John Barton.

Dear John, 6 Jan 1995

With the festive season over and the third test match decided I can turn my attention to 'systems thinking'.

In the discussions we seemed able to find common ground but I am afraid that in Wednesday's seminars I found it very difficult to come to grips with the lack of understanding and concern with systems thinking. Afterwards I had to ask myself whether I have just gotten tied up in theoretical esoterics - "did it make any difference in practice to make the theoretical distinctions?"

It is probably good to get exposed like that from time to time but I find it more fruitful to be exposed, with peers, to the demands of unsolved practical problems.

I suggest that the latter exposure is the key to the educational program that you plan. That was the key to the rise and fall of the post-graduate courses at Lancaster and University of Sussex. That was what led Russ Ackoff from Case-Western to Uni of Pennsylvania.

You must expect a similar challenge, particularly if you are pulled in closer to the older Monash establishment. The challenge typically comes on two fronts. First, and probably basic, is that universities get very nervous when one of their departments enters into collaborative relations with an outside body. The universities expect that they will be held publicly responsible for any resulting corruption and mis-direction. Second, they want each student to be judged on their own work. They have little or no positive experience of project teams being involved in the judgment process.

If you cannot get agreement amongst yourselves for project work on problems in the real world then you might be forced back on the MIT solution (as spelt out by Sterman). Personally I would drop the proposal for Monash if agreement is not reached, and try with someone else eg Uni of Southern Cross. Follow the Sterman line and I think you have yet another bullshit program, and we have enough of those.

I would be inclined not to ask for formal, institutional ratification until you have put some scores on the board. That is, are able to show that the traditional fears are groundless with your staff, your students and your sort of collaborative relations.

You might have wondered why I sent the papers dealing with collaborative relations and responsibility. They were to help you with the first problem with universities, and the morale of your own staff and students. We also have had experience with the second problem.

In talking about systems thinking I tend to think of its historical development because of my personal involvement for so long. This is often unhelpful to beginners because the matters that historically caused the most strife are not necessarily the most significant developments. Thus the argument over purposeful vs goal-directed systems would not have become heated if we had then realized that a deeper issue was involved. The deeper issue had been staring us in the face since Angyal's 1941 book and pushed under our very noses by Sommerhoff in 1950. The deeper issue was that the critical distinction between classical scientific analysis and systems synthesis was not between a part by part view of the system versus a holistic, gestalt view. The relevant distinction was between system-in-itself and the system-in-its-environment.

I do not wonder that we all dodged the issue for so long. It meant that a scientific statement about a system, a micro-world, required a correlated scientific statement about the environment of that

system, a macro-world. Universities and career paths insulated the micro from the macro worlds (eg psychology and sociology).

We were all well prepared for the trap that Bertalanffy had unwittingly dug for us with his 1950 paper in Science". Then, as we were digging ourselves out of that we rushed to throw ourselves into Prigogine's trap (we did not realize that it was Bertalanffy's old trap disguised with new labels).

It is ironic that the full set of equations for a system-in-its-environment had been spelt out by Lars Onsager, in his PhD thesis on thermodynamics of open systems for Trondheim Technical University, 1932. The uni rejected his thesis, it was published in 'Physical Review' and I think they changed their minds about the degree. I was not aware of his theoretical contribution until about ten years after I had re-discovered the set of equations in 1962. Getting to a genuine systems science has been a bit like getting through the sound barrier.

Alfonso Montuori.

Dear Alfonso, 12 Jan 1995

Thanks for your kind remarks about my chapter on creativity. Maturama suggested that I extend the analysis to some others, perhaps Jim Watson. I had thought of Watson and done some reading. The suggestion made up my mind and I have now effectively finished the addendum. It is almost certainly too late for the publisher but that does not matter. I will send you, Ron and Maturama copies for your amusement.

I had decided some years ago to devote my spare time to sorting out systems theory. Your proposal fully coincides with my personal decision, and adds the discipline and time pressure that I probably require, as a 'retired gentleman-scholar'. (Actually I am not much of any of those three things).

Your aim of keeping the volumes in the $20-25 range appeals to me. The key issues that confront systems theory do not require 300 + pages to spell out. Whether supplementary readings are required I would sooner leave for later consideration.

So, the answer is yes for a new manuscript. Fred.

NOTES.

- system principle vs Hall and Hagen

- system-in-its-environment vs system-in-itself.

- systems learning !
-systems memory - files.

- systems levels : goal seeking, purposeful & ideal.
consciousness.

