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Making Urban Models More Realistic: some strategies for future research*

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1. Introduction

Britton Harris (1965) once characterised planning as being concerned with three kinds of activities: prediction, invention and choice. He was among the first to argue that urban modelling could be an effective instrument of prediction and that the mathematics of optimisation could not only be a method for model building in some circumstances, but could also be an aid to design. But all these technical aspects of modelling and planning research have always been for him, and should be for the rest of us, subservient to the issue of choice: that planning methods were simply aids to improve choices, to enable governments to improve the lots of individuals, to give them more freedom to choose for themselves. This all rooted in a liberal's view of the possibility of progress.

The more technical aspects of the programme of research implied by these thoughts have now been actively researched for well over twenty-five years - the modern phase dating perhaps from the development of computers in the 1950's. The purpose of this paper is to review the progress of the programme, to confront some serious issues which are thus raised, and to make some comments about priorities for future research.

In some ways, there has been amazing progress. There is a basic technical competence in many aspects of modelling which could not have been envisaged in the early days. There have been many model-based planning studies. But in many ways, and perhaps in the most important ways, the future has not worked out as the pioneers of modelling would have hoped. The integration of the field across alternative approaches and disciplines has not been achieved. More important, in many significant ways, problems of cities which were identified many years ago are now significantly worse. In British cities, for example, social polarisation is worse and there are consequent problems of the greatest severity and apparent intractibility in inner city areas. It is important, therefore, to take stock and to see if there is an effective way forward.

The argument of the paper proceeds via two main sections. First, in Section 2, we briefly review the state-of-the-art, particularly in relation to our inability to achieve an effective synthesis, but also to emphasise the significance of some relatively recent developments. Secondly, in Section 3, we review research priorities, first in relation to technical issues and secondly in relation to the potential consumers of urban modelling - those suffering from the urban and regional problem of the day. Some concluding comments are made in Section 4.

2. Progress in Urban Modelling

2.1 Introduction

The argument proceeds in four main stages. First (2.2 below), we present and examine six basic dimensions of model design which provide a framework for the review. Secondly (2.3) we discuss different disciplinary stances. Thirdly (2.4), we outline recent developments which imply quite fundamental shifts of focus both for model development and for the use of models. The emphasis here is on the building of dynamical models and on the bifurcation consequences of the high levels of nonlinearities and degrees of interdependence in urban systems. Fourthly, we offer a preliminary discussion of the use of models in planning and the experience gained in relation to planning problems.

2.2 The basic dimensions of model design.

It was argued in an earlier paper (Wilson, 1981-D), that it is useful to consider model design in terms of six dimensions. This provides a useful framework against which to assess progress and problems. We discuss each in turn.

(i) Entitation and system definition. Here we use Chapman's (1977) notion to describe the enumeration and articulation of the elements of a system of interest. One of the most important parts of this process is the definition of subsystems in an effective way.

(ii) Scale. The problem of modelling - indeed in some instances the phenomena to be modelled - change with scale. Three divisions - micro, meso and macro (cf. Haggett, 1965) are useful in reflecting a shift from a concern with individuals at one extreme to whole regions at the other, with the meso scale distinguishing spatial resolution.

(iii) Spatial representation. We will be mostly interested in subsystems described at a meso scale in which spatial structure is explicit with connections to theories and models at micro and macro scales. A fundamental issue then arises: whether to treat space as continuous or in terms of discrete zones. Different theorists have made different judgements about this. One of the contentions of this paper is that, for most planning purposes - and indeed more generally, the discrete zone system is more convenient. More effective mathematical methods are available.

(iv) The partial-comprehensive spectrum. Consider an individual person or a firm. Many theories are concerned with the (locational, say) behaviour of an individual <u>given</u> the rest of the environemnt. It is difficult, particularly in the urban model case, to solve the aggregation problem for these particular theories and to generate models which are comprehensive. This is because of the difficulties, for example, of representing in detail the competition between even a small number of firms (cf. Hotelling, 1929). Thus, while insights can be obtained from marginal analyses, there are many planning problems for which something more comprehensive is required and then new methods - alternatives to aggregating micro-models - have to be sought. (The usefulness of entropy-maximising, as in Wilson, 1967, 1970, was in providing such a method by statistical averaging over individual behaviour; but there are now other methods also.)

(v) Hypotheses. The decisions to be made under (i)-(iv) above can be seen as a clearing of the ground for model building - the preliminary decisions which are required. The heart of the matter is obviously the development of theory, the identification of hypotheses to be tested. There is obviously a close interplay between this process, the preliminary decisions and the availability of methods to be discussed shortly. Many would argue that the interplay was too close and that more ambitious hypotheses were not explored because the techniques were not available to represent them. We return to this issues later.

(vi) Methods. The greatest advanced in modelling have been in the area of methods. Entropy maximising, random utility theory, accounting concepts, mathematical programming and dynamical systems analysis all provide examples of the development of highly refined methods. As will be argued in a number of contexts below, there has not been an effective eclectic synthesis; too much of a tendency to focus on a narrow viewpoint and to see all problems in terms of it.

