to be explained is the reallocation of commodities between firms and households. This amounts to the canonical transportation problem addressed in economics. The most explicit treatment is in the chapter 'Spatial Equilibrium Analysis' by Takayama and Labys.

1.3. Location theory

If the location of firms is made variable, keeping households fixed, we enter the realm of location theory proper, with the most detailed account given by Beckmann and Thisse's 'The Location of Production Activities'. Conversely, if the location of households is made variable, with fixed economic opportunities, we are either in the realm of urban economics or of spatial interaction theory. Urban economic models treat the use of land explicitly and are a subject of a separate *Handbook*. Spatial interaction theory is a cornerstone of regional science and is treated by Batten and Boyce; the main contribution of the theory is a way to disaggregate the volume of interaction of a location with the rest of the world into bilateral flows. One might say that while the transportation problem constitutes partial equilibrium analysis with all agent locations fixed, location theory and spatial interaction theory are partial equilibrium analyses with only some of the locations fixed. Only when all locations are free to vary might one speak of general spatial equilibrium analysis. The editors of the *Handbooks* are explicit on this (p. 3).

> Next, the joint decisions of entrepreneurs and households lead to the problem of the existence of spatial economic equilibrium. This is evidently a complex problem, as the allocation of scarce resources and their spatial location may involve various substitution possibilities, so that complicated trade-off questions may occur.
>
> The latter problem is further aggravated if spatio-temporal dynamics caused by structural changes are taken into account. (. . .)
>
> In light of the foregoing remarks, part 1 of the present Handbook is composed of five main contributions to modern location theory, viz:
>
> • the location of production activities (Chapter 2)
> • residential mobility and household location (Chapter 3)
> • public facility location (Chapter 4)
> • spatial equilibrium analysis (Chapter 5)
> • regional economic dynamics (Chapter 6).

Consider this list in order. In the location theory of production activities, we have a set of locations, households with demand functions and fixed locations, and mobile firms with technologies. The fixed locations of households are typically given by a uniform distribution. The technologies are independent of location, except for delivery cost. Firms choose location and either price or quantity (corresponding to Bertrand or Cournot equilibrium analysis, respectively). The equilibrium firm locations and supplies are thus contingent on the underlying spatial distribution of households. Interesting elements of solutions include the spacing of firms and the price pattern between them. Households attracting close firm locations are lucky as they incur low transport costs. This feature draws our attention straight to the next issue, the question of residential mobility and household location. The main chapter on this subject is not good, particularly with regard to the delineation of issues. Subsection 2.2 is titled 'Defining and characterizing housing markets.' A clue to the insight offered in this chapter is: "The character of the existing stock [of houses] is the context against which household location takes place" (p. 100).

This point of view reduces the question of household location to an assignment problem over dwelling units, the locations of which are exogenously fixed. The problem is solved by linear programming, gravity and entropy models, log-linear models or discrete choice and random utility models. The latter approach is preferred as it provides "... the most coherent and integrated analysis of residential mobility and locational choice" (p. 115). Utilities of alternative dwellings are postulated directly and the maximand is determined without further ado, except for stochastics to model taste variation across households. The utilities assigned to the dwellings are not derived from primitives like (hedonic) commodities and proximities. Prices and incomes are treated only implicitly in the discussion, and are exogenous to the models. Household location theory does not meet the standard set by firm location theory. In principle, however, there is no conceptual objection to a parallel development; such a theory would yield equilibrium household locations contingent on an exogenously given spatial distribution of firms.

Spatial equilibrium analysis could then finish the job. Household and firm location theories are like simultaneous equations. An attempt to determine equilibrium household and firm locations simultaneously is given by Papageorgiou and Thisse (1985). To prevent customers from flocking close to supply points, they must impose a spatial externality which, unfortunately, remains unexplained. A natural candidate to explain such a spatial distribution of customers is explicit modeling of residential land use and explicit land (not location) pricing. Alternative formulations of the residential land use component of such a model can be found in Berliant (1985) and especially Berliant and Fujita (1992). One model presenting a unified framework with land is Fujita and Thisse (1986).

1.4. General equilibrium

As mentioned, this line of development is not followed in the *Handbook*. The approach to the location of production activities constitutes a fitting building block, but the approach to household location does not. This raises the interesting question of how the problem of general spatial equilibrium analysis is handled in the *Handbook*. One of the features of the *Handbook* is that the chapters do not fit in a common structure. Would it not be reasonable to expect that firm and household location theories provide the elements of supply and demand for general spatial equilibrium analysis? Well, the problem of equilibrium analysis is treated by Takayama and Labys with total neglect of the preceding chapters developing theories of the firm, household, and public facility location.

