Qualitative Modelling in System Dynamics

or

What are the wise limits of quantification?

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Abstract

The tradition in system dynamics is that a problem can only be analysed, and policy guidance given, through the aegis of a fully quantified model. In the last 15 years, however, a number of purely qualitative models have been described, and have been criticised, in the literature. The paper briefly reviews that debate and then discusses some of the problems involved in quantification. Those problems are exemplified by an analysis of a particular model which turns out to bear little relation to the real problem it purported to analyse. Some qualitative models are then reviewed to show that they can, indeed, lead to policy insights and five roles for qualitative models are identified. Finally, a research agenda is proposed to determine the wise balance between qualitative and quantitative models.

Key Words: Qualitative Models, Quantitative Models, Influence Diagrams, Research in System Dynamics.

Biographical Details

Professor Geoff Coyle has been active in system dynamics for more than 30 years. He has published two textbooks and many papers and consulted widely. He was the first recipient of the System Dynamics Society's Lifetime Achievement Award. Geoff is currently Visiting Professor of Strategic Modelling at the University of the South Bank and an expert advisor to HVR Consulting Services Ltd as well as consulting and teaching on a freelance basis.

Introduction

The discipline of system dynamics has long been based on the building of fully-specified quantitative models of strategic problems in all manner of domains. Such models were, and are, seen as the essential means by which the dynamics of a problem could be simulated and from which insights might be generated into policies to improve system behaviour. The originating text in the field (Forrester, 1961) defined this theme which was followed without hesitation in subsequent textbooks (Coyle, 1977; Richardson and Pugh, 1981)

A cardinal point in this *genre* was that the dynamics of a system can not be inferred simply by reasoning from an influence or causal loop diagram and that quantified simulation is the *sine qua non* of policy analysis. It is, however, worth recalling that one of the earlier system dynamics texts contains numerous exercises in which the reader is asked to reason about the dynamic behaviour of fairly complex systems (Roberts *et al*, 1983).

By contrast to the original emphasis on quantification, the early 1980s witnessed the development of purely qualitative modelling in which only an influence diagram was drawn and there was no simulation. Several sources emerged practically simultaneously but Wolstenholme and Coyle (1983) took the view that there could be value simply in rigorous approaches to system description. Description of the system might be a precursor to simulation, in which case it would be valuable for the description to have been rigorous and disciplined. However, the case example, that of the nomads of the Sahel, seemed to them to involve so much uncertainty and doubt about the values of the parameters that simulation might be of questionable value. This theme of qualitative work has been developed in further refereed papers (Coyle, 1983, 1984A, 1984B, 1985; Coyle and Millar, 1996; Coyle and Alexander, 1996; J Coyle *et al*, 1999; Wolstenholme, 1983, 1985, 1999). It has been expounded in textbooks which also deal with quantification (Wolstenholme, 1990; Coyle, 1996).

In none of this work was it stated or implied that dynamic behaviour can reliably be inferred from a complex diagram; it has simply been argued that describing a system is, in itself, a useful thing to do which might lead to better understanding of the problem in question. It has, on the other hand, been implied that, in some cases, quantification might be fraught with so many uncertainties that the model's outputs could be so misleading that the policy inferences drawn from them might be illusory.

This stream of work has attracted some adverse comment. Lane (XXXX) has gone so far as to assert that system dynamics without quantified simulation is an oxymoron and has called it 'system dynamics lite (sic)'. Clearly, if someone had the authority to decree that system dynamics **must** involve simulation then the qualitative work is not system dynamics, but that is a road to fragmentation.

Richardson takes a more balanced view. In an important paper on problems in the future of system dynamics he reviews the qualitative work and argues, citing other work, that dynamic behaviour cannot be inferred from qualitative models, though the qualitative authors never said that it could. He poses the question of 'what are the wise uses of qualitative modelling?' (Richardson, 1996). Later in the paper he rephrases it as ‘when to map and when to model?’. Richardson revisits the same theme later and calls for rigorous research into the limitations of qualitative models (Richardson, 1999). This paper supports Richardson’s identification of the need for research, and will propose an agenda, but places the emphasis on the *relative* limitations of quantitative *versus* qualitative models.

Further support to Richardson’s argument appears to come from data kindly supplied by a leading consultancy which suggest that quantitative models generally give more insight into problems than do qualitative models. The firm stresses, however, that the claimed insights were on the part of the consultants, not the clients. While it would be rash to dismiss data from such an authoritative source it is hard to know how much weight to give to them. What is an insight, what were the circumstances of each case, how experienced were the consultants?

In short, there is a serious need for very high-quality research into the problem of the proper roles of quantified and diagrammatic modelling. Are we, as Nuthmann remarks, in danger of producing ‘plausible nonsense from our [quantified] models’ (Nuthmann, 1994). Where does the wise balance lie?

The authors most active in the qualitative work have had decades of experience of quantified modelling and continue to practice the art. One should not speak for colleagues but the present author has no doubt that, in the right circumstances, quantified models can be valuable tools of policy analysis. It is, however, also his view that qualitative modelling can be useful in its own right and that quantification may be unwise if it is pushed beyond reasonable limits. It is the purpose of this paper to develop that theme.

Problems of Quantification

When modelling purely ‘hard’ variables such as production, cash flow and so forth, there are few difficulties in quantification. System dynamics, however and rightly, is strategic in orientation and it is often seen as necessary to introduce ‘soft’ variables such as Consumer Satisfaction as an influence on, say, New Order Inflow Rate. There are several ways in which that can be done, typically:

new_order_inflow_rate=basic_inflow*satisfaction_multiplier

in which satisfaction_multiplier is a variable, ranging from, perhaps, 0 to an upper limit which may exceed 1, and having a non-linear relationship with Consumer Satisfaction. This involves two uncertainties, one is that Consumer Satisfaction is indeed a determinant of orders, the second is the shape of the non-linearity. Since the latter is a real function, it has infinitely many possible values so, strictly, the uncertainties are non-denumerably infinite. That will be ignored for the present.