It is of interest to review the history of urban modelling in terms of these dimensions - see Wilson (1981-D) for a more detailed account. The work of the 'classic' geographical theoretists can be seen in these terms. There is at least one for each major subsystem in a chronological order which is probably determined by their relative importance in history: von Thunen (1826) for agricultural location; Weber (1909) for industrial location; Burgess (1927) and Hoyt (1939) for residential location; Christaller (1933) and Losch (1954) for settlement structure. They used continuous space representations and usually adopted partial viewpoints.

What they lacked were the mathematical methods of the 1940's onwards to make more effective progress. Subsequent developments can then be seen as part of a spectrum, but looking backwards in this way also enables us to build some new models by combining old problems (eg. von Thunen's and Weber's) with new methods. We return to this later in Section 2.4.

2.3 Disciplinary stances

The three disciplines most directly concerned with the theoretical basis of urban modelling are geography, economics and sociology; and those most concerned with urban problems and their solution are town planning and transport planning, the latter often as a subset of civil engineering. Engineers and architects also have an obvious interest in buildings, but this is not our prime concern here. There are subsidiary disciplines such as social and public administration and politics which are relevant, and others which are even more subsidiary but whose contributions may be crucial - such as history, English, philosophy and mathematics. A focus on the city and region obviously demands a multidisciplinary perspective. This has been attempted in the development of regional science and, more occasionally, through concepts of 'urban studies' or 'environmental science'. This position has barely changed over twenty-five years and an analysis is useful because it can tell us something about the deficiencies of the field and about the needs for future research.

Of the traditional mainstream disciplines, the geographers perhaps stand the best chance of providing the foundations of urban modelling. There is a basic concern with spatial analysis and a tradition of synthesis in using the concepts of adjacent disciplines. The so-called 'quantitative revolution' within the discipline made it more outward looking and provided a valuable impetus for urban modelling within it. Much of the momentum of this has now been lost, however: the energy has been dissipated in less productive statistical analyses; except for a few departments it has not proved possible to teach anough students mathematical skills in sufficient depth; there is considerable fragmentation within the discipline - a general topic to which we will return; and there is the competition of the new fashion of Marxist perspectives.

In the case of economics, 'urban economics' has always been the poor relation among its subdisciplines. ("Putting the word 'urban' before 'economics' has the same effect as putting 'domestic' before 'science' as Lloyd Rodwin once said to me). Most economists operate at micro or macro scales and fail to understand that the intermediate meso scale, with space made explicit is interesting and important on the one hand and demands new additions to the economists repertoire of methods on the other. (There are distinguised exceptions, of course.) They also have an infatuation with particular perspectives and methods - such as always attempting to aggregate micro-economic theories and using 'continuous space' representation for modelling - which do not provide the best basis for tackling the most interesting questions at an urban scale. Some of the assumptions of their theories are also inadequate for the urban case - the assumption of prices for housing being determined through perfect markets, for example. These attitudes usually combine with an inability to look outside the discipline of economics for concepts and methods which would help to solve new kinds of problem. It must be emphasised, however, that the concepts of economics remain linked to many tasks in urban modelling, but where possible within a multidisciplinary framework.

Sociologists, particularly in the recent work of Giddens (1979, 1981), have attempted to tackle more fundamental questions about the structures of society and the processes of change, than economists. This provides new insights which are particularly relevant for the discussion below on the continuing existence of several urban problems. It provides new questions for urban modellers, but not, on the whole, specific new ideas or 'answers' at this stage.

Of the subsidiary disciplines, politics is close in spirit to sociology in what it offers. Social and public administration offers a focus on particular services which are increasingly important and there is much to learn from that source. Mathematics and philosophy are important for aspects of method. History and literature can be seen as different approaches to the study of society. In the former case, it should perhaps be an integrating discipline, but has not yet been sufficiently effective in this role. The latter provides an example of source material which should not be neglected - cf. for example Raymond Williams (1973) The country and the city.

What, then, of the potentially integrating disciplines whose prime focus is the city and region? Town planners have in general shown relatively little interest in urban modelling and what it could offer them. They have tended to restrict themselves to more traditional methods. More importantly, the promise of planning in the 1960's has not been achieved. As we have noted earlier, many urban problems are worse and planners seem impotent. It can be argued, of course, that this is not mainly the fault of the planners. Deeper forces are shaping cities than they - or the rest of us - understand or can control even when they do understand them. The analyses from sociology and politics may be crucial here. Planners do attempt to draw their concepts from all other relevant disciplines; but the result has not been effective enough. And the development of the discipline has been bedevilled by the sometimes narrow concerns of professional societies.

And so to regional science. The academic development of the discipline has been hampered by the fact that when it should have been growing most rapidly, pressure on public expenditure increased and the growth has not been possible. What it has succeeded in doing, with great success and in a variety of forms, is providing through its Associations, conferences and journals, a focus for multidisciplinary perspectives. Here, the variety of foci internationally has proved significant. In North America, there has been a greater dominance of the economic perspective than there has been in Europe, for example.

The main question at the end of this broad-brush analysis is: how can we be more effectively multi- inter- and cross-disciplinary? And as a lurking, implicit, subsidiary question: how can we rise above the imperatives of fashion? We return to these and related questions in Sections 3 and 4.