In fact, the general spatial equilibrium model of Takayama and Labys involves *fixed* locations of firms and households, returning to the economic problem of the allocation of commodities in the presence of geographically fixed factors of production. This is the well-known transportation problem subsumed by economics, particularly international or interregional trade theories. It is *not* general spatial equilibrium analysis!

A conclusion that one might draw from this is that the *Handbook* does not give a fair account of regional science as a science that explains simultaneously location and allocation problems. It may provide a review of aspects, elements, and techniques, to which we shall turn below, but offers little framework for consolidation.

Papageorgiou and Thisse (1985) is 'mainstream' regional economics. They attempt to explain locations and allocations of households, firms, and commodities without recourse to exogenous geographics such as fixed resource distributions or climate amenities. In fact, in their model all firms are identical and all households are identical. As is well known (see Starrett, 1978, and Fujita, 1986a), if all agents are identical, there is no basis for exchange and equilibrium analysis becomes futile. The trivial no-trade solution of economics has its analog in regional science. When agents are identical in terms of preferences or production possibilities of locations, the solutions, trivial in nature, are full concentration and homogeneous dispersion. Returns to scale determine which one emerges. So the essential question addressed by Papageorgiou and Thisse is if the dichotomy between households and firms is enough to establish spatial differentiation. It turns out that the aforementioned intuition concerning the triviality of solutions also applies to Papageorgiou and Thisse (1985); they must therefore resort to a spatial externality in order to obtain non-trivial equilibria. It is all the more noteworthy that even in the presence of their spatial externality, the solution will reduce to homogeneous dispersal when the underlying geography is freed from special features such as centers and end points as in Papageorgiou and Smith (1983).

The lessons of these regional science studies is that spatial asymmetries between economic agents are required to explain non-trivial equilibrium distributions of agents. A prime spatial asymmetry is given by the indivisibility of locations when geographical space is not aggregated into regions. Ultimately, locations are points in a continuum and each is occupied exclusively by one firm or household. This indivisibility provides a seed required for non-trivial allocations; it induces a natural asymmetry. By aggregating locations into regions, not only for practical requirements but also for a conceptual foundation for location theory, regional science must resort to externalities, as in Papageorgiou and Thisse, to explain non-trivial locational equilibrium. Alternatively, locations can be fixed exogenously, as in the *Handbook*.

From this discussion one can conclude that regional science contributions capable of generating non-trivial equilibria must model geographical space or land use explicitly (see Berliant and Wang, 1993, and Wang, 1993, for example). The dichotomy between regional economics and urban economics, exemplified by the division of the *Handbook*, may be practical but yields insurmountable problems pertaining to general equilibrium analysis. There remains ample scope for useful partial equilibrium analysis and we shall now turn to the task of reviewing the various contributions.

2. Review of the Handbook

2.1. Part 1: Locational analysis

Beckmann and Thisse present three models of firm location. In the first model, there is a single commodity with a given spatial density for demand and fixed supply capacity. Demand and capacity are perfectly inelastic. In the aggregate there is excess capacity. Capacity utilization is determined over space to minimize transport costs. In the second model, firm locations are given. To have something to explain, consumers' demand is made elastic and varies with the distance between their (fixed) locations and the supply point. This yields straightforward modifications of Bertrand and Cournot competition. However, "Perfect competition is not consistent with patterns of market areas as discussed here" (p. 49). Moreover, the exogenous locations of firms and households remain unexplained. The third model introduces the element of location. Firms play a two-stage game. The first stage involves choice of location. The second stage is the Bertrand or Cournot competition as described above. Typically, equilibrium does not exist. To us it seems that the short cut to the particular game form causes trouble. A more modest approach would involve neoclassical competition with firms taking prices as given, including the price of land. This approach is more true to unity with general economic equilibrium analysis, has more scope for positive existence results, and identifies the game-theoretic nature of the problems of prevailing location theory. It would require modeling location through explicit land use.

From our viewpoint there are two fundamental reasons to analyze firm location along neoclassical lines, assuming price-taking behavior. First, perfect competition is a simple benchmark. How can we understand more complicated game-theoretic analyses without having dealt with the benchmark case first? It seems important to know which problems and results stem from the spatial setting as opposed to the assumption of imperfect competition. Second, perfectly competitive outcomes are relevant in spatial models when markets are contestable in the sense of Baumol et al. (1982), even when locational differentiation makes the assumption of perfect competition itself implausible.

