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Painting by numbers. Mathematicals models of urban system.
Pintando por números. Modelos matemáticos del sistema urbano.

Mark Smith

Filiation
Departament of Urbanism, F.A.U. University of Chile

Abstract
The objective of this paper is to contribute to the understanding of the phenomenon of mathematical modelling in urban studies and to stimulate a deeper debate about the use of models in planning. This is done by means of an identification of key assumptions made in the process of the interpretation of reality as a set of algebraic relations; assumptions which, it is argued, underlie virtually the whole of mathematical modelling but which appear never to be openly discussed. An example of the use of these assumptions is then given in the theoretical development of the logit model of discrete choice, and the paper is concluded with preliminary recommendations about the future development and use of the mathematical model.

Key words
Mathematicals models of urban system.

Resumen
El objetivo de este trabajo es contribuir a la comprensión del fenómeno de la modelación matemática en estudios urbanos y de estimular un debate más profundo sobre el uso de modelos en la planificación. Esto se hace por medio de una identificación de los supuestos básicos en el proceso de la interpretación de la realidad como un conjunto de relaciones algebraicas; supuestos que, se argumenta, la base de la práctica totalidad de los modelos matemáticos, pero que parece que nunca se discuten abiertamente. Un ejemplo de la utilización de estos supuestos se da entonces en el desarrollo teórico del modelo *logit* de elección discreta, y el documento se concluye con recomendaciones preliminares sobre el futuro desarrollo y el uso del modelo matemático.

Palabras clave
Modelos matemáticos del sistema urbano.

Contents

1.- Introduction

Much as in any aspect of social science, mathematical modelling has its enthusiasts and its sceptics. The enthusiasts accuse the sceptics of not understanding the models, and the sceptics in turn accuse the enthusiasts of not understanding the reality. As in the case of GIS therefore (Sheppard, 1995) the problem is perhaps not so much the rights and wrongs of modelling itself as the lack of communication between the two camps; to use the respective stereotypes, there are the intellectually insecure pseudo-scientists versus the woolly-minded ignorants.

A stumbling block in the debate is a certain confusion about what exactly is the phenomenon of modelling; opinions seem to range from those who believe it a means of justifying undemocratic decisiones but to not sure how, and those who believe it science but can’t say why. The aim of this paper is to stimulate this debate by examining the roots of the process of modelling, and suggest future directions for research in the field. As a critique of modelling of urban systems, the intention is not, as in the famous paper of Douglass Lee (1973), to write their "requiem" so much as to re-visit the basics (if ever they have been visited) before the probable boom in technical advance predicted by Openshaw (1995) materialises. This paper takes as a starting point a perspective more akin to that of Lee's less publicised contemporary Tribe (1972, page 77), who states that the accepted methodology has rested upon ″purely 'objective' modes of relation between the observer and object of observation," this premise, "deriving in part from insecurity about the intellectual credentials . . . . . of social science".

2.- Is modelling objective science?

What is science? Of course this depends on whom one asks. Amongst modellers there is an implicit acceptance that it necessarily involves the use of mathematical description. Undeniably, the language of mathematics has its appeal. Once translated into this language, a description has properties which are independent of the observer; anyone who knows the language will be able to make the same deductions from the same description. However this property, of endogenous objectivity, is purely internal to mathematics - the interpretation of reality as mathematical structure *per se* cannot be said to be objective. Exogenous objectivity of this latter form, a property characteristic of 'hard' science, is not intrinsic to the mathematical language. The only case where it can be said to exist is when there exists a controlled and repeatable process of observation of the real phenomenon. This in itself has nothing to do with the use of mathematical language (except for the requirement that the language must possess a degree of internal rigour). The fact that this is possible within definable error for many natural systems does not infer the right to model any system at any level of complexity mathematically without such a process of observation, and claim objectivity.