2.4 The Impact of Recent Developments: grounds for optimism?

In broad terms, the elements of a comprehensive model system can be summarised under the following headings:

(i) macro population backcloth

(ii) macro economic backcloth

(iii) spatial interaction

(iv) the location of population activities

(v) the location of economic activities

(vi) the development of physical infrastructure.

These also combine under the heading of

(vii) spatial pattern or settlement structure.

The list represents roughly increasing order of difficulty. It is also appropriate to make a number of remarks at the outset: first, that each item in the list needs to be subdivided in relation to different subysstems; secondly, that it is not always easy or appropriate to distinguish between

(v) and (vi); thirdly - a more general version of the point just made - it is sometimes appropriate to define subsystems by combining elements from different parts of the list. For example, it might be best to study residential location (iv), the behaviour of house builders (v) and the supply of house building (vi) as one system of interest. With these qualifications in mind, we now review progress for each element of the list in turn and emphasise the significance of recent developments.

We can assume that adequate models exist for the macro-backcloths, and even that the two models can be linked through appropriate submodels of migration and labour supply. The major surge in the development of urban modelling as we now know it at the meso scale came with the development of spatial interaction models in their modern form from the late 1950's onwards, initially in the context of transportation studies. This was quickly followed by the realisation that what we would now call singly-constrained models also function as locational models, particularly for population activities such as use of services and residential location. By the use of quite complicated attractiveness functions, these models can be made very rich. And if economic models are put in the same format - by using discrete zone systems for example - it becomes possible to integrate models which have been developed from different perspectives. It should also be recognised that a variety of methods can be used to develop appropriate models in different circumstances: entropy-maximising, random utility theory (Williams, 1977) and micro-simulation (Clarke, Keys and Williams, 1979) provide examples. A relatively recent documentation of all this is provided by Wilson (1981-A).

There are good reasons for this kind of progress being possible: the subsystems involved are (or in the macro-economic case is treated as) what Weaver (1958) called systems of disorganised complexity and each of the methods which have been used is some form of statistical averaging method. In the case of the location of economic activity or the evolution of physical infrastructure - and hence also the settlement pattern as a whole, the subsystems are of organised complexity and different methods are needed. What is more, until relatively recently no appropriate methods were available. It is the provision of these methods which may prove to be significant for the future.

The essence of the new method is the solution of differential or difference equations with the capacity to analyse the nature of the solutions. In many cases, equilibrium solutions exist which can be identified with spatial patterns in geographical reality. Thus, there are two modes of analysis: equilibrium patterns and modes of change. The interest arises because the nonlinearities and interdependencies generate multiple solutions, some of which can appear and disappear at critical parameter values. This can lead to the system jumping from one state to another. The detailed analysis of these phenomena is provided by Harris and Wilson (1978), Wilson and Clarke (1979), Wilson (1981-B), Harris, Choukroun and Wilson (1982) and Clarke and Wilson (1983-A). When the system is analysed in dynamic mode, it turns out that there are new kinds of bifurcation (Wilson, 1981-E, building on May, 1976).

The multiplicity of equilibrium states, means that in reality, where perturbations and noise abound, there is an enormous number of possible paths of development (or evolution) of any system of interest of this type. This analysis provides one simple but extremely important insight: while it is the case that models should be able to explain the historical path of development, detailed forecasting of the future in the manner hitherto assumed possible in much urban modelling is no longer feasible. It may be that this statement should be qualified if it is possible to classify system patterns into 'types' in a useful way, but this is a research problem to which we return below.

The most detailed working out of these methods of dynamical systems theory (in relation to catastrophe theory and bifurcation theory) has been for retail systems. But it is now clear that it is, in essence, a general method. Let X_j be a quantity of some structural variable in zone j. Let $D_j(X_j)$ be the benefit (or revenue) derived from its presence and let $C_j(X_j)$ be its cost. Then the dynamics can be described by differential equations (or the corresponding difference equations) of the form

$$\dot{X}_{ij} = \epsilon[D_j(X_j) - C_j(X_j)]X_j \qquad (1)$$

and the equilibrium conditions are

$$D_j(X_j) = C_j(X_j). \qquad (2)$$

These equations are, of course, deceptively simple. The functions D_j and C_j will typically be nonlinear in X_j and will also (for example, to represent competition) be functions of all the X_k, $k \neq j$. The equations (2), therefore, represent a set of simultaneous equations in $\{X_j\}$ and the degrees of interdependence and nonlinearities present will produce a rich variety of possible solutions and bifurcation phenomena. Equations (1) play the same role for the dynamical system which is not in equilibrium.

The model is general in that it is usually possible to find representation of D_j and C_j for most locational phenomena (Wilson, 1983-A). For any particular subsystem, the interest then becomes the detailed specification of D_j and C_j and the analysis of bifurcation phenomena for the particular case. In addition to retailing and services - see the references cited above - these ideas have now been developed for residential location (Clarke and Wilson, 1983-B), industrial location

(Wilson and Birkin, 1983, Birkin and Wilson, 1983-A) and agricultural land use (Birkin and Wilson, 1983-B).