Beckmann and Thisse should be complimented for their review. Through the development of the three models, it offers a clear discussion of the state of the literature. We refer to Stahl (1981) for a critique of the assumptions used in the models reviewed by Beckmann and Thisse.

The next piece, on residential location modeling, is at the other end of the spectrum. Clark and van Lierop fail to state a central problem, to specify a model, or to summarize the main results. They seem to reduce household location to a mechanical assignment problem over a fixed set of dwellings. Linear programming (where utility is assumed to be the same as savings), gravity–entropy models, log-linear models, and discrete choice or random utility models are reviewed. Only the last ones seem reminiscent of economics and individual maximizing behavior. It is unfortunate that the other approaches are not related to utility maximization, as in the work of Anas (1983, 1987).[1]

Public facility location theory is next. The chapter by Johansson and Leonardi is a rather complete survey of the literature as of the publication date of the *Handbook*. In this chapter, firm and consumer locations are fixed (except in Section 6, where there is no explicit spatial structure). What are the questions asked in this literature? Are they normative or positive? A list of models classified by their extensive forms (in the sense of game theory) does not address these issues. Most of the models appear to use a mix of positive and normative concepts, equilibria, and optima. At best, they can be interpreted as partial welfare analysis. In our opinion, after proper statement of the questions, one should go about characterizing Pareto optima in explicit spatial models of public facility location, and then move on to equilibrium and the welfare theorems, followed perhaps by some analysis of second-best situations with institutional constraints. Johan-

[1] Anas gives an elegant proof that the entropy approach and utility maximization can generate the same logit model from the point of view of an abstract econometrician analyzing the problem from the outside. However, this is *not* to say that the entropy model is justified by individual maximization, since from the point of view of the individual utility-maximizing agents, there is no uncertainty in the model; there is only uncertainty on the part of the econometrician analyzing the problem. It is also possible to interpret the random utility components as representing an uncertain part of utility. In this case the entropy function measures expected utility and maximizing it is equivalent to optimizing ex ante welfare and the optimum coincides with the equilibrium.

sson and Leonardi were keenly aware of the state of public facility location theory and the need to develop a coherent structure:

> The theory which is used to formulate models as those in (2.4) and (2.5) is mostly both fragmentary and implicit (p. 139).

> In all the preceding six sections we have been forced to conclude that much work remains to be done before one may claim that there is a comprehensive theory of public facility location (p. 168).

We would like to add that it is of utmost importance that the questions addressed by the literature be clarified and an overall research agenda proposed. Fujita (1986b) provides an excellent basis.

All of the elements above presumably combine into spatial equilibrium analysis, but, as mentioned before, that chapter is an independent statement of a transportation problem with fixed locations of agents. The Takayama–Labys chapter is also notable in that it does not even mention standard general equilibrium models of trade with distortions, and computation of their solutions. Of course, these models are not equivalent to quadratic programs, the primary modeling form used in the chapter.

Dynamics are introduced by Anderson and Kuenne. They survey the literature on several topics related to dynamics without necessarily detailing the models. The topics treated in this chapter are the minimization of transport costs over a period of time including Puu's extension to a continuum of locations, the dynamics of spatial interaction, the extension of macroeconomic growth models to interregional models with trade governed by import coefficients, the innovation and diffusion of technology, and an application of catastrophe theory. Models of interest feature 'assumptions' including

> ... a game theoretic conjecture that some rival will react by relocation to cause the largest possible loss of the initiating firm's market ... the steady state is identified with a Nash equilibrium (pp. 204–205).

The unraveling of conjectural variations in the game-theory literature must have gone unnoticed, while the second 'assumption' is not even an assumption! Moreover, the notions of steady state and of Nash equilibrium are conceptually unrelated. The analysis of dynamics starts as follows:

> Consider the decision making of a seller of goods who find [sic] it most advantageous to locate at the point of maximum population concentration (see Figure 2.1). At time $t = 0$, we suppose that the distribution of population, $P_0 = f(z; Y, T)$ is as depicted in Figure 2.2, where Y is aggregate income ... (p. 209).

The decision problem of the seller, the commodities, T, f, and so forth, are undefined. Moreover, where are prices?