In system dynamics practice the uncertainties are usually justified by the argument that one is concerned with general patterns of behaviour rather than with precise numbers. That might be reasonable in many cases though not, perhaps, in consultancy work where the client may want precise answers, or believe that they are being provided when, in fact, they are not.

The problem becomes more severe when several multipliers are used. For example:

new_order_inflow_rate=basic_inflow*satisfaction_multiplier*quality_multiplier*
price_multiplier*etc.

The number of uncertainties in the combination of relationships is now very large and there is a **VERY** strong additional assumption that the multipliers are, indeed, multiplicative. The effects can be dramatic. If, for example, three multipliers each have the value 0.5 their net effect is a multiplier of 0.125. If the relationship is really one of minimisation, in the sense that the worst factor dominates the others, the effect should be 0.5, or 4 times as much as 0.125. One has seen cases involving as many as 10 multipliers, which one can only describe as being absurd, 0.5^{10} being equal to 0.000977 which is 500 times too small if the process should have been a minimisation. With more than one multiplier, there is, as in this example, a serious risk of double counting (Coyle, 1996, Chapter 10, describes means of avoiding that).

The difficulties are compounded further by the consequence of the money generated from those orders affecting the spending on, say, quality which, in turn affects the quality multiplier. If the uncertainties combine and compound in such ways, it may be hard to believe that the dynamics of the model, **and the policy inferences made from it**, are more 'correct' than can be achieved from a qualitative model.

Having considered some principles, one may now turn to a case example. It is deliberately an old one, so that no one can feel that they are being personally criticised.[1]

Quantitative Uncertainty - A Case Study

The Classical Maya civilisation of Central America flourished during the early centuries of the Christian era. It produced many monumental structures, striking art and a sophisticated calendar. Sometime in the 9th century AD, the civilisation collapsed.[2] This phenomenon has attracted a vast amount of scholarly effort and many explanations have been proposed. However, as Coe (1971) remarks 'Almost the only known fact about the downfall … is that it really happened. All the rest is pure conjecture'.

This dynamic process was modelled by Hosler *et al* (1977). Their purpose was to simulate an explanation of Maya collapse put forward in the archaeological literature and they developed the influence diagram shown, slightly simplified, in Figure 1. The essence of the matter is that the ruling elite controlled the building of monuments and their prestige was bound up in monument construction. Faced with pressure on their prestige, from one of a number of possible causes, their reaction is to build more monuments. Naturally, they do not do this themselves, but dragoon commoners into labour, the rest of the commoners carrying on with agriculture. However, the reduction in food production increases deaths and, perhaps in an attempt to placate the gods, yet more monuments are constructed. Whether the commoners obligingly starved to death while building monuments or whether something else happened is not clear from the archaeological theory.

It is, however, clear from the feedback loops in Figure 1 that this particular theory does provide an explanation for Classic Maya collapse and, in that sense, the qualitative modelling has been useful. It might well be valuable from an archaeological research perspective to test other theories in the same way. The comparison of contending speculations might be illuminating in that domain of scholarship.

Hosler and her co-workers went one step further, as ‘classical’ system dynamics would require, and produced a quantified simulation. In some ways, this is a modelling masterpiece. It has a clear purpose and boundary and captures the Classic Maya collapse in about 40 variables; a marked contrast from some of the vast ‘models’ now being built in system dynamics (the record seems to be about 35000 variables).

To populate their model, Hosler’s team required parameter values (the structural assumptions being determined by the archaeological theory they sought to represent). Apart from such obvious parameters as agricultural productivity and the rate at which monuments could be constructed, which must be somewhat uncertain, three more parameters illuminate the modelling difficulties.

One is the average normal lifetime of a Maya, set at 25 years. This might have some validity if derived from the study of the skeletons of sufficient numbers of commoner burials, though funerary inscriptions would apply only to the elite.

The second is the period of 1000 years assigned as the lifetime of a monument. Certainly, many of the Meso-American monuments are now well over 1000 years old but whether that has anything to do with what might be called the liturgical life of a sacred monument or the political life of a palace is another matter. Tens of thousands of European churches are many hundreds of years old but they have been drastically modified several times as tastes in worship altered. There are scores of cases in which royal palaces which had lasted for centuries were simply demolished to make way for something more modern or fashionable. This parameter clearly illustrates the dangers of jumping to conclusions.

The third is the period of 75 years (3 assumed generations) chosen as the ‘elite’s averaging time for monument construction’. The assumptions here are colossal: that the elite behaved as proportional controllers; that they had some ‘target’ for monuments to reflect their prestige; and there is, of course, not a shred of evidence for 75 years.

Finally, the model has four non-linearities (one of which is used twice). One relates lifetime to food supply; speculative at best. Another, called the placation multiplier, placates the gods by increasing desired monuments as food supply falls. The quantitative evidence for this is difficult to discern.

The dynamics of the model are shown in Figure 2. Population and monuments grow steadily for 300 years from 450AD, though average lifespan (a variable Hosler does not plot) declines slowly.[3] Around 700 AD there is a surge in monument construction, about 150 years before the collapse took place in the 9th century. In 800 AD, there are some 3 million Maya, with an average age of 13 years, many of whom are actively building monuments. After 800 AD, the collapse accelerates and some 3 million Maya die in the space of 10 years. After about 820 AD the number of Maya is effectively zero (in the order of 10^{-36}), their average age being 8 months.

It is hard to see this as consistent in timing or behaviour with the actual Maya collapse. Indeed, Sharer (1977, the same volume as contains Hosler's paper) remarks 'Interruption of [production of luxuries for the] elite … occurred over the 9th century. There is little evidence for the sudden demise of non-elite classes'. Coe (1971) cites 889 AD as the date for the last few stelae and states that the Maya had vanished, as opposed to died, by the 10th century, or 100 years later than the simulated collapse. Hosler's simulated extinction of the Maya does not fit with the millions of their descendants inhabiting Guatemala to the present day, maintaining much of their culture, language and distinctive appearance (Coe, 1971). None of this bears much resemblance to the model's behaviour.