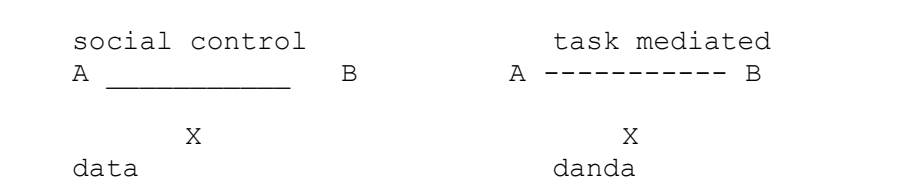
DRAFT OUTLINE.

Introduction (C.P.Snow, Guillam)
PART 1 General System Characteristics.
1. Closed & open systems (reductionism & holism; analysis & synthesis; data vs danda.
2. System and its environment.
3. Logic of systems (abstract vs concrete universal; psycho-logics)
PART 2 Social systems.
4. Relation of social & psychological systems (Asch, Mead

& Marx; Intro of ST Vol 1 ; foremen & managers)

5. Management & self-management of social systems (the two principles)

6. Communication in social systems (Choice of Futures)



Mechanism Organicism Contextualism Formism (Aristotle / Acquinas)

DRAFT INTRODUCTION.

The behavior of 'purposeful systems in their environments' reaches its peak in the scientific enterprise, not in being 'a community of believers'. Paradoxically, that is better expressed in the writings of the leading Protestant theologists, Jurgen Moltmann, than the writings and TV interviews of leading physicists such as Paul Davies & Murray Gell-Mann. It is the nature of the scientific enterprise and the paradox of its current presentation (which has many precedents) form the context within which we will consider systems thinking.

Within the scientific enterprise the task of physics has always been that of identifying the sameness amongst physical motions. It has never been the task of physics to identify and explain what is newly emergent. It is for the other branches of science to account for differences and emergent qualities. That is, the task of physics committed it to the scientific strategy of reductionism. This reductionist strategy has been very successful for physics and very helpful to chemistry and other branches of the scientific enterprise where it has proven that many of the problems that they have faced are essentially previously unsuspected motions of physical particles in physical fields of force. The reductionist strategy has been adopted by many leading practitioners in chemistry, biology etc with some success. Thus biology was much advantaged by the discovery of the nucleic acid composition of genes and physiologists breathed a sigh of relief when it was established that the troublesome human stomach ulcers was caused by bacterial infection rather than psycho-social stress. The sciences other than physics also have the task of reducing to lawful sameness the differences they are critically charged with studying, e.g. the differences associated with the character of different materials, or states of material, with life, with mind & with society. This part of their task is sufficient in itself to lay a stress on reductionism. Nevertheless, a whole-hearted addiction to the reductionist strategy seems to have contributed significantly to the failure of each of these branches of the scientific enterprise to elucidate the critical difference that is their raison d'etre. The effects have been far more reaching in everyday practice. The tactics that have been successful in physics have been given the highest status in the other sciences eg experimentation and the use of the mathematics of the physicists.

Experimentation has been narrowly defined as methods of keeping extraneous influences at a minimum. This method of isolation makes a lot of sense in classical physics and even in chemistry, providing relatively pure samples of material have first been prepared. The method cannot

stand alone when the subject matter of a branch of science is precisely the inter-action with an environment. The method can be, in fact, quite misleading when the subject matter are other purposeful human beings. Some of the high standing of physics was no doubt due to its historical priority.

The emergence of the scientific enterprise and the dominance of the reductionist strategy cannot be put down just to the requirements of physics and its historical priority. Pursuit of science necessarily entails two assumptions a) that a world out there exists and is changing in its own right, b) that the world out there is knowable. Thus, like King Canute, and unlike his advisors, scientist do not believe that ocean tides can be changed by an act of will. Unlike Descartes, scientists do not grant that something could exist and be unknowable. If something is in its nature unknowable then such a thing does not exist, and hence could be of no concern to humans in their scientific enterprise. Individual scientists, or groups of them, may choose to ignore these assumptions but provided they do what is expected of them as scientists little harm is done to science. If the scientific enterprise turns its back on these assumptions than it becomes no more than a source of authority for myths that competes with churches and other authoritative sources of myths. To put it crudely and simply there is no point to scientific endeavours if they do not tell us 'what is happening when we are not looking'.

Two general constraints exist for the scientific enterprise at all times and under all realistic conditions, ie conditions that meet the two criteria of independent existence and knowability. These constraints are that the scientific enterprise is an open-ended enterprise and the scientific enterprise is a social enterprise.