The last two sectors were developed as obvious extensions of the Weber and Von Thunen models respectively. This is a convenient point, therefore, to stress that this shift forward can be seen as arising from particular choices (or new developments in the case of method) associated with the model design questions of Section 2.1. What has proved difficult in the past has been the handling of competition between units and the mathematics of continuous space. These are solved by the use of a discrete zone system combined with the developments in the mathematics of dynamical systems analysis, together with some tricks of analysis which are appropriate for particular systems. Because of the way competition is handled within these models, it is also possible to build comprehensive rather than partial models. Nor is this model framework particularly dependent on the set of hypotheses used in the illustrations cited above. Alternative hypotheses simply imply the redefinition of the functions D_j and C_j and this will usually be possible.

2.5 Modelling and planning: grounds for pessimism

It is important to conclude this reviewing section by returning briefly to the topic of models in planning. The preceeding subsection concluded on a note of high optimism, implying that it is now possible to see how the major problems of model-based analysis can be tackled. Should this optimism be carried through to planning? The first point to note is that the new models are not easy to calibrate and use - they demand new kinds of data - and there is the fundamental point about the difficulties of forecasting made earlier. When transport models were first developed, then given a survey, they could be calibrated and they were immediately useful in their specific context. It will be harder work with the new generation of models.

The second point, and the most significant one, is that substantial elements of the so-called radical critique (begun, say, in Harvey, 1973) are true in the sense that the most important determinants of the present urban problems are not directly reflected in the models discussed above. They are essentially political. As Harvey argues, we do not need new factor analyses to understand the scale of inner city problems. What the radical critique begins to offer, is a different kind of analysis about why these problems develop (cf. Wilson, 1981-C). It remains the case that model-based planning could be valuably grafted on to any initiatives which arose out of that kind of analysis. 'Solutions' still have to be invented; resources have to be assessed. To deny this, as some critics have, is a nonsense. Equally, to deny that it is necessary to look more deeply to find the causes (and likelihood of solutions even being feasible) of current urban problems, in an inadequate response. So there needs to be a new and major element of an ongoing research programme to which we will return in Section 3.

3. Priorities for Research

3.1 Introduction

The field is rich in detailed interesting technical questions associated with urban modelling, and we discuss these first in Section 3.2. Secondly, there are important research topics associated with particular subsystems and we discuss these in Section 3.3. Thirdly, we again confront the issue of models in planning and the apparent intractability of urban problems. Our broad conclusion takes the following form. First, it is important for us to understand cities and their nature as deeply as possible for the same sorts of reasons that we would pursue fundamental questions in physics; chemistry, biology or literature. Urban modelling has a contribution to make to this - one of many - and it is important that this acadmeic base should be articulated and defended. Secondly, some of the models developed in this way will have obvious uses: the siting of some public service facilities is an obvious example. These uses need to be spelled out. Thirdly, it is important to engage with urban problems: to analyse them, to understand their social and political basis; where appropriate, even, to engage in the politics. We can explore the extent to which urban modelling can inform these analyses and debates. We can see whether the setting of new research priorities will potentially lead to the development of urban models which make a more fruitful contribution to problem solving.

The bases for these three broad conclusions about kinds of research are developed in the succeeding three sections below. One final comment should be borne in mind at the outset. Much research which is actually done is essentially trivial. It is impossible to escape the task of making judgements about what is interesting and important. It is essential, than, that a variety of such judgements is available, and that these are the food of public debate. What follows is one such contribution.

3.2 Research in urban modelling methods

Perhaps the best way to delineate the concerns of this section from those of the next is to distinguish between methodological problems and those of substantive systems, even though the former are inevitably illustrated by the latter. We proceed here by taking the existing stock of knowledge about models as read and present a number of points which illustrate the difficult research tasks which lie ahead. Most of these examples arise out of the research programme on dynamics which was sketched in Section 2.4 above.

(i) It is clear that the models of geographical structure we now have generate a great variety of patterns as the parameters of the model change. It is also clear that some of the changes of pattern represent jumps - there are bifurcations at critical parameter values. These are illustrated by a simple retailing example in Figure 1 and for a more complicated residential location and housing model in Figure 2. We need to know more about this variety of structures. We need to be able to classify patterns and to identify 'types'. We need to be able to recognise a real pattern as a perturbation of some idealised type. And so on. In general terms, we have a problem of pattern recognition and the transformation of patterns.

(ii) Although some progress has been made in the analysis of the mechanisms of change - as in Harris and Wilson (1978) - there are at least two particularly difficult problems for further research. First, there is the 'backcloth' problem, spelled out in Wilson and Clarke (1979). The analysis method is for a single zone and involves the study of $D_j - C_j$ relationships for varying W_j. The question is: what to assume about $\{W_k\}$, $k \neq j$, while this analysis is going on? The problem is of much higher dimensionality than we are currently dealing with and seems to be genuinely difficult to handle within the present capabilities of mathematicians. So there may be a mathematical research problem here.

The related problem is a corollary of the first: we need to extend the analysis of zonal change to the analysis of pattern change.