The purpose here is not to criticise a particular model simply for the sake of it. The point was, in fact, as in much else in system dynamics, best made by Forrester (1961, page 63) who states 'In the **proper** formulation of a system dynamics model the model variables should correspond to those in the system being represented. … Sufficiently close correspondence of model and real-system variables is obtained [by carefully ensuring that] the decision functions represent the concepts, social pressures and sources of information that control the actual decisions'. In short if, as in this case, that criterion cannot be met, one should, perhaps, restrict the analysis to the qualitative level which, as has been argued, was useful in this instance.

Quantification Difficulties - Another Instance

In one of the rare publications by consultants Doman *et al* (1995) describe an analysis of life insurance companies. Such companies have 'a profusion of "soft" variables such as salesforce motivation'. They discuss at some length the causal maps involving the soft variables and derived from discussions with their own, and industry, experts and comment 'We gained a number of key insights from the causal mapping process'.

Later, they mention their 'preliminary (*and possibly controversial*) simulations' (emphasis added). It is not clear why the simulations were controversial but it must have been difficult to overcome the compounding uncertainty of the 'profusion of soft variables' so as to add plausible value to what had been learned from the qualitative analysis.

Insights From A Qualitative Model

Figure 3 is an influence diagram of the treatment of psychogeriatric patients, its origin being discussions with a friend who is a consultant in psychogeriatric medicine (Coyle, 1984B; referred to by Dangerfield, 1999; commented on by Richardson, 1999).[4]

The patients in question suffer from relatively mild psychiatric disorders of age. Normally, they can manage at home under the care of general practitioners (GPs) but require occasional short periods of hospital treatment, often little more than proper diet, hygiene and establishing a regime of medication. However, if the psychogeriatric beds are fully utilised, patients cannot be admitted until there is space. The solid lines show a flow module of admission to, and discharge from, hospital after a delay which reflects a prevailing medical opinion rather than a definable recovery from a condition.

After a further delay in the community, the condition of some patients will again deteriorate to the point where they are put on the waiting list for hospital admission. Not all patients recycle in this way. Some die, some recover, some deteriorate to the point of requiring institutional treatment and some simply get fed up with going back into hospital. New cases arise as people age. These physical flows are easy to recognise and are shown by solid lines from which stocks and flows are clearly identifiable (*pace* Richardson, 1999).

The behavioural relationships are less clear cut and sometimes involve +/- signs to show that the effect is sometimes positive and sometimes negative. Thus, up to a point, increase in D_M will increase D_R, the delay before patients recycle, the hospital treatment having had the desired effect. Beyond that point, further increase in D_M may make D_R *decrease* as patients become practically institutionalised. Other +/- or -/+ signs are similarly interpreted. The shapes of the curves and the points of inflection are scarcely determinable and, as there are four of them, quantification of this model might, in Nuthmann's words, be plausible nonsense or even controversially implausible.

Nonetheless, there is a real human and management problem to solve and Figure 4 indicates lines of thought which may help, as shown by the finely dashed lines. The psychogeriatric medical staff have little knowledge of the real numbers on the waiting list as there are actually two such lists. One is the formal list at the hospital, the other is GPs' recognition that Mrs X (most of the patients are women) is getting to the point where she will soon need treatment. A partial solution is to establish a medical committee which will meet, say, quarterly, to review waiting lists and recycling fractions and re-assess the treatment duration accordingly. A related idea is to use capacity utilisation not only as a regulator of admissions but also as a discharge control, though a careful balance would be required.

The role of the influence diagram has been two-fold. Firstly, it has suggested this admittedly empirical approach to the problem. Secondly, and probably far more importantly, this problem has at least four stakeholders: consultants, GPs, patients and their relatives, and hospital administrators and it shows them that they are in a **dynamic** system. The diagram should, in fact be part of the agenda for the proposed meeting.

Heath care is beset with similar problems of uncertainty, largely arising from the need to combine dealing with human beings and their frailties in the context of medical opinion on appropriate regimes of treatment. For example, Wolstenholme (1993) has using qualitative modelling to study the unintended effects of policy on the delivery of community care.

The research question for system dynamics is to identify a way of measuring the value added by attempts to quantify problems such as this. For the research to be successful, there would, however, have to be some kind of sensible metric and an avoidance of dogma about what is, and is not, system dynamics.

Another Qualitative Model

Figure 5 is a diagram of the Year 2000 problems of a major utility company. The issue is that, if Y2K strikes this company, the effects on British national life could be little short of catastrophic. What the diagram makes clear is that there are three aspects to the solution of Y2K problems. One is the vast amount of money being spent on the solution of the Y2K problems themselves; a typical project management problem with the customary associated rework as new problems emerge. The second is the diversion of effort from day-to-day system maintenance with consequent effects on current operations. Finally, there is a large, and very costly, effort to develop contingency plans to operate the system even if Y2K causes problems.

Above all, though, the diagram shows that UTILITY's Y2K problem has wide strategic ramifications. The project planning part of the problem is probably amenable to quantification but the wider issues for UTILITY's senior management are less so. Those are shown on the left hand side of the diagram and relate to the financial and regulatory pressures implicit in any perceived likelihood of failure to solve Y2K. There are numerous stakeholders: the company itself, the customers and *their* customers (UTILITY provides a service which is used in industry), the regulatory authority, the government and, finally, the stock market.

What the qualitative analysis does suggest is that, unless UTILITY can be sure to have solved all its Y2K problems, it should emphasise those aspects of Y2K which support the ability to implement the contingency plans, otherwise the vast sums spent on contingency planning will be wasted. If, of course, they can solve Y2K, the contingency planning will be a waste of money in any case.

Any attempt to answer questions about UTILITY's Y2K strategy by modelling this would probably be nugatory, not only because of the uncertainties already discussed, but also because some of these stakeholders do not share common objectives and may, indeed, be to some degree in direct conflict. In particular, quantification of the ability to implement plans is fraught with uncertainty. A scale from 0 to 1 is superficially tempting but what would 0.5 mean? If it means that half the plans can be implemented, which half is it? If it means that all the plans can be half implemented then what is half a plan? It is into that kind of trap that relentless emphasis on quantification can lead.

