

MODELS OF NATIONAL SETTLEMENT SYSTEMS:
COMMENTS ON TWO PAPERS BY CORDEY-HAYES

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August 1974

WP-74-31

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Martyn Cordey-Hayes and I, though both members of the IIASA urban project, seem to have traded absences this spring. As soon as I got back from one lengthy trip, Martyn left for the Center for Environmental Studies for six weeks. Thus a lot of what would ordinarily be conversation between colleagues had to be written down. For research planning purposes and for the convenience of present and future colleagues in the urban group here at IIASA, we decided to take advantage of these circumstances and print his two papers and my reply with his comments as internal working papers. The present one, a lightly-edited version of a letter to Martyn dated 30 May 1974, makes no sense at all without reference to IIASA WP's 74-23 and 74-2. The paragraph numbering system below is taken directly from those papers.

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WP 74-23

(2.1.ii) Agglomeration economies probably do operate over sets of cities, as well as within cities, though the empirical evidence is somewhat wanting (Bergerman et.al., 1972); but what do models of this sort have to say

about them? Then (p.5), there is much controversy over whether diffusion of various sorts is hierarchical (or by what measures); a recent Lund monograph by Pred and Törnquist (1973) which was reviewed in the latest Annals (Borchert, 1974) may offer some help. It's on order. Otherwise, one might check the spatial parts of economic development literature: cf. e.g. Riddell (1970) for one example of a corpus of which I'm largely ignorant.

Question: Should we have a paper at the December conference on the nature of diffusion or information flow in urban systems? Indeed can we afford not to, given the proper stress on information fields in migration decisions in your section 5.6? Who'd be good?

Yes, I agree we should have a paper on the nature of diffusion or information flows in urban systems. Hägerstrand and Berry come as the first names to my mind. (M.C-H)

Question: A related question has to do with whether and how spatial restructuring, as opposed to education, might reciprocally affect diffusion, or "mean information fields", and that in turn brings up the definition question in 2.2.

(2.2) Curry and MacKinnon (1974) have been exploring Markovian approaches to urban policy modelling under contract to the Canadian Ministry of State for Urban Affairs. I have yet to finish plowing through this magisterial tome, but it

is clear that you and Ross have many interests in common. He arrives 2 September and will be staying for one year; I expect he and Les Curry will have plans for turning this somewhat rough manuscript into a monograph. A second point: the restructuring, abstractly considered, isn't very interesting, though its concrete regional manifestations most certainly are--as you later recognize (2.3): still, it's worth expanding on.

Lever's conclusion is directly contradicted by Berry's work (Lever, 1973; Berry 1973, vol. 1, p. 22); Berry's 1960's to be directly related to size, though with large variance. Of 106 daily urban systems (DUS), fully half of those growing at rates greater than the national average had 1960 populations or more than one million; the 14 DUS's in the size class 100,000-225,000 in fact had a negative population growth rate in that decade. This may be an artifact of two utterly different ways of looking at the tendencies inherent in the system at a given time.

But the contradiction stresses once again the necessity of a standard regionalization technique, insofar as data allow. I lean to Berry's labour-cum-housing markets (DUS's) or some variant thereof which can be applied in international comparative studies; it seems in principle applicable to a wide range of situations, socialist and market, traditional and modern, and makes technology, particularly urban transport and communications technology, assume a properly prominent place in subsequent analysis.

Question: Should there be a study of national data sources in (for a start) IIASA member countries to see whether an agreed DUS-type delimitation can be cobbled together and recommended as a base for comparative study? Who should do it?

Q.2 relates to Q.3 on page 3:

The problems of urban restructuring and 'urban' definition are well worth studying. Tobler has done some work on the latter and it may be something that he could trigger. Also Berry may be able to suggest someone appropriate. David Gleave could also do a useful job on this. (M.C-H).

The last sentence in 2.2 leaves one with the uncomfortable feeling that one can always argue that the present (or any observed) distribution, assuming no barriers to migration, represents the Panglossian optimum for the particular mix of individual value systems that happen to be extant. If so--if "city size" is traded off against all other things in a lifestyle specification and folks act accordingly--we can all go home. But again, the elaboration and qualification of such a view ought to find a place in December. Know any Voltaireans?