This leads us to a useful definition of objective science for the purposes of this paper - that of a process of observation. One is not suggesting that other forms of science are less valid; there is an increasing body of work for example in urban systems (exemplified by Allen and Sanglier 1981, Batty and Longley 1989) which develops models as learning tools, without the same emphasis on predictive capacity. The issue here is not so much 'science or non-science' as the *type* of science to which modelling aspires. Much of the language used in modelling appears to suggest that this is predictive, objective science. Phrases such as "models improve knowledge of reality" or "much can be learned from model behaviour" slip easily off the pen, but need more careful consideration. Mackett (1993) and Openshaw (1995), amongst many others fall into this type of trap; language of this sort implies a definite *deduction* of reality from the model which requires a degree of objectivity in the *induction* from that reality. Any objectivity possessed by a model arises from the property that observations made are repeatable by any observer at any place or time within experimental error.

One is given to asking how many models of urban systems are subjected to such criteria, and how many would stand up to them. The common disclaimer that one cannot apply the same degree of rigour to social systems is quite irrelevant; without the same degree of rigour one cannot employ the same methods nor the same language nor the same concepts with the same degree of liberty as modellers frequently do. The difference between the 'hard' and 'soft' sciences is deeper than just the size of the error term (see below discussion). To pick examples at opposite ends of the scale, one might consider:

(a) Models of traffic flow. Given that in a single lane of traffic, one is analysing a 1-dimensional movement in which the human influence is observable only in terms of distance, speed and acceleration, and in which the emotional motivation is a relatively simple one, it may be possible to define appropriate limits of tolerance and to formulate a mathematical expression whose predictions may be falsifiable by observation falling outside those limits.

(b) Models of land-use. It is difficult to imagine a series of controlled experiments with urban development which could confirm numerical laws of long-term land-use changes. The basic problem is that the typical time scale of the development is similar to the evolutionary time scale of the system itself. In such ontogenetic systems, where the very rules themselves evolve with the system, it is generally impossible even to define useful limits to quantitative behaviour on a macroscopic scale. In the absence of such limits, it is by definition impossible to verify a model by any experiment.

Of course there exists a whole range of models within these two extremes, with varying degrees of verifiability. Given that there exist models of urban systems which are accepted and used (Klostermann, 1994), and which appear to be unverified in this manner, one must ask the question, "If not science, what?"

3. -So is it art?

Continuing the theme of the process of observation, one might pause to consider as an answer to the above question the idea of art. In this instance, different qualities are valued in the observation of reality. Contrary to the observer-independence of science, an artistic interpretation is in general identified specifically with one person or group, and the more uniquely so the better. (One is here considering art in the contemporary Occidental egocentric interpretation.) The artist is expected to put their own distinct contribution to the work in a way that no other does, although their observation is expected to reflect some universal or common truth in human experience. If this contribution includes political or cultural bias, so be it.

Acceptance of a work of art depends partly on this ability to invoke common intuition and partly also on certain peer group norms. These norms are not themselves fixed but evolve with innovation, technical advance and changing cultural environment.

There are interesting parallels between this description of art and the dynamic of the modelling community.

Whilst there are ideas of seeking universal qualities in the description of urban systems, there is also a great diversity of styles and individual interpretations, as Wegener (1994) and Webster et al (1988) demonstrate. This may be seen on the one hand as representing a positive creativity in approaches to a difficult problem, or on the other as evidence of niche-building, depending on one's point of view; however the fact that all these examples are considered legitimate efforts points to the existence of some form of peer group norms. Given that the relative looseness of empirical support removes an external point of reference, acceptance is much more dependent on the internal dynamic of the academic community and the language it employs.

However, these parallels will only extend so far. An important difference is that in art there appears to be little consensus as to the existence of a fundamental, unassailable base from which all artistic activity is derived, as the ever more introspective extremes of postmodernism perhaps demonstrate. Mathematical modelling however contains an implicit, accepted interpretative foundation - the use of a common medium of expression (algebraic language) is in itself sufficient evidence of this. However this foundation, as far as the author is aware, has never been explicitly stated - hence perhaps the confusion over the status of modelling. The next section will attempt to remedy this by setting out three key assumptions common to almost all modelling of urban systems, which between them form that foundation.

4.- The three canons of modelling

A.-If you can count it you can calculate it

There can be no assumption more fundamental to algebraic modelling than this, which relates a form of observation to the very concept of number itself. Mathematically it is not in fact always true.