The trap is further illustrated by claims by Brown (1988) that Vensim 'allowed [us] to model issues such as morale, shock, [and] surprise .. that are generally left out of combat models because they are too difficult to represent'. In fact, these factors have defied analysis despite decades of patient and rigorous effort. To claim that a system dynamics package has solved them is absurd. This is not a criticism of Vensim; it is simply to say that powerful software should not be preposterously misused.

The research issue for the wise balance between quantitative and qualitative models thus requires not only a metric for the added value of quantification, it also calls for a trade-off against the uncertainties **inevitably** inherent in quantification and, in addition, guidelines against this kind of seductive, plausible, nonsense.

A Portrait Of A Human Disaster

To draw these themes together and lead to a better-developed agenda for research it is worth briefly revisiting the Angolan model (Coyle, 1998; Coyle, Bate and Hamid, 1999).

The full diagram is in Figure 6 and, as described elsewhere, includes parties who are in violent, indeed murderous, conflict involving unimaginable human suffering. Given the number of uncertainties and their immensity, not to mention the plethora of competing parties and interests, it is hard to avoid concluding that quantification of it would not be plausible nonsense, it would be verging on science fiction.

Any model, even one written with words and arrows, is intended to be a simplification of reality, a tool for thinking with and, ideally, intended to answer reasonably well-designed questions, in the felicitous phrase of Richardson and Pugh (1981), the notion of *designing* the questions being vital The reality of Angola is so complex that Figure 6 is starting to fail the simplification test, simplified though it unquestionably is. Figure 7 simplifies further and now puts the emphasis on the various policy levers α, β, γ and δ. Whether even this simpler version *could* reliably be quantified is an open question. Whether it would be worth doing is another matter as so much of Figure 6 has been lost in going to Figure 7.

This raises a research question first, as ever, alluded to by Forrester, of what is a suitable model boundary. In the context of the wise balance between quantified and qualitative models, that now means the boundary beyond which quantification should not go.

The Uses of Influence Diagrams

Before tentatively proposing a research agenda it may be useful to summarise the uses of influence diagrams.

> They put a very complex problem, which might require many pages of narrative explanation, onto one piece of paper.
>
> As in the psychogeriatric hospital management problem that may be a helpful reminder during discussions; effectively a form of agenda which, unlike the normal serial agenda, shows the relationships between the items being discussed. The simpler version of the Angolan model shows the interaction between the four mechanisms of external intervention, α, β, γ and δ.

Identifying feedback loops from the diagram may help to explain behaviour or to generate insights, as was the case with the Maya collapse and the management of insurance companies.

Study of the diagram may identify the wider contexts of a modelling task. That was the case in UILIITY where modelling of the Y2K remedial work could be placed into a more strategic environment. The modelling in question does not have to be system dynamics; a good diagram might show the feedback context in which, say, production planning by linear programming resides.

Finally, of course, and where appropriate, a correctly drawn influence diagram is the basis for a quantified model and is easily transformed into equations, whether it be by a text-editor or an iconic package.

Only the last of these uses bears on classical system dynamics. The utility of the others can scarcely be gainsaid and qualitative modelling only fails to be system dynamics under the most dogmatic definition of the discipline.

A Research Agenda for Quantification, O*r A Prudent Approach To Difficult Models*

Having discussed some of the potential problems of quantification, it is now necessary to consider more fully the research questions which have been raised.

The first stage in system dynamics modelling is description of the system by some type of diagram. That is true whether one traces out many influences, as in the insurance company problem or in Angola, or whether one starts by identifying some particular levels and then maps the feedback connections between them. The second stage is very likely to be study of the diagram, if only during the process of checking its suitability for the problem. It is, therefore, only at the third stage that quantification arises so the research task boils down, as far as can now be seen, to two questions.

The first is general and may apply to all models: 'how much value does quantified modelling add to qualitative analysis?'. This is likely to involve some kind of measure of the added benefit from quantification in relation to the cost of the work. Since the metric will probably relate to understanding and confidence in recommendations it will be necessary to have some sort of definition of 'understanding' to take us away from the glib repetition of 'insight'. In particular it seems likely that it will be necessary to be clear about where the insight lies. Is it that the modeller found out something which the 'client', whether that is a fee-paying sponsor or a research community, already knew perfectly well?

The second relates to when one dare take the risk of the kind of manifest uncertainties which have been described earlier. These are the difficult models mentioned in the sub-heading. A number of steps suggest themselves.

- Identifying more precisely the types of models and the domains of investigation in which difficult models are likely to arise.

- Considering how variables such as Disaffection in Angola or Market Perception of the Severity of the Y2K problem in UTILITY might acceptably be measured.

- Attempting to establish principles for deducing the shape and values of non-linearities.

- Developing a technique for handling multiple non-linear effects on a given variable, so as to avoid double-counting.

- Defining a procedure for establishing the forms of relationships involving multipliers, that is whether the factors are multiplicative, additive, minimising or whatever.

- Some of the difficult problems mentioned in this paper involve parties which have conflicting objectives, are in discord or are even in violent conflict. It is probably a whole research programme in itself to discover how to model problems of that degree of difficulty. The RAND corporation has previously done modelling on the politics of violent conflict, though not with system dynamics, so a study of that could be useful;

- Find some formal measure of the extent to which uncertainties in formulating equations or obtaining data affect the reliability of the model (measuring variations from a reference mode by sensitivity testing guided by statistical design ?);

Such a programme will require a range of disciplines and will draw on all the talent of the system dynamics community. Perhaps the Society should sponsor it, using some of its wisely accumulated funds to sponsor work under the aegis of a truly international panel of acknowledged and experienced people in the field. Maybe they should not themselves do the work but guide the hands and minds of some of our brightest and most innovative students.

One suspects that the experience of challenging some received wisdom in the field might be either bruising or stimulating and, as with all research, the results might be negative, but they might indicate the safe limits of quantification. It is worth rephrasing Nuthmann's question to be 'how can we guard against models which risk becoming plausible nonsense?' .