(iii) When the equilibrium problem is solved, one of the key features is the existence of multiple stable solutions (cf. Croutchley, 1983). Our present procedures, in most cases at least, generate the solution which is the global optimum when the problem is reformulated as an equivalent mathematical programming problem. (This appears to be achieved by the case of uniform starting values in an iterative procedure.) Our concern with bifurcations is to identify critical parameter values at which the nature of this solution changes. The research problem is this: we need to be able to generate, and to understand, a greater range of the multiple, local, solutions and the ways in which noise generates bifurcations between them. This problem has been explored indirectly using simulation methods by the Brussels group - see Allen, Deneubourg, Sanglier, Boon and de Palma (1978) - but much analytical work needs to be done. This is particularly important in relation to learning how to confront these models with reality because there is no a priori reason to think that the system should adopt the global solution. So we need to know, for example, whether there are local solutions which are 'near' to the global one in terms of an objective function but which are 'distant' in terms of pattern.

(iv) The problems formulated so far have been in terms of equilibrium patterns. Notwithstanding Britton Harris' remark that bifurcation 'makes comparative statics interesting again', we also have to recognise that real systems will not, typically, be in equilibrium. We have already noted that, by extending May's (1976) ideas, new kinds of bifurcation can be identified. We should also note that even if a system is not in equilibrium, its trajectory will often be partly governed by (possibly shifting) 'underlying' equilibrium states. A major problem is the understanding of such trajectories and their relationships to any underlying states; and whether it is possible to estimate model parameters from data on such trajectories.

(v) It was argued earlier that the key notions of the analysis would remain the same if alternative detailed mechanisms were substituted. Another important research area, therefore, is the exploration of the consequences of adopting different basic model hypotheses within the overall framework. By implication, this will be picked up again in

Section 2.3 below. This kind of investigation could provide a basis for comparative research on different kinds of models, and this would have its own interest.

(vi) The results of research on topics (i) - (v) above may provide the basis for tackling a more fundamental question which we can approach initially through a biological analogy. Key fields there are the study of the development of organisms and the evolution of species - though the distinction between the two concepts becomes more fuzzy when we try to apply them to cities. In the first case, we are concerned with the development of a system 'programmed' according to a given set of rules. These could be the rules implied by the hypotheses of the models used here. They could be used to explore forms of development over a long period of time. Evolution might then be understood to involve changes in the rules which allow 'higher order' species of city to emerge.

3.3 Particular subsystems

In this section, we pick out examples of particular subsystems, in effect from 'within' the list of Section 2.2. We consider the typical range of models used in urban studies,

(i) Most work to date has been on the retail model. What is needed now is the empirical justification of the model in a number of real contexts. We know that pattern bifurcations have taken place - see, for example, Wilson and Oulton (1982) - but they have not yet been charted in any spatial detail. G. Clarke (1983) has begun this process. The modelling task involves exploring and calibrating a range of attractiveness functions for time series data, and exploring the bifurcation consequences of each.

(ii) Another obvious example is the residential location and housing field. A theoretical exploration has been carried out by Clarke and Wilson (1983-B) and empirical work is needed, particularly on changing housing stock by tenure and the behaviour of different kinds of suppliers.

(iii) We have already mentioned the development of new models of industrial location (cf. Wilson and Birkin, 1983 and Birkin and Wilson, 1983-A) and

(iv) agricultural land use (cf. Birkin and Wilson, 1983-B).

(v) An important field is the geography and planning of public services. This is an area where the planning problem is often relatively well defined and modellers can make a straightforward contribution. Recent examples in Leeds work include applications to health (see, for example, Clarke and Wilson, 1983-C) and education (see Wilson and Croutchley, 1983).

(vi) Little needs to be said about the development of conventional transport models, except perhaps that it would be interesting to integrate, or at least compare, the traditional approach with those of newer alternatives such as activity analysis (see Jones et al, 1983). There is one new problem, however: the application of the methods of dynamical systems theory to the evolution of transport networks. This is more complicated than the locational examples because of the higher degrees of interdependence brought about by the network nature of transport supply (see Wilson, 1983-B).

(vii) Finally, we need to examine more comprehensive approaches. Traditionally, these have been based on extensions of the Lowry model. It is now possible to add explicit models of service supply and housing development using the methods mentioned above and also, in principle, of industrial location and agricultural land use (which could be used to determine the urban-rural fringe) and even, eventually, the evolution of transport networks. This is obviously a major research programme the importance of which turns on the (not-yet-known) answer to another question:

to what extent are the degrees of interdependence in a comprehensive model vital to our understanding of urban development and urban problems?

(viii) There are potentially other approaches to integration - via activity analysis as already mentioned in relation to transport, or through economists' theories which include time allocations and budgets as well as money (as in Becker, 1965 or Evans, 1972); the geographers approach to time is also relevant (Hagerstrand, 1970, Parkes and Thrift, 1980). We need to find the best ideas out of each and to look for ways of using them more eclectically. Of course, they all involve a shift to a micro scale, and so we may need to find some way of using them in a complementary way with meso models.

3.4 Planning problems and research priorities

We focus our comments on three points here: the direct products of academic research; information needs of planners; and the need to study planning problems directly, particularly in respect of the present impotence of planning methods in relation to these problems. We discuss each of these areas in turn.