Mathematically, the process of counting consists of creating a bijective map (a one-to-one correspondence) between an observed set of objects and an abstract set of discrete, ordered symbols, thereby assigning to the observed set the cardinality corresponding to the ultimate symbol used in the abstract one. (Cardinality is the set-theoretic term for the 'size' of a set.) In everyday terms this means, for example, that we count the fingers on one hand by assigning to each in turn a member of the set N = {1, 2, 3, 4, 5, 6, ...} and when the fingers are exhausted taking the last-used member of N (usually 5) to represent the 'number' of fingers. However, one does not need to use the set N. Equally one could use {A, B, C, D, E, ...} or {glue, sporadic, knees, glbpht, dog-biscuit, ...} and the result (E or dog-biscuit) would be mathematically equally meaningful. The difference with the set N is that the symbols used have associated with them a spurious meaning due to their use in algebra. This meaning is derived from the fact that that there exists in algebra a set of relation between the symbols, which are defined by a set of 15 axioms. These axioms define all the commonly understood operations of addition, subtraction, multiplication and division, and completely define the properties of the real number system. The real numbers themselves are defined by these relations and have no objective meaning as symbols outside of them. Now of these 15 axioms, only six suffice to define an ordered set of elements, basically those defining the relations of succession (symbolically <, = and >) which apply equally to any other ordered set such as those above mentioned as to the set N. The mere use of the symbols of the set N for counting does not allow one to infer the validity of the other 9 axioms, with all the algebraic relations they imply.

Indeed this argument holds equally for any form of measurement other than counting. Given that any measurement has a finite precision, the process of measurement simply becomes one of counting the number of smallest observable increments.

The unique justification for involving the other 9 axioms is when the relationships postulated can be confirmed by observation.

A common example of this collapsing of a concept into a variable is the 'benefit' of cost-benefit analysis. In the evaluation of a road project for example, where benefits are associated with time savings, it is reasonable to suppose that for a given person at a given moment in time a quicker journey is preferable to a similar but slower one - implying an ordered relation between journey time and benefit. From this assertion, the assumption of calculability is deployed to create a numerical value of the 'benefit' of an individual out of a phenomenon apparently displaying nothing more than an ordering property.

An instance where one *could* justify the use of real numbers is in the use of indicators. If one uses a mapping between observation and number which preserves an ordering property of the former, one can use the indicator arrived at for *comparison* between cases (as comparison uses only the relations <, = and >). However, the moment one begins to *calculate* figures from the same number, the indicator becomes a *variable*, relationships involving which need to be justified. For example, one may use average L_{10} noise level as an indicator of acoustic pollution in an area, for purposes of comparison with other areas. If this figure is used to predict, say, house prices in that area however, there is immediately a more-than-ordering relation with other quantities.

This assumption of calculability is the that upon which the entire process of modelling is founded. Given that this assumption has been made therefore, and that algebraic relations are to be employed, a second crucial assumption is then made:

B.- It's not what you leave out but what you put in that matters

The second assumption relates to the obvious idea that what is left out of the model is either constant or negligible. This is an assumption universal to any form of model (if one builds a miniature replica of a building, one might assume that not reproducing every fibre in the carpets will not detract from the model's usefulness) but in mathematical terms it has a specific interpretation.

Taking a general form of model (one does not claim it to be the most general, merely to be illustrative) to be a relation of the type:
$x_i = f_i(x_1, x_2, x_3, . . . x_n, t)$ $i = 1, . . . , m$
- two things are immediately obvious. One is that the choice of variables $x_1, x_2, x_3, . . . x_n, t$ will determine the phenomena included in the model as a whole, the other is that for any given function f_i, the arguments of that function will determine the possible interactions with the variables. These define the limits of the model. Everything not explicitly included is assumed disjoint, constant or negligible by default. It is this process of assumption by default which is the aspect of this assumption least often recognised.

It is normal when building a model to think in terms of choosing interactions, but in doing so one is implicitly also choosing invariances. The choice of connections is an active one and the choice of eliminations a passive one. However, given the concept of a model as a simplification of reality, it would be more logical to make the choices the other way round, i.e. passively assume no invariances and actively eliminate possible interactions one by one, taking into account what is lost each time. One might characterise this as a "top down" approach as opposed to the normal "bottom up" one.