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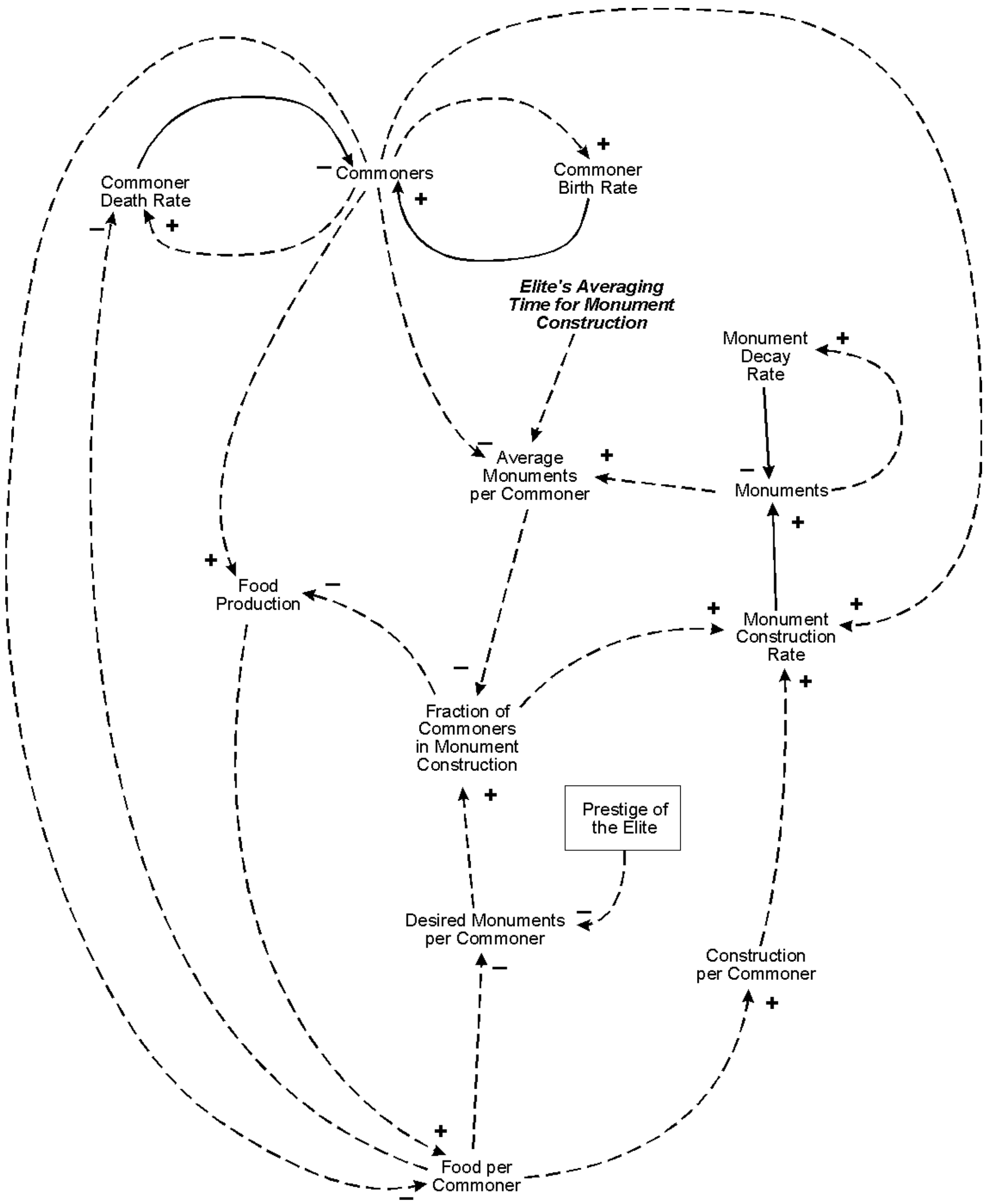

Figure 1 The Decline of the Maya

(after Hosler et al)

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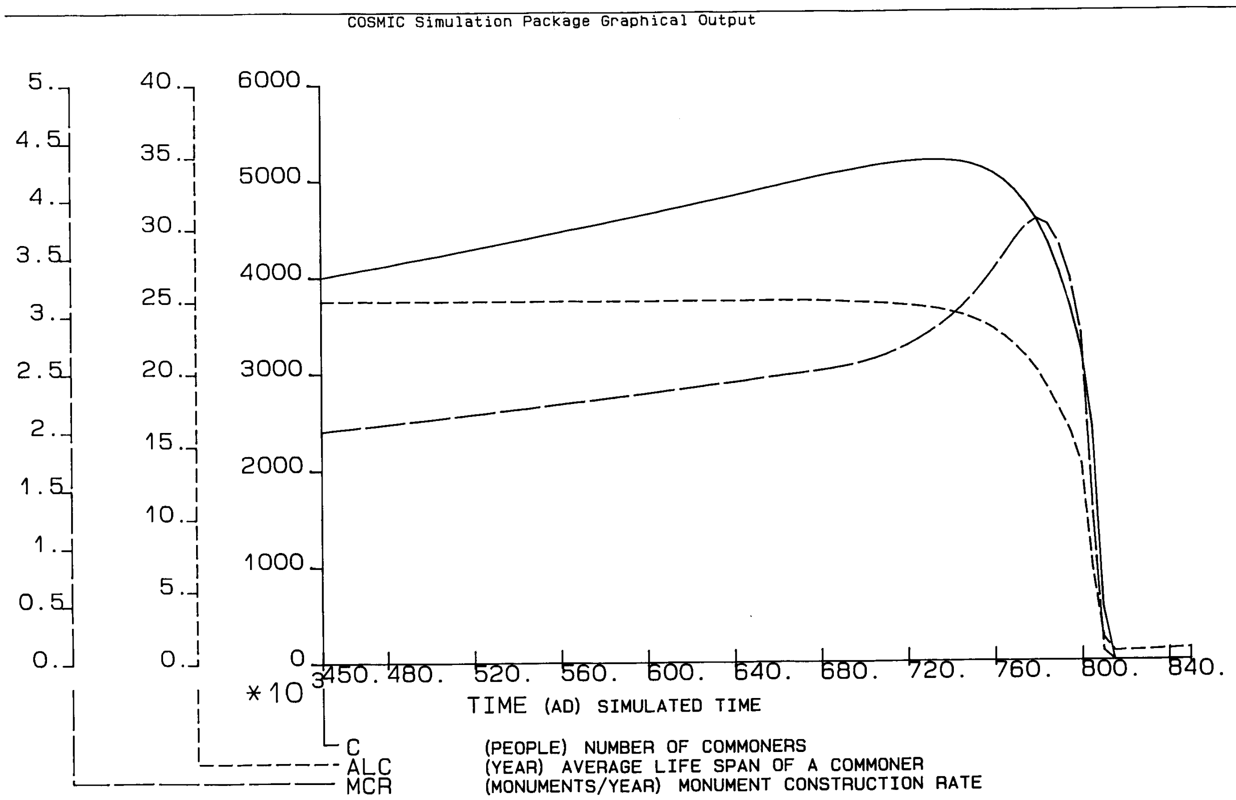