(i) Planners and policy makers need the best insights they can get from analysts. Modellers should be capable of making a more direct contribution than they evidently do. We have seen, by implication, that most model-based contributions relate to well-defined problems of specific subsystems. This contribution needs to be broadened. It could be helped along if modellers could demonstrate for a single place the whole range of contributions. (This was attempted in a rudimentary way for Leeds and West Yorkshire in Wilson, Leigh and Rees (Eds.), 1977.) Unfortunately, this needs research on an academic scale which is rarely feasible because of the resources required. It could be accomplished within planning agencies, but again, it typically is not, usually because there are not sufficient model-skilled staff available.

Even if demonstration projects along these lines could be accomplished, much more thought needs to be given to the way in which the outputs of models can be related to planners concerns. A research programme is feasible here, but the required methods would emerge more quickly if model-based planning was more widespread and routine. All this raises questions about the organisation of education and research in planning and related disciplines and we return to these in Section 4 below.

(ii) One way of encouraging better-informed planning would be the development of better (computerised) information systems. This concept was seen as a panacea in the 1960's and has perhaps been seen since as a failed idea since, in spite of numerous attempts, no one seems to have succeeded in effective implementation. But perhaps the idea came too soon. Modern concepts of computing offer much better interactive information systems than were available then, much more cheaply. It could also be that one of the main contributions of modellers would not only be to provide a conceptual basis, but to fill in gaps in information systems using entropy-maximising or micro-simulation techniques.

It is important that any research programme in this field has a conceptual basis and is not simply seen as 'information technology'. But it does seem that a major project is now desirable. This could provide: (a) a conceptual basis, linked to modelling and theory; (b) a computer system design; and (c) an explicit link to the range of data and sources needed to back it up.

(iii) We sketched out in Section 2.5 some of the reasons for the impotence of planning methods in the face of planning problems. This must, therefore, be the basis of a major research programme. It would involve the study of

(a) theories of the state and the extent to which planning in its present form could be expected to ameliorate problems - which could all be described as understanding the metasystem which generates the impotence

(b) a review of theories of urban development, particularly in areas where modellers are obviously inadequate - for example, a concern with power rather than the vacuous assumptions of neo-classical economics. This could provide new hypotheses for modellers.

(c) From the modellers' point of view, an important research topic is the articulation of variables and parameters which are wholly or partly controllable (or, to use a more neutral word, plannable) in a variety of different circumstances.

4. Concluding Comments

We conclude with comments about three topics, two of which have not yet been addressed, one which is useful to reiterate. First we discuss the organisation of research and education and the impact (of lack of it) which it has on the research topics proposed here. Secondly, having reviewed over twenty years work and proposed a research programme in the light of it, we ask what the future might look like in a further twenty years. Thirdly, we reiterate the need for a variety of approaches, for eclecticism.

It is helpful to begin by observing that research is usually used via a research-development-application (R-D-A) chain. In terms of the organisations involved, there are two size distributions which are of interest. Across the chain, application organisations (planning agencies) will be larger than development organisations which in turn will be larger, usually, than research organisations. Within the research sector, there will also be a distribution - skewed so that there is a large number of small units (say individuals and research assistants in universities) and a much smaller number of larger ones. When these ideas are used to review the organisation of what could broadly be called the planning sector, there will typically be deficiencies in the development sector - in Britain virtually total absence - and, as already noted, in terms of skilled personnel in the applications area. In research terms, there is usually an inadequate number of large units - and these are sometimes needed (as is recognised in many other areas of research, like defence) for solving certain kinds of problems. So, although this is not the place to argue the point in detail, it can be said that some of the deficiencies are not simply to be put at the door of academics, but relate to overall organisation and investment.

The comment to be made about the organisation of education builds on the brief analysis of disciplines of Section 2.2 above. As far as the field of urban and regional studies is concerned, there has been a failure of organisation within and between disciplines. This is shown by the relative absence of synthesising scholarly work across disciplines for example. There is a need therefore for more multidisciplinary work and, ironically, given the likely lack of resources to create new kinds of departments, much of this will have to be within existing departments with some organisational contributions from outside. (It is perhaps significant, for example, that the Social Science Research Council in Britain no longer organises its Committees on a disciplinary basis.)

What will the equivalent review to this look like in twenty years' time? Given the past rate of progress in solving technical problems of model development, we can first expect that there will be an enormous amount of further progress. The problems which have been offered as examples here will probably largely have been solved and a host of new ones available for research. I would also speculate that, if only because of the nature and power of the latest generation of computer systems, the development and use of improved information systems will be widespread and routine. There will be a corresponding expansion of model-based applications in planning. The timing of this is more difficult to predict. But it may also be a reasonable conjecture that the present range of urban problems will, if anything, be worse: unemployment and its consequences, inner city problems, social polarisation, the decline of some public services; and so on. Can the tide be stemmed? The answer to this question will not lie in the hands of urban modellers. But they can help to provide information to fuel the wider debates.