There is a bizarre section on 'generalized multiregional growth equilibria' (p. 232) that contains a theorem, claimed to be original:

> For the system $\dot{y} = Qy + h$, with $h(t)$ continuous in an interval $\alpha < t < \beta$ and starting in a point t_0 also in that interval, and with Q possessing a linearly independent set of n eigenvectors, there exists a unique solution on the specified interval (p. 233).

The conditions are unnecessary; reference to Section 3.4 of Coddington and Levinson (1955) will suffice. It is stated in the chapter that the proof of this theorem is available from the first author. We wish to mention that a closed-form solution plus derivation of a more general result appears in Coddington and Levinson (1955):

> "Put simply, a dynamic model or theory is one whose structural equations contain nontrivial temporal forms of the endogenous variables" (p. 201).

Given this sentence in the introduction of the chapter, what must we think of the immediate substitution of balanced growth conditions in the dynamic input–output equation, reducing it to a static equation (p. 236)? Listing of more dubious points would amount to overkill. The presence of this material casts some doubt on the care exercised by editor Peter Nijkamp.

2.2. Part 2: *Regional economic models and methods*

Regional and multiregional models are surveyed by Peter Nijkamp, Piet Rietveld and Folke Snickars. The review of the literature contains a few conclusions as follows.

> It may be concluded that input-output analysis has played a dominant role in regional modeling. It has been a powerful tool in the empirical description of the space economy (p. 264).

> . . . it may be feared that the methods used in empirical practice are not at the frontier of econometric research (see also Chapters 10 and 11 of this Handbook for further expositions) (p. 266).

> In conclusion, new methodological design principles for building operational (multi)regional models are some of the most important items on a a research agenda for modeling the future space economy (p. 290).

Input–output analysis is the subject of a long chapter by Hewings and Jensen. It consists of three parts: basic input–output analysis, regional detail, and applications. The point of departure is the UN System of National Accounts (pp. 299–300), that features data on the total flow of each commodity into and out of each industry. An industry is thus represented by an input column and an output row. However, the Leontief accounting balances used by the authors are defined in terms of flows x_{ij} (a *scalar*) *between* industries i and j. It is incomprehensible how the flows x_{ij} are extracted from the system of accounts and what their commodity compositions are, in view of the multiproduct nature of industry outputs. How are interindustry flows x_{ij} defined? We admit that this problem is not specific to this essay, but the authors could have done a better job of separating the wheat from the chaff. Explicit discussion of United Nations (1968, 1973) would have been illuminating.

In the case of a region rather than a nation, the problem of incomplete data is pressing. Various remedies are suggested, for example the use of national proxies, but they fail to meet econometric standards. It is suggested that the point of reference for filling in gaps in this data is the accuracy of the multipliers collected in the so-called Leontief inverse, but from an economic point of view this is very dubious. For example, if the regional input–output coefficients were used in the activity allocation model of Takayama and Labys, a pattern of regional specialization would emerge. Under such conditions regional Leontief inverses, which account for local production of all commodities, become irrelevant.

The interregional extension aggravates the problem. Conceptually, sectors are made region-specific so that flows are now represented by x_{ij}^{rs}, where r and s index regions. Application of standard input–output analysis implicitly assumes fixed import coefficients, not only by commodity, but also by region of origin. Since all interindustry as well as interregional flow proportions are fixed, there is no role for economic behavior, such as the exploitation of comparative advantages. As is customary in regional science, however, emphasis is placed on practical problems of method. The problem of incomplete data becomes overwhelming since full knowledge of all x_{ij}^{rs} flows requires bilateral interregional trade statistics for each commodity.

The system of national or regional accounts involves not only commodities and industries, but also institutional, household, and interregional accounts. The 'problem' of determining the Leontief inverse is solved by obtaining a generalized inverse (p. 330). Again, why do we want to calculate multipliers? To answer the question, an economic problem must be formulated. It remains to be seen if the Leontief multiplier is an appropriate tool. Moreover, which generalized inverse should be used? There is a whole class of them. Regional scientists typically pick the Moore–Penrose generalized inverse, but the choice must be dictated through analysis of the underlying economics. For example, in the context of capital theory, the appropriate generalized inverse is some other member of the Rao class, as detailed in ten Raa (1986).

After the development of the input–output tools, Hewings and Jensen turn to economic problems. There is an interesting cost–benefit analysis of project appraisal involving value-added coefficients (including the rate of interest) in the criterion. If the Takayama–Labys model were applied, value added could have been derived endogenously. A great opportunity to unify chapters of regional science is missed.