The assumption implicit in the bottom up approach is that with every functional relation added, the model gets in some sense 'better'. What is better (or worse) in modelling terms is not always defined explicitly, but generally tends to appeal to the idea of accuracy. This is a back-justification of the first mentioned assumption; in the concept of accuracy is implicit the idea that a 'perfect' model exists, one that will generate numerical outputs which correspond to observed values within some pre-specified error. These outputs may be phenomena clearly non-numerical and unobservable, such as the above example of benefits of a project, but the idea of accuracy is employed regardless. Even when the outputs of the model are observable quantities, the difference between the observed and calculated values, if they are due to qualitative misconceptions in the model, cannot be treated as merely numerical errors. The idea inherent in the bottom up approach is that as one adds more factors into the model, it will somehow converge to this 'perfect' model. It is logical to suppose that with the top down approach that elimination of factors (numerical or otherwise) will somehow diminish the representational power of a model, but the converse is not necessarily true with the bottom up one. There may not be some path-independent process that necessarily leads to an ideal point, but an infinity of possible paths diverging to quite different models. The idea of the model as an approximation of reality, without any recognition of the significance of 'proximate', persists however, and leads to us to the third fundamental assumption.

C.- That which is not deterministic must be probabilistic

This assumption relates to a way of disguising the previous two, by treating any numerical discrepancy between model and reality as mere random error.

If the idea of 'approximation' is used to hide the assumption of the existence of algebraic relations, this is in turn hidden by the idea of the 'error term'. The error term is supposedly a random term frequently added onto the end of equations to acknowledge the modeller's ignorance of the real system. In the case where the equation calculates an observable quantity (i.e. not a fictitious variable such as utility, as in the below example) it is obviously tautological to say that the error term makes the equation numerically correct; given a sufficiently loose (and therefore meaningless) definition of the error term, any equation using one can represent any quantity correctly. The value of the error term therefore resides entirely in its precise definition.

In the physical sciences, it is common for the error to appear in terms of limits. These limits describe uncertainty in the observations, which will translate into corresponding uncertainty in predictions. Probability appears when for a sufficiently large number of controlled experiments on an isolated system, observational errors due to limited precision of measurement or sampling error can be assumed to follow some distribution subject to the laws of probability.

Now probability is a quantity which can only be defined under either one of two conditions:

(i) if one has an *a-priori* idea of equiprobable outcomes, based on knowledge of the system's dynamics, e.g. if a coin has two equal faces, the probability of tossing heads equals 0.5.

(ii) if one can perform a series of experiments in which the normalised distribution of the outcomes always tends to the same distribution.

It ought to be obvious that in the social sciences, there are many phenomena which are not, at least at any observable level, deterministic. However, neither can one consider them probabilistic (*at that level*) unless one of these conditions holds. There are many instances, for example in economic theories (see example below) where although neither condition holds, the idea of probability is used to patch up conceptually contentious models. To deterministic and probabilistic must be added a third category of system, the indeterminate, whose definition depends on the observability of the system's behaviour. At the level at which we observe social systems, non-deterministic does not therefore imply probabilistic.

An argument commonly used for such use of the error term is that the error accounts for the modeller's ignorance of the 'exact' value of a variable. However ignorance is no excuse for employing notions of probability in models - on the contrary the definition of a probability actually requires *a-priori* knowledge. An error term, where meaningful at all, cannot necessarily be assumed probabilistic.

As an aside, many models using this concept of assumed probability, such as the logit model below analysed, are very often totally deterministic. Variables are assigned error terms with a fixed distribution, and an optimum outcome (referred to as the "most probable" outcome) calculated. This is merely a deterministic model with extra parameters, those describing the distributions assumed. To call it probabilistic without qualifying the assumptions made in using the word may be at least misleading, and possibly wrong. The outcome of such a model is always the same, unlike the unambiguously probabilistic type of model where the outcome depends on some random number generator called by the model. This latter class of model, exemplified by the already mentioned examples of the urban evolution model of Allen and Sanglier (1981) and the fractal growth model of Batty and Longley (1989), represents a genuine recognition of ignorance of the influence of small-scale events. This contrasts with that school of thought which assumes that these events result in, or are subservient to, macroscopic forces which always drive to model to some invariant optimum solution as calculated by maximum likelihood methods, e.g. Anas (1982, ch 4). However, the difference between a random model and a model of randomness still fails to be recognised in many cases, such as in so-called "random utility" theory (Manski, 1977) or its complement "random bidding" theory (Lerman & Kern, 1983).