(2.3) Von Bönventer's point is a key one and requires amplification. If the plateau is broad (or our ignorance great) should the role of government be the provision of a wide, wide variety of ecological niches within "reach" of all citizens? That is, encourage place diversity and old-

fashioned geographers?--some come to mind, and, dismaying to the tidy scientific mind, they're always the best people-- Mather, Zelinsky, P. F. Lewis. But still, the positivistic program in the second last sentence is well taken.

(3.1, p.8) Niles Hansen's work ought to be relevant here. He's visiting for a day on June 7, and I shall pump him on these things.

(3.2) Unsure what you mean by "one-off applications": can you expand this a bit? Otherwise conclusion merely repeats the bald assertion (6.1.ii). And I admit my ignorance, but are there such things as genuinely dynamic input-output models?

My 'one-off applications', I suppose I mean case-studies or applications specific to an area. I.O. methods are very demanding on data and consequently at the time of writing I thought them inappropriate when aiming at a general synthetic model for impact analysis. But I suppose this need not be, one could invent 'typical' data. As I understand 'dynamic' I-O, it means one considers changes in the technical coeffs, and growth/change in final demand over time and the associated changes in investment required to meet the changes. (M.C-H)

(3.4) Mera may be valuable here too.

(3.7) The criticism implied in the first sentence ought to be given a sentence or two, especially given the

relatively lengthy exposition just preceding. I had never heard of FLEUR--ought we to have a spokesman from this group? Whom?

I'm equivocal about Paelinck and FLEUR. If Hansen and Mera come to the December conference then Paelinck isn't essential. I'll ponder this for a day or two yet. Paelinck will be at Karlsruhe so I can follow this up again if necessary. (M.C-H)

(4-5) These sections are the strongest in the paper--it's easy to see where your cool, objective, value-free heart really lies! Very little to add to an excellently developed argument. However:

(5.3) Surely, 5.3.ii can be enriched by some positive statements: Morrison, Leroy Stone, even your correspondent have been among the multitudes seeking explanatory models of migration. For instance, in 1966 (I think) I did a paper contrasting outmigration patterns for blacks and whites from the American South. While "traditional" measures of distance and economic opportunity performed well for whites, the only significant destination predictor for black flows was the black population of the destination. This was a crude stepwise regression model, but the near-zero contribution of a good measure of distance costs (busfares) for blacks was nonetheless surprising. I ought to repeat that with better statistical procedures and 1970 data.

(5.10) I agree with the direction your argument takes you; there is no doubt where the Holy Grail lies. But it may be a rough road.

(5.12) An excellent point, rarely stumbled on by policy people.

(5.13) They may be more universal than you modestly admit: at any rate it's worth having a go and then stretching to see if they're applicable in tough cases like a developed socialist country--USSR?

(6.1.v, p. 22) The case for this approach is well made. I think it's really up to you: if you have time to make a start on this and still produce the December paper, by all means go ahead. And if you would like to arrange for Paelinck or an associate to visit during the summer for a month or two, go ahead and see what you can arrange. The budget will stand it. If you want to stop off in Holland on your way back here, the budget will stand that too. As you say, we've got to get into this, the timing is the only question, and if you feel you want to make a start this summer, by all means go ahead.

(6.2) Yes, our ignorance of socialist methods here is great indeed. Kudinov is editing and translating some of the material given us in Moscow this April, and hopefully Sokolov will be able to sketch in the more technical parts. However, we should be realistically aware that the Soviets have not yet sent us their promised account, to be prepared in the Central Research and Design Institute for ~~Town~~ and Country Planning, despite our request of 3 April and subsequent orders by Chairman Fomin of the State Committee on Civil Construction and Architecture. Possibly translation is the

hold-up. But again, on the models used for analysis, it appears that Sokolov will have to make a special trip home to gather materials.