THE COLLAPSE OF THE MAYA
FIGURE 2 MAYA3 MODEL FROM HOSLER ET AL

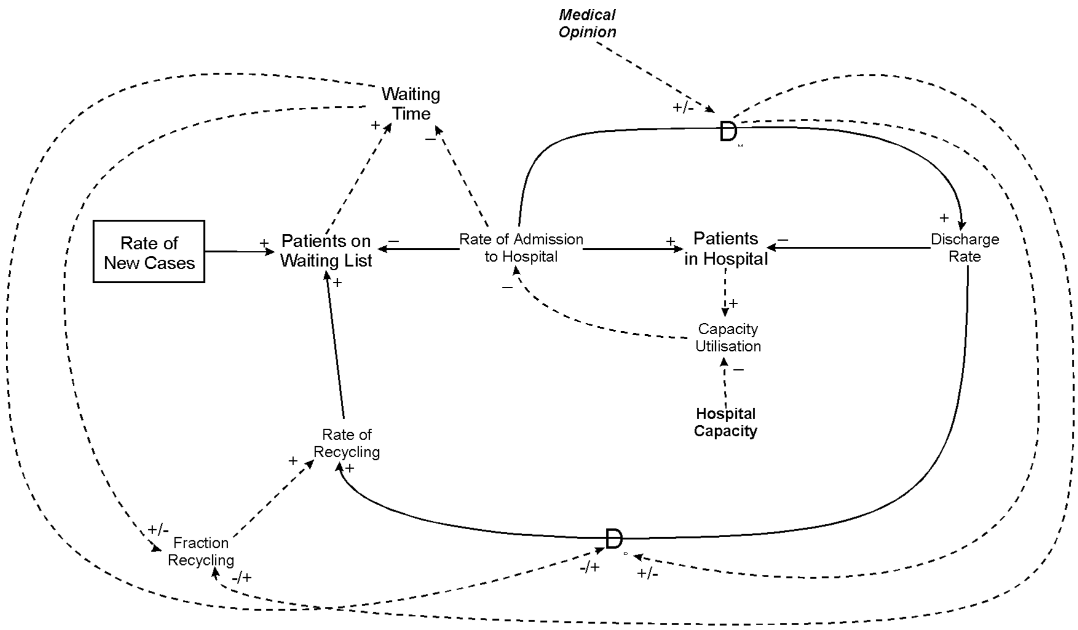

Figure 3 The Treatment of Psychogeriatric Patients

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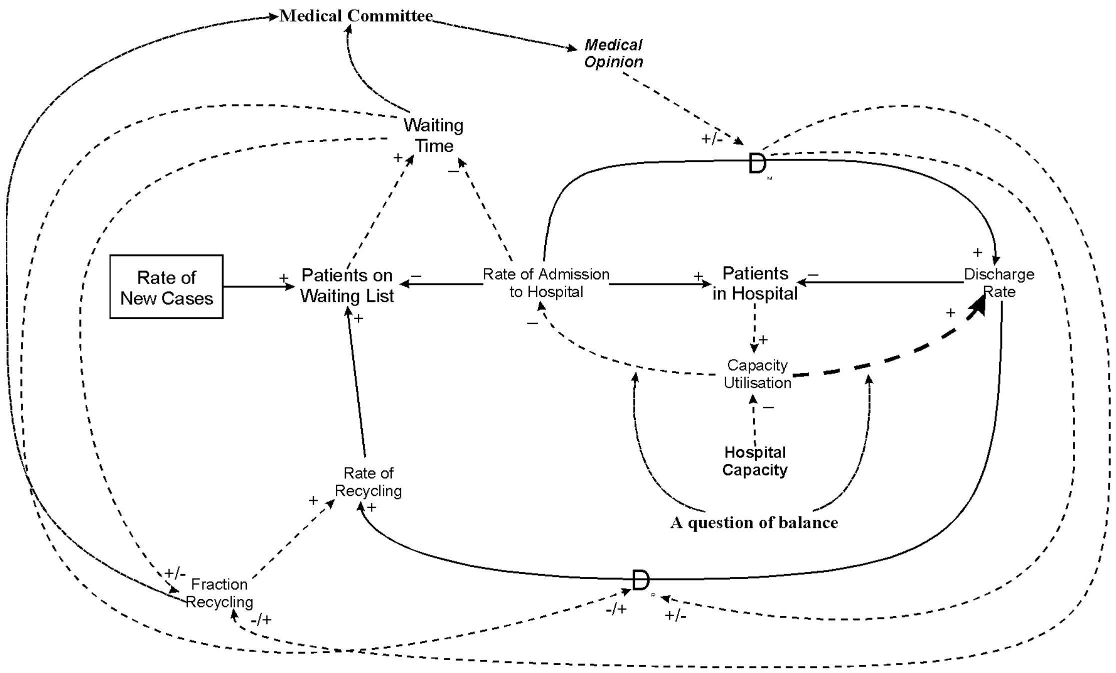