That the scientific enterprise is open-ended means that none of its principles or facts are indubitable and it is not able to rule with absolute authority on any of the matters with which it is concerned. This is a self-contradictory statement in that it seems to be asserting that there are absolutely no absolute truths. Scientific studies, in any field, are only concerned with material (concrete) universals and therefore in no position to argue or lay the law down on any abstract universals or Absolutes. What is being asserted is that the pursuit of any Absolute truth is a foolishly improbable aim when there has never been any evidences that production of newness is declining toward, or converging on some sort of equilibrium state. Actually more than that is asserted. The scientific enterprise has always been involved in searching for new ways to better answer existing questions and to get to grips with newly emerging questions. When people claim to have found an Absolute truth scientists are going to ask 'How?' Just as scientists question claims to access 'normal' truths by unusual or abnormal means. However, the latter claims can be expected to be verified, or falsified. No way has been found to verify claims to Absolute truth and no-one has conceived as to how such verification might be possible.

This has not prevented individual scientists, or even collections of scientists, from publicly claiming that science has established some absolute truths or that science is on the verge of "The Theory about Everything', Sometimes this is the understandable hubris of scientists who are elated by the advances recently made in their speciality, and lack historical perspective and are ignorant of what is happening in other fields. More frequently this seems to be the trap that mathematical theorists fall into.

Systems thinking was beginning to emerge in the middle of the 19th century. It was emerging as science was successfully extending into the

fields of organic chemistry, botany, biology and physiology. By the end of that century science was extending itself into psychology and sociology. What was distinctive about this systems thinking was the claim that the analytical, quantitative methods of science were a misguided attempt to reduce the explanation of phenomena to the behavior of their simpler constituent parts.

A great deal of this early systems thinking was, naturally, concerned with defending the traditional, religious, philosophical and cultural explanations from the encroachments of science. All sorts of vitalistic forces were postulated to account for these higher order phenomena, eg for purposefulness and spirituality. Just giving these phenomena a distinctive name was not the same as giving a scientific account of them. What these vitalistic forces had in common was that they invoked self-determining, unknowable entities that just had to have the miraculous higher order properties. It was a regression to Aristotelean entelechies. However, there were some dedicated scientist who genuinely believed that in these new fields their scientific methods and concepts had to be adapted to the higher levels of organization they were encountering. Amongst these there was the widely shared belief that only the unit of analysis had to be changed. With a unit of analysis appropriate to the higher level of organisation they could proceed with the basic scientific methods of isolation, controlled experiment and formal analysis. Formal analysis was taken as quantitative measurement and mathematical theory. A few, a very few, were suggesting that science was unwittingly carrying forward some assumptions taken from the religious, philosophical and cultural traditions it was displacing. These assumptions, they suggested, might have more to do with the difficulties experienced with coping with higher levels of organization than any matters of methods (As we shall discuss later, these assumptions would come to make science its own worst enemy).

The most bothersome assumption was that reality is simply the interaction of propertied things. That had for aeons been a simple and very convenient way to see the world; and is still so for most practical, day to day, affairs. In the development of the physical, chemical, geological and botanical sciences we saw the spirits and ghosts driven out of the hills, streams and bushes. Those sciences could grow a great deal before they felt the pinch of the assumption about things. In biology, psychology and related scientific disciplines the pinch was almost immediately felt. In each of these disciplines there were sub-fields that were not much bothered and they could steam ahead by closely aping the methods of the 'physical sciences'. Those sub-fields could usually gain entry into the old universities. However, the very core of these disciplines concerned higher order phenomena.

The relative balance of the forces contributing to system thinking had changed greatly by the middle of the 20th century. This was not primarily due to the growth and extension of science.

By the sixties scientists thought that they had found the answer to coping with higher orders of organization.

Barbara Bernstein,
Hampton Press.

Dear Ms Bernstein, 7 March 1995.

I am grateful to Alfonso Montuori for creating this link to Hampton Press. I am not too sure whether my time-table and purposes fit

your plans for the series, "Studies in Systems Theory and the Sciences of Complexity".

First, with regards to the time-table. This will probably be my last book-length go at systems thinking (age!). So, I am concerned to make my main points and to get them as right as possible. The earliest target I can live with is December, this year.

Second, with regards to purposes. I have definite ideas about what kinds of systems thinking are relevant to the biological and social sciences; and those ideas are not everyone's cup of tea.

The strand of 'systems theory' that comes from the design of complex engineering systems and operations research is peripheral to my concerns. That strand is only about the methodology of mathematically modelling input-output systems. Sometimes we have found those methods useful for some well-defined part-problems but they do not even confront the major problems we have faced as multi-disciplinary social scientists.