The lack of Soviet methodology literature concerns me also--I think that my December paper will cover only the literature known to me. Valeri Sokolov should take full responsibility for a short summary of the Soviet methodology literature related to national settlement planning. (M.C-H)

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WP 74-28¹

(i.1) The objectives are specified appropriately: how to model complex, poorly understood systems in ways presently useful to policy planners (though perhaps not decision-makers: cf the distinction between strategists and arbiters in IIASA WP 74-XX) and still provocative of good research. The first constraint leads to time-dependent prediction ('conditional forecasting'), transparency as to process, spatial and social disaggregation--but not necessarily to a high degree of numerical accuracy in the results. Describing the inherent tendencies of a system within given

¹This paper was later revised and extended by Cordey-Hayes and Gleave and is to be published as a IIASA Research Report.

policy limits often suffices. ' Imperfect realization of this demand from the policy side has been an important stimulus to large-scale metropolitan simulations in the U.S. and U.K.; the most trenchant critiques of the American attempts are by Brewer (1973) and Brown et.al. (1972). Your argument, though, goes from here to dynamical systems theory, which, though doubtless accommodating many variants, leaves me with the feeling that the meal has been selected before the menu has been thoroughly studied. What really is the range of choice for the problem at hand?

Yes, the meal was chosen without elaborating the menu. These notes were also intended to reveal my prejudices at an early stage. (M.C-H)

(1.2) What is "the Lagrangian viewpoint of classical mechanics"? It sounds interesting and ought to be worth a sentence or two for non-physicists.

(3.3) The key to the whole of section 3, and particularly to the idea of constructing a simulation based on "escape probability" (ξ_1) and "capture cross-section" (μ_j)--marvelous terms, equally applicable in urban studies, physics, but especially penology--is exactly "an interpretation of these parameters in terms of hypothesized causal relationships".

Question: Does the migration literature allow this? Should we do, or commission, a review of the behavioural literature at some early point? Or is basic work, as opposed to time- and place-specific calibration, yet required? (There are

also questions of a fuller specification of the demographic side, perhaps in the Rees and Wilson (1973) format.)

The CES work is aiming to interpret ξ_i and Q_j in terms of a stochastic approach to the labour market--with a dynamic concept of job vacancy being the key variable. Don't think we need a review of the behavioural migration literature, but would be useful to have someone working on this. Also relates to the earlier question of information diffusion. Sweden is where one might look for someone on this topic.

Yes, the model needs to be linked to a demographic accounting framework. But to make quick/interesting progress have assumed births equal deaths, this can be relaxed in a variety of ways at various levels of sophistication. But see below. (M.C-H)

Question: As a matter of modelling tactics, when does one incorporate the "external forces" into a single simulation and when does one accept or model them exogenously?

(3.4) Since you wrote this, you have solved the ξ_i Q_j model analytically and compared the trajectories with a more standard first-order Markov model based on $[a_{ij}]$. Which is to be preferred, and why?

The Markov should give better results, but then it uses ten times as much data. What I'm trying to do is develop a family of models of national settlement systems at varying levels of approximation. (This tries to avoid the common problem of model building which always says I'll have a great model for you in X years at Y' thousand dollars... Then after X years, "nearly ready, only need another year and Y thousand dollars" and so on.) At the simplest level one should be able to do quantitative calculations on the back of an envelope that interest policy makers. We have these already, and are considering the characteristics of the second, third and fourth levels of sophistication. And who knows the crude estimates may be just as good as the dynamic I.O. linked to a Rogers/Wilson demographic model. (M.C-H).

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On the whole, I am much impressed with the work presented here, agree with its implications for a research strategy, and await the further amplification promised in (4.5). A well-developed strategy, however, would have to fit these demographic and economic concerns into the wider

framework of settlement strategies and urban policies. If this approach gets at the core mechanism in the growth and development of an urban system, it still requires constraining by normative and resource considerations. A genuine and short-term goal for the group must be the 'mapping' of the system in question, even if only in a preliminary way. But as you say, that's another paper.

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