Linear programming is also addressed in this input–output chapter. In fact, the so called "...MORSE model... is probably the most advanced..." (p. 337). In this model, employment and energy utilization are positive terms in the criterion of a maximization problem (pp. 338–339). Cost maximization is thus advocated as the most advanced contribution of regional input–output analysis.

Not only in regional input–output analysis, where intersectoral flows are often approximated by shares of national flows, but in regional science generally, a central issue seems to be the substitution of tools for data. The most prominent tool is spatial interaction analysis. It is a trick used to fill the cells of a matrix when only the row and/or column totals are known. Regional input–output analysis is an obvious area of application. Transportation flows are often disaggregated in this way. Interregional commodity flows can be dealt with in the same way, yielding an alternative to international trade models. According to spatial interaction analysis, the ijth element of a matrix is biproportional to the ith row and/or jth column totals and (in exponential decay fashion) to the distance between locations i and j. The proportionality coefficients and the decay factor are used as calibration parameters. Batten and Boyce provide an interesting review of theories that may generate this disaggregation mechanism for row and/or column totals. Among these, once again discrete choice theory seems to be the closest to economic theory, but the underlying utilities are defined on origin–destination pairs rather than commodities. Another great opportunity to unify this approach with other chapters is missed: the transportation model of Takayama–Labys is directly applicable to the spatial interaction issue.

The chapter by Bennett and Hordijk on regional econometric and dynamic models contributes further to the impression that regional science is a meeting point of various economic analytical tools. If anything qualifies as an original contribution, it is the study of spatial autocorrelation. However, in the final analysis, the latter is a replacement of economic theory by econometric technique. A step further from economic theory is the domain of 'Qualitative Statistical Models for Regional Economic Analysis' (Chapter 11). It involves things like data mining using the so called Delphi method, where decision makers say what they find important. It is the ultimate form of measurement without theory. One can earn a buck or guilder with it in the consultancy industry.

2.3. Part 3: Regional economic development and policy

A return to theory is attempted by Nijkamp and Rietveld in their multiple objective decision analysis. It is introduced as follows. "The major strength of multiple objective decision analysis is that it addresses – in an operational sense – evaluation and choice problems by various conflicting interests" (p. 494). This sounds like game theory. However, the conventional approaches to multiple objective decision analysis are:

(i) the transformation of the different objectives into one decision criterion by assuming a utility function, and

(ii) the representation of positive and negative effects of alternative projects on groups of persons in terms of one indicator: economic welfare.

This suggests that multiple decision analysis is not game theory, but social welfare theory under a different jargon. Given the lack of theorems, it is difficult to delineate the structure of the theory, which is provided in Section 3 of the chapter. The discussion concerns incomplete preferences. The analysis determines linear utility functions consistent with them. If point A is optimal for a bigger subclass of linear utility functions than point B, then it is more 'probable' that A is optimal. The size of the subclasses is determined by the uniform prior distribution on (linear) utility coefficients.

This approach is incredibly naive. Why use linear utility functions? Why have a uniform prior distribution? Why have such a distribution at all? Does this theory have an axiomatic basis? Anyway, in the final analysis, the concern is a determination of weights for different objectives. We can only reach the conclusion that multiple objective decision analysis is substandard welfare theory. Incidentally, there is nothing 'regional' about it. In an application there is a weird objective function, again featuring forms of cost maximization; for example, the total demand for labor as well as the occupation rate of land are maximized [points (4) and (7) on p. 536].

Methodological recommendations for regional science are articulated succinctly by Isserman et al. (p. 545):

> To compound the difficulty, data available for national modeling efforts often are not reliable or even tabulated on the regional level. Consequently modeling strategies and methods must be invented that recognize and compensate for these data limitations. The models themselves then are the product of an intricate interplay of theory, data, and method.

We agree with this frank assessment of the state of the field. The independent role of 'method' is also apparent in their own review of regional labor market analysis. The authors consider it to be a synthesis of economic and migration modeling. Migration is modeled demographically, time-series analytically or through Markov processes. "Such demographic approaches can be considered atheoretical, . . ." (p. 549), while the time-series approach ". . . does not preserve any of the advantages and information of demographic accounts" (p. 551). The Markov process approaches are based on exogenous indexes of attractivities. We note that the economics of regional labor markets, as exposited here, ignores wage rates in all but one region, except when a 'leading region' is used for comparison. As regards the interplay between labor markets, it is simply concluded that

> Although the existence of these economic–demographic interrelationships may seem obvious in that they constitute a considerable simplification of the functioning of labor markets, demographers and economists very rarely model population and the economy as if they were interconnected (p. 574)

and

> Without more meaningful representations of regional labor markets, the contributions of such regional economic models to labor policy analysis will and ought to remain very limited (p. 575).