My lasting interest has been in that strand of systems thinking that seeks to understand goal-seeking and purposeful behaviour as characteristics of our real world, i.e. I am concerned with the ontological issues, not just epistemological conveniences. I am unimpressed by theories that seek to explain those behaviours away as illusions or epi-phenomena created by random dissipative forces or the complex, chaotic or catastrophic arrangement of directionless elements. Those theories, like input-output models, may help with some of our part-problems but do not confront our central problem. Within the bio-social strand General Systems Theory (GST) was for many years a favourite. It sought, in Aristotelean fashion, for those properties that systems had in common across the different levels of organization. They were seeking for the properties of systems-in-themselves and disappeared in a cloud of irrelevant abstractions.

In the Readings on Systems Thinking (Penguin, London, 1969 and revised two volume edition 1981) I had to provide the classic background papers but, as stated in the Introductions, my concern was with systems thinking that revealed 'the general in the particular' and that could be done only by studying the system-in-its-environment. Only by revealing the interdependencies between a system and its environment could we hope to establish what 'business' the system was in.

I can be more specific about the manuscript I have been working on. Through 1982-84 I was Busch Professor for Social Systems Science in Russell Ackoff's department at the Wharton School, Uni. of Pennsylvania. I took that post, although I had settled back in Australia in 1969, because we thought we would prepare a second edition of our 1972 book On Purposeful Systems (Aldine-Atherton, Chicago). That was not to be. Ackoff had come to regard the book (primarily his) as a Bible that only needed more formalisation. I thought it was a flawed book that needed to be replaced by "Purposeful Systems in their Environment". I resigned, came home and have continued with this task in my spare time. It has of late become my major task.

The best example of where my thinking is at is the chapter I was invited to provide for Kenyon De Greene's "A Systems Approach to Policymaking" (1993. Kluwer, Boston). It is not, however, an example of the level of treatment I intend (De Greene's book was something like a pissing contest between a lot of the old heavies so I was happy to rub their noses in formalization). I think that what I have to say can be spelt out in plain English, with a minimum of formalization. This is because I will not be aiming at those in the 'complex engineering stream' nor at those attracted to the formalism of GST. My target is those who still hold some hope of understanding goal-directiveness and purposefulness in an objective and non-reductive way.

That was an enthusiastic market in the sixties and early seventies. It died, as I discovered with the second edition of my Readings, as systems thinking was seen as another sophisticated tool of the McNamarra's and their like. I think that the market might now be ready to go again - but I am only guessing. (For myself I do not care much about the fickleness of the market. I am used to waiting decades. The attractiveness of your proposal is simply that it might put pressure on me to pull the threads of the manuscript together while I am still interested. I do not really mind if the manuscript then lies around for a few years).

The hubris of the system theorists and the Acquarians seem both to have passed. The 'central dogma' of molecular biology is looking quite sick and silly. The 'Big Bang' theory of the cosmologists is looking increasingly theological. I think that many are concerned with understanding the world that they comprehend. The flood of people to the Fielding Institute, to the old Ackoff center and the Bow Group (Calgary) suggests that the baby-boomers are coming back to the matters that once consumed them. A perusal of the textbooks suggests that the disciplines within the universities are dropping further behind and are even less able to meet the questioning that still attracts students to them. Those students need some intellectual help. The biggest potential market arises from the demands for more participative forms of management and of community organization.

In the markets I have mentioned many have given up any hope they might have had in science; many clutch, hopefully, at the latest 'breakthroughs' by Hawkins on Time, by Chaos Theory or by the Santa Fe Institute for Complexity. Those developments, I suggest, are no guide to the future of systems thinking. Maybe a market for systems thinking is re-emerging, but only if we can advance our understanding of directed behaviour.

A fuller c.v. and a table of contents can wait till we have more mutual understanding.

Sorry that this has been long-winded but we do start as strangers.

yours sincerely,

copy to Alfonso Montuori.

Dear Alfonso, 3/23/95.

Herewith a straight copy of the letter I wrote for Ms Bernstein, Hampton Press. Unfortunately the printout I sent was jumbled. I think I must have been called away and failed to advance the printer. Anyway I faxed her a straight printout.

I do not know how far or in what manner you are involved with this venture of Hampton Press, but I assume you are acting as some sort of advisor. At least for starters.