Thirdly and finally, I want to argue for variety of approaches in research and for a continuing effort to integrate the best of the ideas from this variety. Some would see the continuing striving for integration as a dialectical process. I once described myself as an 'unrepentant eclecticist' (Wilson, 1978) and I would stick to that less ambitious label. What we do need is sufficient effort and resources to ensure that a variety of attacks on problems are possible.

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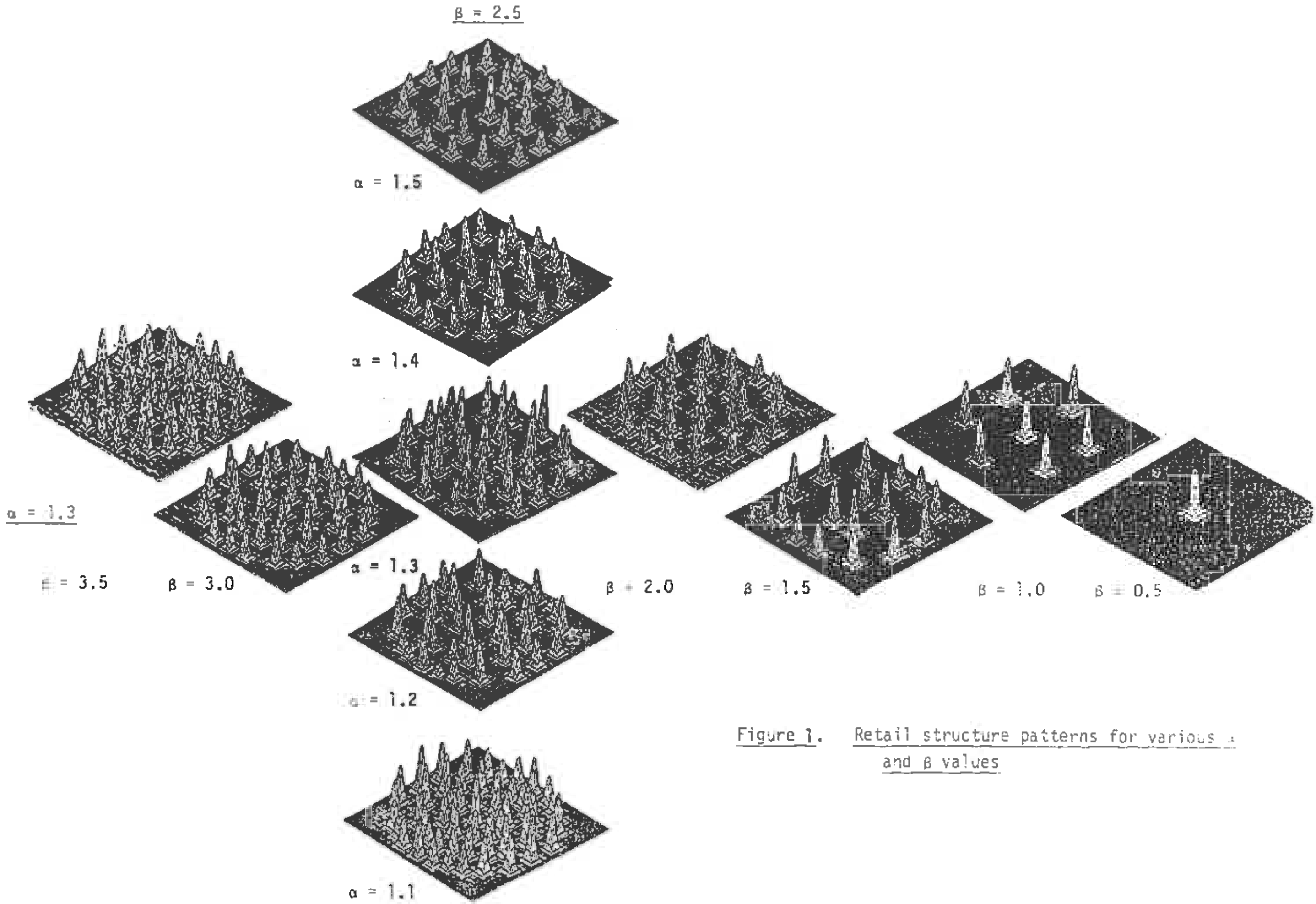