Figure 4 Policy 'Insights' for Psychogeriatric Patients

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Copyright R G Coyle, 1999

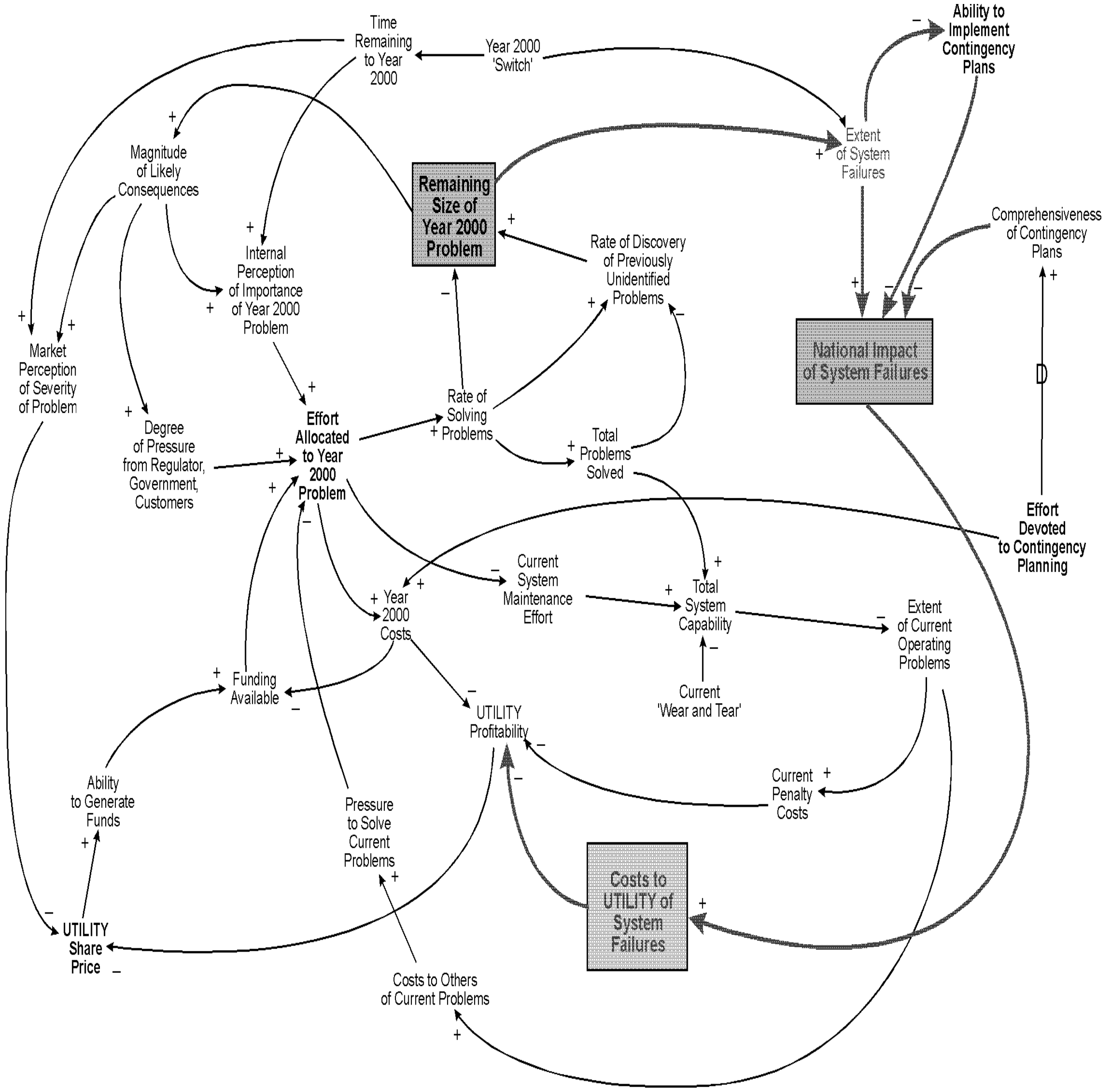

Figure 5 Overall Strategic View of UTILITY's Year 2000 Problem

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Copyright, R G Coyle, 1999

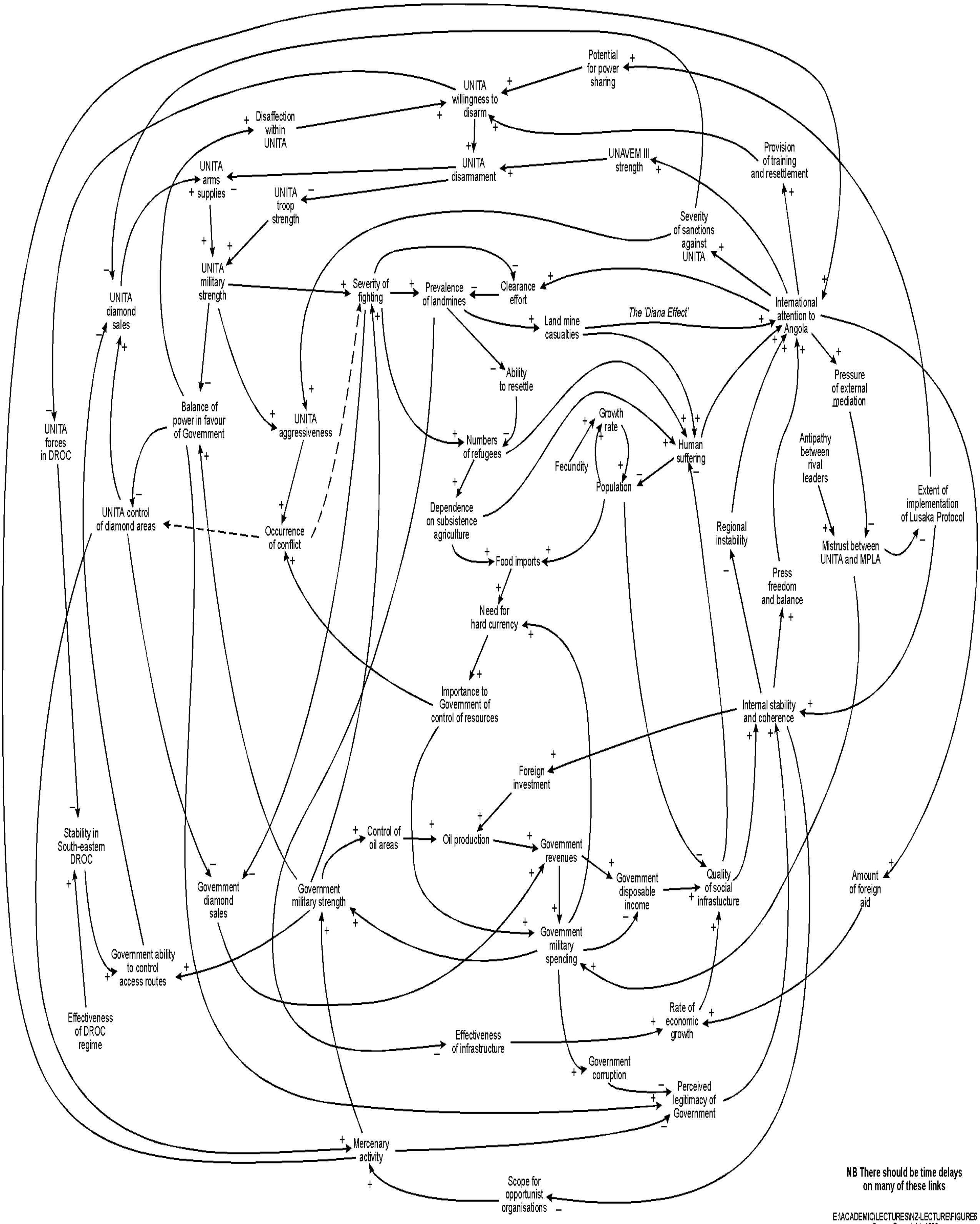

Figure 6 An Influence Diagram for Angola

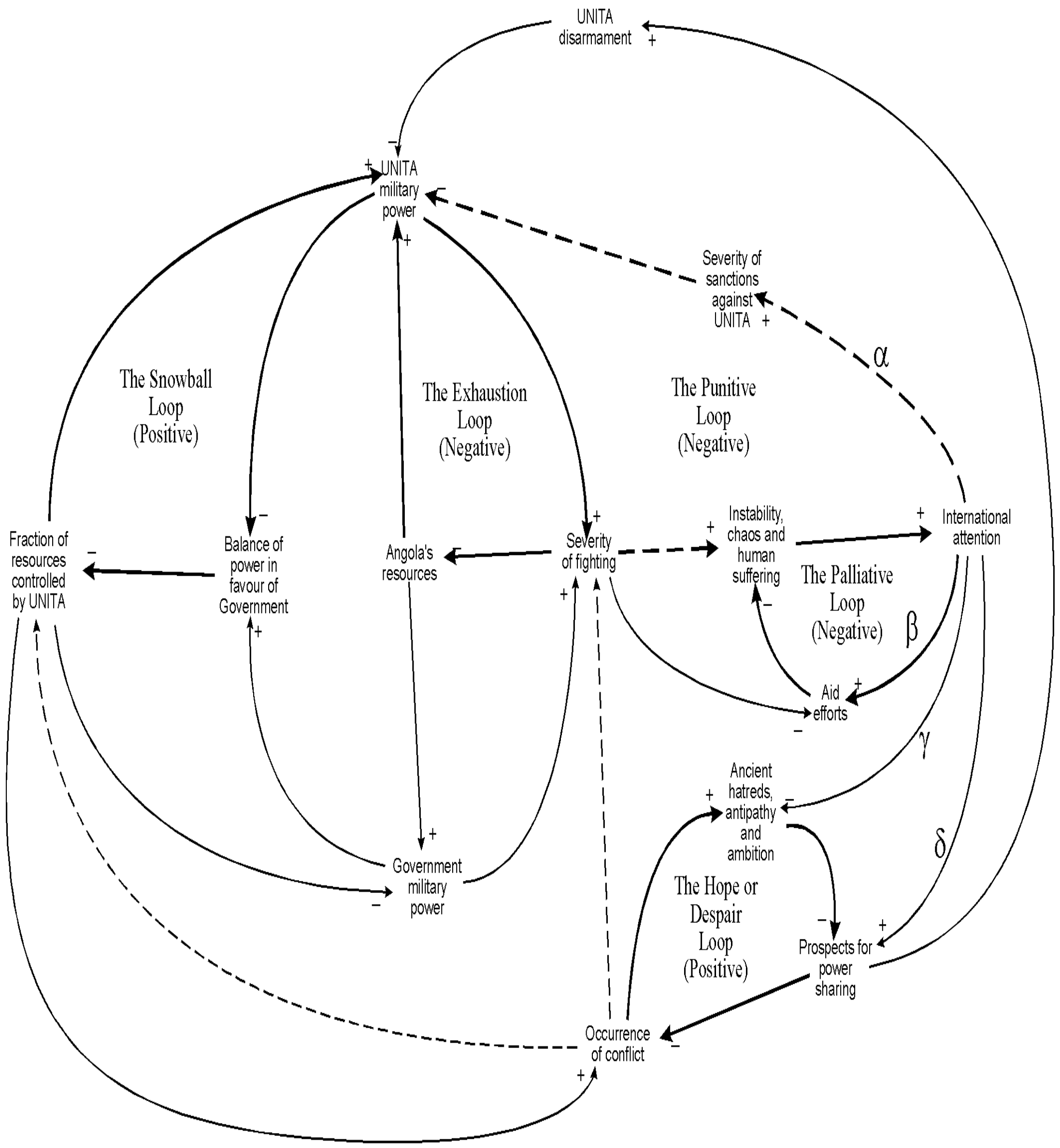

Figure 7 A High Level View of Angola

NB There should be time delays on many of these links

E:\ACADEMIC\LECTURES\NZ-LECTURE\FIGURE7

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[1] The modeller in question, a truly delightful person, died tragically young. One apologises to his shade.

[2] The Maya had, of course, their own sophisticated calendrial system but dates in the Christian era are used here for simplicity.

[3] There are no DYNAMO print statements in the published version of the model.

[4] In the UK, a consultant is the most senior grade of hospital doctor.