The next subject, energy and the environment, is discussed in the same vein. There is an excellent survey of the theory and application of energy models, but the discussion of models of environmental resource use leaves much to be desired. The latter employs a slightly unorthodox and faulty theoretical framework,[2] and ignores commonly proposed adjustments for market failures due to externalities, such as markets for the right to pollute. A firm link with Pigouvian taxation in particular seems within reach, but is not established. Practical applied models are reviewed. Occasionally, interesting regional elements such as spatial pollution coefficients are featured. Lakshmanan and Bolton draw a frank conclusion.

> The implication is that a complex general equilibrium model, incorporating both interregional trading patterns and interregional ownership patterns, is required in order to analyze fully the effects of a change in energy markets on the interregional distribution of income. As discussed below, there are not yet any completely satisfactory models of this kind. Therefore any analysis must remain somewhat partial and fragmentary (p. 600).

The *Handbook* concludes with a chapter on regional structure and a chapter on regional policies in developing countries. The first considers regional analysis in a cross-sectional study of capital vintages and labor heterogeneity. The second does not lend itself to characterization in the absence of any statement of a problem or model, and suffers from internal incoherence (for example, the 'conclusion' does not follow logically from the body of the chapter).

[2]Specifically, the functions used in Eqs. (3.1) and (3.2) should be made to conform to standard production theory by making all outputs of a firm dependent on all inputs, condition (3.4) should be summed over regions, (3.6) can only be used under the assumption of concavity, and (3.7) characterizes optima only under strong concavity restrictions.

3. Conclusion

Regional science is a multidisciplinary tool box used to address macro issues; it has the potential to provide a unified analytical framework for location–allocation problems with indigenous spatial features. The claim of the editors that regional science has fulfilled its potential is not warranted by the chapters in the *Handbook*. The editors conclude from these papers that the subject has made rapid progress in a mere 20 years, and in fact they call the subject 'mature'. In this paper we focus on the half of the glass that is empty rather than the half that is full. While the papers inform us of progress, they also illuminate the deficiencies of what has been understood so far. The various pieces of partial equilibrium analysis, firm and household (as well as public facility) location theories, are not related to a general spatial equilibrium model. Firm location theory seems rather well developed, albeit along rather particular game-theoretic lines instead of competitive lines with explicit land markets, and is indeed contingent on exogenous household locations. Household location theory, however, is not contingent on exogenous firm locations, but rather on exogenous dwelling locations. These pieces do *not* fit together, while the general spatial equilibrium analysis presented in the *Handbook* employs fixed locations of actors. It thus reduces the field to applications of transportation and trade models of commodities and activities. What makes such models distinctive as the building blocks of 'regional science'? The authors of chapters, particularly those pertaining to regional models, labor, and energy, bluntly admit that the field is in its infant stage. The assessment of Nijkamp and Mills concerning the state of regional science must be based on wishful thinking.

Even if we take into account articles that are not covered by the *Handbook*, such as Papageorgiou and Thisse (1985), it appears that there are some intrinsic difficulties in reconciling the various location theories into a general equilibrium framework. Regional scientists typically assume away taste and technology differences in order to explain spatial equilibrium on the basis of intrinsic spatial elements. To avoid degeneration into trivial solutions of uniform dispersal or full concentration, one must take into account land use. We have tried to suggest ways in which this might be accomplished. However, this aspect is eliminated from regional economics by relegation to urban economics. If this division of a single field persists, regional science will remain what it currently is, a collection of tools. These tools may be capable of substituting for missing data, a prominent feature of the field, but they will never be able to fill the gap in theory, notably the absence of a conceptual framework. In our view, this is the central reason why regional science has not attained the prominence of economics.

As economists, the consequences of such an absence seem quite clear to us. First, it has led to a literature that emphasizes technique over the development of models that *directly* attack interesting problems. Models with shaky foundations are developed so as to be able to exploit recent fads in applied mathematics, such as chaos and catastrophe theory (see Chapters 6 and 10), rather than formulating an interesting problem and then exploring what types of mathematics are needed to solve it. Second, it has placed mechanistic models borrowed from other disciplines at the same level of acceptance as economic models, without ensuring consistency with economic paradigms such as maximizing behavior and the price mechanism. Without such consistency, both the normative and positive content of a model are suspect.

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