The aforementioned idea of equilibrium is closely tied to that of probability. On the one hand it is explicitly a feature of the idea that (for example) an urban system will, like some isolated thermodynamic system, tend to some entropy-maximising 'optimal' state dependent on the probabilities assigned. Miyagi (1986) in fact shows a mathematical equivalence between random utility and entropic models. On the other

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hand it is implicit in the assumption that probabilities exist. Given that no clear equiprobable outcomes exist for a given process, one needs to invoke condition (ii) above to be able to define a probability. Clearly, to be able to observe such probabilities, one would need to observe (or theoretically be able to observe) a tendency towards a steady state which is not influenced by external processes, i.e. an equilibrium state. The concept of probability therefore brings on board an implicit idea of equilibrium which may not always be recognised, or desired.

In summary therefore, far from being a catch-all assumption which corrects flaws in imperfect knowledge or a genuine recognition of observational errors, the use of probability brings with it a basket of other implicit assumptions which may well compound the conceptual error.

5.- Example - the logit model

As a means of illustrating the modelling process as represented by the aforementioned three canons, we shall consider here the conceptual development of a model commonly used to describe (among other things) urban localisation, the logit model of discrete choice. (See various examples in Webster *et al*, 1988).

This typically has the form:

$$p_i = \frac{e^{U_i(\underline{x}_i)}}{\sum_j e^{U_j(\underline{x}_j)}}$$

where: p_i is the ‘probability’ that the actor chooses option i,

U_j is the expected ‘utility’ associated with option j,

$\underline{x}_j$ is a vector of attributes possessed by option j.

The first assumption, that of quantifiability, is deployed in the creation of the utility variable - a classic example of the use of all 15 axioms without any apparent attempt at justification. In the formative debate on utility theory, Kaldor (1939, page 551) stated of the economist that, ". . . the scientific status of his prescriptions is unquestionable, provided that the basic postulate of economics, that each individual prefers more to less, a greater satisfaction to a lesser one, is granted," and this remains ingrained in current orthodoxy.

The relations Kaldor invokes here, of more/less, greater/lesser, are relationships merely of ordering. Despite this, although utility is rarely assigned an actual value, it is typically treated as an algebraic variable, which implicitly therefore has a numerical value subject to arithmetic operations. Similarly to the case of ‘benefit’ given above, the supposition that at any given moment the options available to a person are in some way ordered according perceived ‘satisfaction’ is reasonable enough, but the conceptual leap from ordering to quantification seems in general to be made unthinkingly.

A good attempt at justifying this conceptual leap may be found in Ben-Akiva and Lerman (1985, pg. 39). Considering consumption bundles $Q^i = \{q_1, . . . , q_n\}$ where q_k represents the quantity of good/service k, they define rational behaviour in terms of a "transitive preference ordering" of the form
Q^i ³ Q^j & Q^j ³ Q^k Þ Q^j ³ Q^k (1)

They deduce from this the existence of an *ordinal* utility function $U = U(q_1, . . . , q_n)$ which (whilst correctly distinguishing it from a cardinal one) in their words, "expresses mathematically the consumer's preferences and is unique up to an order preserving transformation" (page 40).

What does this phrase "order preserving transformation" mean? It means that the preferences described by the transitive ordering (1) can be described by any function of the quantities $q_1, . . . , q_n$ for which the corresponding values of $U(Q^i)$ maintain the same order as in (1). This by implication includes numerical functions which fulfil this condition as a subset of these ordinal functions. However, in any given case, the existence of a numerical function of $q_1, . . . , q_n$ displaying the same ordering as (1) is not the issue; this is in general a trivial matter. If the function U is cardinal, then so must be the corresponding transformation - the assumption has merely been shifted from the existence of the utility function to that of this transformation. For the model to be representative of real choices, the order would have to be further preserved under the process of aggregation and all the transformations in the model; this will not be guaranteed merely the by existence of such a function U. In this example therefore, the assumption is not eliminated, merely concealed within the nuances of the descriptive language employed.