It has occurred to me that the decline in popularity of the systems approach was not just due to the McNamarra/Viet Nam phenomena. I think some consideration must be given to the spread of 'the central dogma' of DNA and the Human Genome Project. As presented, and endlessly re-presented, this was a fantastic vindication of the reductionist program, and of the Aristotelean metaphysics that beats strongly in the hearts of the college graduates. After losing millions the venture capitalists are realizing that they were sold a pup (Economist, last month). Fortunately a great deal of research effort is now going down the channels indicated by Barbara McClintock'; but it has not the same simple minded newsworthiness.

Seeing as this will probably be my last go at the problem I have had to assume that I was just one more of those who were sucked into

what was then the latest fashion in intellectual rebellion- systems thinking. A very unpleasant feeling but it is a real possibility. So I have gone back to the debates of the last century when science really started to bite into religion with Wohler synthesizing piss and Darwin going ape. Reaching back into the past is not just a personal whim. Many of the most promising contributions were just by-passed and many of the latest fads, eg catastrophe, chaos and complexity, were tried before. It is not my intention to belabour the reader with this history but to find a way through this maze and reassure myself that I am not just trying to re-cycle an old fad.

Back to work.

best wishes,

INTRODUCTORY STATEMENT.

Current systems thinking is a conceptual mess.

Claims on systems thinking have been made for every one of the competing world hypotheses and for some views of the world that would have to be regarded as pre-scientific. It would seem that all they have in common is a list of 'buzz words' Some sections of so-called 'systems theory' would claim that such a derogatory description certainly does not apply to them. They would point to the consistency of their applied mathematics. The fact that they characterize their product as systems theory strongly suggests that they claim to be more than just applied mathematics. It is those claims that are questionable.

We have allowed systems thinking to drift into this mess because we have thought of systems thinking as a special way of thinking required to cope with the goal-seeking behaviour of living systems and, since WWII with complex engineering systems involving cybernetic sub- systems. A third class of problems has usually muddied waters around consideration of these two classes of goal-directed behaviour. Sheer complexity seems sometimes to give rise to new levels of organization and new laws relating to these emergent wholistic properties. This question of levels should be put to one side except insofar as it relates to the difference between animate and inanimate entities. Within these distinctions the debate seems to have been only a reaction to an ideological, and unsustained program for reductionism.

Limiting the claims of systems thinking to the two areas of goal-seeking behaviour seems to leave the classical sciences unchallenged. It was a matter of indifference, very cold indifference, to those engaged in the classical sciences as to who was involved in fighting for the scraps. They had a long tradition of living with their work being interpreted within the three traditional world hypotheses. This should not be confused with the attacks upon classical science by those of a theological bent, Acquarians and those who saw deeper truths in ancient Eastern thinking. The latter attacks were seen as undermining the extra-scientific legitimation of science.

I suggest that the classical sciences had comprised their own position in order to gain acceptance of their activities and that systems thinking emerged to re-capture the new world hypothesis that was emerging from science.

A TABLE OF CONTENTS. June 95.

8. Human organizations as systems.
9. Systems management and re-design.
10. Systems thinking and our future governance.
11. Ideals and common ground.

NOTE. The intention is to bring together, in one volume, my thinking about using system concepts to understand human beings and their world. I would strongly defend some of my thinking but some of it is no more than speculation. The volume is thus a report on work-in-progress. If I lived long enough to spend another fifty years working in the social sciences it would still be only a report on work in progress. As I hope to show there has been some real advances in the past fifty years, despite the truly deplorable lack of progress in the academically based social sciences. The advances that have been made show only that we can solve today some problems that were insoluble yesterday; they bring us no closer to any end that could be captured in a magnum optus.

NOTES ON THE CHAPTERS.

1. Strictly speaking this chapter is about the precedents to my thinking about systems. It tells the reader where I am coming from.
Angyal's dimensions vs things; Cassirer & two different languages;
Feibleman and material universals - Harre on universals
Gunther on polycontextualism.
2. The distinction between internal and external relations and
Peirce's categories (related to u-relations).
3. Bertalanffy, Ashby, Prigogine, Varela, Sommerhoff.
- family in Woomelang, language in Mullewa.
4. Causal texture.
5. Bunge's mm to mM, Mm to MM. Fields of directive correlations.
6. Chein on motivation, Trist on social relations & Greco. Asch.
7. Asch , Heider, Labov Fanshel.
Quality paper, Rommetweit.
8. Socio-tech, design principles 1 & 2.
9. Bureaucracies, assembly lines, PD workshops.
10. Regionalism and Toward Real Democracy.
11. Ideals, values, search vs delta, cttees & conferences.