Figure 1. Retail structure patterns for various α and β values

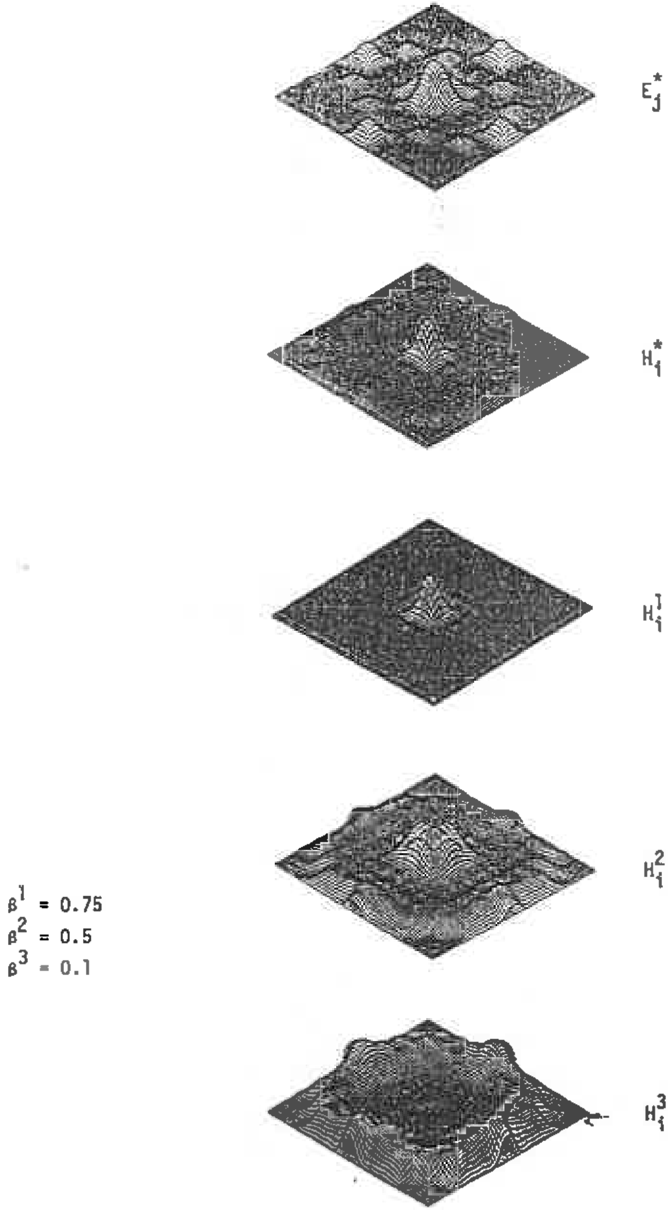

FIGURE 2(i) : Housing Distribution with Low β^w.

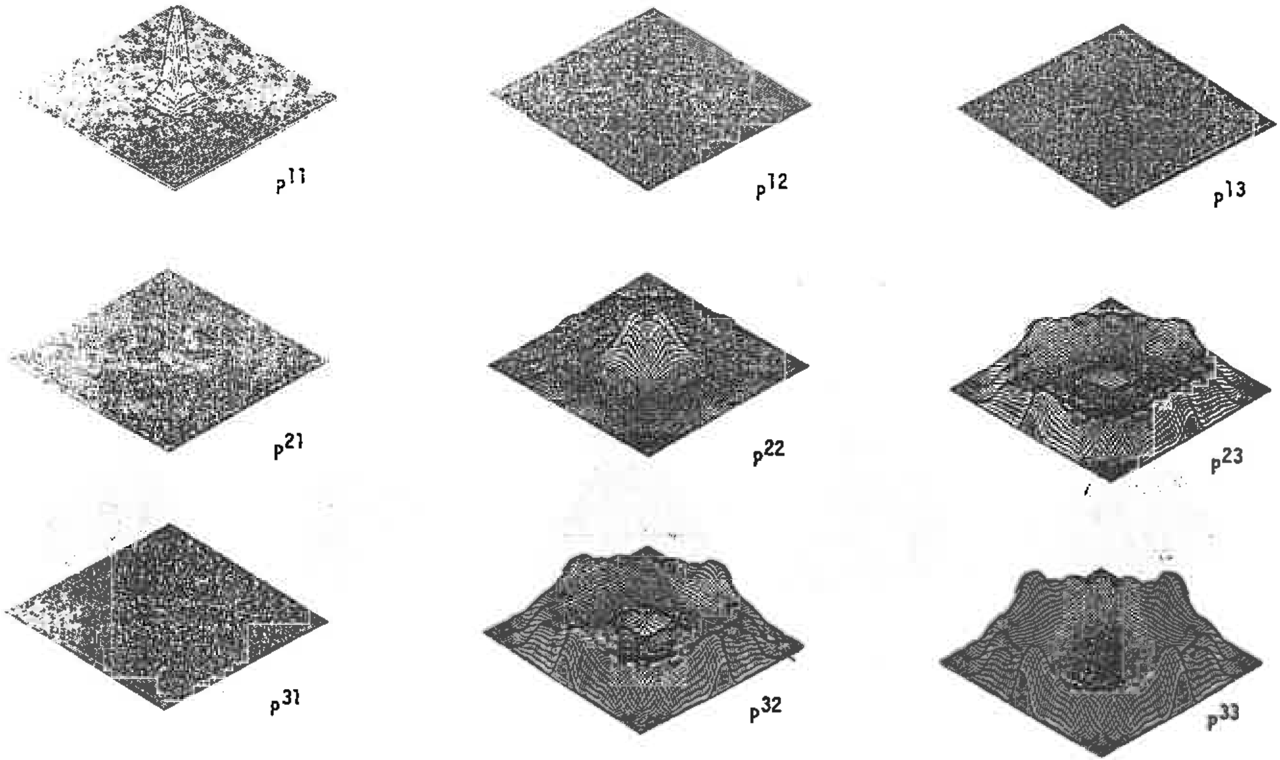

FIGURE 2(ii) : Population Group by Housing Type with Low β^W.