The second assumption, that of the interactions, manifests itself in the choice of attributes $\underline{x}_j$. Conversely, as stated above, this is also the choice of invariances, i.e. what attributes, quantitative of qualitative, are not included.

Utility may be intuitively defined as the perceived satisfaction derivable from a given option. Given the subjectivity of 'satisfaction' it is inevitable that the definition relies somewhat on intuition. Therefore, intuition being the intrinsically holistic phenomenon that it is, it makes more sense to start with the intuitive concept and ask what one loses by subtraction than to try to construct 'satisfaction' from scratch.

An appropriate metaphor might be finding out how a car works. A person who has no idea about mechanics might go about learning by two ways. Either they might piece by piece dismantle a vehicle, finding what properties are lost each time (the "top down" approach) or they might go to their local hardware store and try to build what they think is a car from the bits and pieces they find there (the "bottom up" approach).

Most people would think the top-down method more sensible; however the bottom-up one appears to be the dominant paradigm in modelling. In terms of the car metaphor, utility is would be equivalent to some variable (or perhaps a transformation of a variable) such as 'car-ness', which has a value of 1 for a complete vehicle and close to 0 for most other objects. The bottom-up approach implicitly assumes that adding a further component to the assemblage will augment its car-ness, making it tend towards the limiting value of 1. However, this of course ignores the multi-dimensional nature of the functional relationships; adding a windscreen wiper to the glove box will not contribute to the authenticity of the vehicle. Likewise, adding more attributes to a utility function may not improve its representation of satisfaction.

The same metaphor is also useful for illustrating the third assumption, that of random error. Imagining the motley assemblage of washing machine spares and garden implements which an incompetent modeller might have constructed to represent a car, the error term would be equivalent to a magic component which is capable of taking on any guise such that it 'corrects' the numerical difference between the "car-ness" of the assembly and 1. It of course cannot correct the inappropriateness of car-ness as a useful description of a car.

In the present example, the logit model calculates 'probabilities' of different choices based on the assumption of a certain form for the error term attached to the utility function. This form however, the Gumbel (or Weibull) distribution, is not based on *a-priori* knowledge of the utility; as Ben-Akiva and Lerman (1985, pg. 104) state, "the assumption that the disturbances are Gumbel distributed . . . . is used only for reasons of analytic convenience." (The convenient property is that the maximum of a set of Gumbel-distributed variables is also Gumbel-distributed.)

Thus the logit model cannot be said to calculate choice *probabilities*; rather it constitutes (in the discrete choice case) a *partition* function of individual choices, deterministically optimised over a set of fixed 'acceptability' functions (the Gumbel distributions, whose common dispersion parameter determines the sensitivity of the calibrated model to the dispersion of the data).

This example serves to illustrate the anatomy of the modelling process. The purpose is not to rubbish the logit model; it makes no more nor less sense to say the model is "rubbish" than to say it is "accurate" or "correct". The intention is simply to shed some light on the interpretation process which is modelling.

6.- What price a science of modelling?

As argued above, claims of mathematical modelling to be scientific will always be debatable without deeper understanding of its basic premises. The hypothesis of this work, which requires a range of analysis and case studies far broader than is possible in any one paper, is that *the three above identified assumptions between them underpin almost all modelling, and may be made unthinkingly.* One is often expected to accept them without attempt at rigorous empirical justification of the model, as in the case of land-use models. Mere calibration on a specific case does not constitute justification of the assumptions employed; the model may thus become an economical description of the data but will still be a subjective one. Validation on an independent data set will improve the situation, but only if the data covers all possible situations for which the model might be used. In any science, the best a model can do is set limits of outcomes within known numerical error, and if the errors are qualitative or are due to unknown causes, limits cannot be set and the model can have no pretension to be either predictive or objective.

Does it matter whether or not a model can be said to be objective or not? The answer is no - for as long as the model is not used to support planning decisions which influence people's lives. In this case it becomes highly important that the model as a phenomenon be understood in its entirety, both in its derivation and its effects. Wachs (1985) discusses the use of models in planning in the light of such effects. The mere assertion that a model is the "best we have" does not justify its use in decision-influencing without this understanding - in the absence of which the word "best" cannot be meaningful.

A consciousness of urban systems involving modelling would therefore be unlikely to be simply a matter of numerical description; rather it would have to include:

(i) a science of the process of observation and interpretation of the urban system, which seeks to actively discover the intrinsic gains and losses of the processes, and not passively ignore them.

(ii) an understanding of the effects of the use of the model, via decision making, on the urban system itself.

7. Which direction for modelling therefore?

Currently, the common expectations for modelling are mainly restricted to the realm of technical development - increased power and sophistication of computing equipment, better algorithms for solving numerical problems and so on. The orthodox answer to criticisms about modelling has thus become to state that in n years time we will be able to do it faster and better (the latter being an appeal again to the often spurious idea of accuracy). Most vocally amongst these is Openshaw (1994, 1995), but one might add Mills (1987, p711), Birkin *et al* (1995) and many others. Such technical advance is to be applauded, but ought not to be allowed to lead to a generation of modellers who have no deeper understanding of mathematics than the ability to manipulate formulae and program computers. To slip into the mentality of "we've done all the philosophy years ago, now there's just the numbers left to do" may simply lead to adding floors to a building without foundation. This is not to question the undoubted value of contributions such as the regularly cited Wilson (1970) or Domencich and McFadden (1975), merely to contend that their pleasing theoretical roundness does not necessarily mean that the "totality of the phenomenon [*of urban activity*] could be explained," as suggested by De la Barra (1995, page 250; this author's translation of quote). The fundamental debate must be kept open.

A problem in this is a certain stigma attached to subjectivity - a common perception being that subjectivity precludes any possibility of the existence of a science of planning. This perhaps represents a laudable desire for impartiality in planning, but impartiality is *not* the same as objectivity. A person who supports neither team in a drawn football match may be impartial, but their opinion about which team was better would still be subjective, depending on aesthetic preferences of style, seating position in the stadium etc. Equally, a modeller might have no preference for model A or model B on technical grounds, but if the assumptions made in the two are different, there is still a subjective choice to be made, which may have unforseen consequences in resultant planning decisions. However, if this subjectivity in itself becomes a legitimate subject for study, there is no reason why a science of some sort should not be possible.

Modelling of social systems must therefore embrace subjectivity, not just as a necessary evil, but as a positive asset, as in the arts. The more that modelling is perceived an occult activity pursued by a select group of specialists shielding their arcane 'knowledge' from the public eye, the more it is likely to be criticised as elitist and anti-democratic.

This phenomenon may extend well beyond the realm of urban planning; there exists a wealth of literature, typified by the popular contribution of Capra (1982), alleging a domination of politics by so-called "economism" of a highly mechanistic, and unsustainable, nature. Economism, by which is here meant the metaphysical belief system which connects human emotion and desire to highly complex mathematical descriptions of international commerce, may well be seen to be founded in the same three canons - this is left for speculation.

The foregoing discussion leads to two principal recommendations:

(1) That a fundamental requirement of a model is that it be transparent, i.e. that all the assumptions made be recognised and presented as an integral part of any results quoted from the model, and their influence over those results understood where possible.

(2) That the value of a model as a planning tool be judged on its ethical effects as part of the whole decision-making process, not on unsupported considerations of technical merit.

Perhaps a cue could be taken from visual forms of modelling, as employed in architecture or more participative forms of urban planning. Be it in 2 or 3 dimensions, a visual simulation is a perfectly transparent representation of a reality existent or projected. The assumptions are there for all to see - if the cars in an artist's impression of an urban motorway appear six inches high compared to the happy pedestrians, one can laugh and say that the artist must have shares in a road construction company. If one sees nothing but a mysterious number labelled 'benefit' one can say nothing.

If mathematical modelling means that the artists paint by numbers, so be it. The numbers however must not remain hidden by a glossy top coat.

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