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AGAINST CONFERENCES OR THE POVERTY OF REDUCTION IN
SOP-SCIENCE AND POP-SYSTEMS

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"AGAINST CONFERENCES" OR "THE POVERTY OF REDUCTION IN
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ABSTRACT

Science is a consensual system which is imaged, in miniature, by a conference. After examining the consensual system "physical science", attention is directed to the Social or psychological (S.O.P) sciences relevant to the theme topics of this meeting. The mainstream movement in Sop-science is an oversimplified, but socially viable, copy of physical science, replete with reductionist paradigms, but with little relevance to the issues under discussion. General System Theory and Cybernetics may address the theme topics, if the crippling (though organisationally attractive) errors, which render the existing mainstream movement irrelevant, are avoided. Valid analogical reasoning has a significant part to play in this enterprise. Some indication of the present state of affairs may be obtained by noting to what extent this conference differs from conferences symptomatic of an inbred mainstream cult which the title is "against".

INTRODUCTION

Experience shows that unless you are against something, nobody takes the slightest notice of what you say. On this occasion, the most obvious target for anti-sentiment, is a conference; so I am against conferences, today. Not against this one, for that would be rude, and not against any in particular, for that would be overly general. Taken as a social occasion, as a surrogate for learned society, a conference is a capital affair. This is the happy face of a conference. But any conference, or almost any conference, has an ugly face, as well.

The ugly face of a conference is quite dispassionate. It has no glint of wickedness, no shade of guile. It is the bland, immobile face of a review

* I shall not speak this paper, which is intended for reading, but will make comments, appropriate to the meeting, about some parts of it. [*other footnotes at end of paper*].

committee, dedicated to the central limit theorem; a pride or pack of sober citizens informed, at greater distance, by the paid-up members of referees-anonymous. Not far off, there is a faceless nightmare world of pure impartiality; its peer groups may be summoned by an automatic search of author-indexed-abstracts from "the literature".

Such egalitarian arrangements give rise to one result, the entrenchment of norms and foibles proper to what Lakatos calls a "programme of scientific research" a self perpetuating "mainstream". If a conference does that then I really am against it, for, on a small scale, it embodies the quintessence of pathologies, latent in the scientific community at large, but rampant in the mainstream movement of Social or Psychological Science.

By token of the authors and the titles in the *Preliminary Table*, this conference has no such ugly face; not surprisingly, for system theorists should be able to avoid the dangers. It is, for all that opportune to scrutinise the pathological mechanisms which nearly always do come into operation when a body acts in a consensual mode; whether it is a conference or all of an endeavour called "research".

SCIENTIFIC RESEARCH

Lakatos' thesis is that research consists in one or several "scientific research programmes" which are socially regenerative. Some research programmes, "the mainstream", become resilient due to a form of dissonance (Festinger) that rejects or distorts ideas and evidence contrary to the established mores. The thesis is supported by present day, as well as historical, evidence, and I take this picture of things, at least in outline, as given.

The architecture of a mainstream "scientific research programme" is an hierarchy of committee-like organisations, usually supported by a social environment of the same kind. Within this framework there are consensual mechanisms of

communication, decision, and equilibration; the organisation depends upon their activity and inertial properties.

Individuals who take part in programmes of scientific research, interact with nature by consensually agreed instruments of observation such as microscopes and chemical tests. Moreover, specialised "programmes for observation" differentiate notably in astronomy, particle physics, and biochemistry. Data from these observations is candidate evidence, accepted or rejected by a scientist, according to criteria of coherence truth.

COHERENCE

"Coherence Truth" is used in much the same way as Bradley and Rescher use it, and the meaning is congruent with Gaines and Zadeh's "possibilistic truth". "Coherence" stands for a state of affairs in which some body (of scientists, say) have a theory, or a well tried set of hypotheses which are represented as inter-linking propositions. The data, candidate evidence, are represented in like manner, and are more or less compatible with the theoretical beliefs ie. more or less plausible. Data, as such, is licensed by coherence with the consensual measuring instruments, all the assumptions that go into their manufacture and use. It is accepted as truthful evidence, if it does fit both the observational norms and the currently-believed-in-theory. If data fits only part of a theory, then a deviant hypothesis may be rejected, but, before any crucial modification (for example, Popper's falsification criterion) is applied, the reliability of the evidence is checked by a further consensual subsystem, to do with statistics, experimental design, and the like.

It is useful (and legitimate) to extend the meaning of "coherence truth" by translating the static image of propositions and hypotheses into a kinetic picture, on the grounds that scientists are sentient beings; that they and their thoughts, concepts, and hypotheses are processes which may be tagged by procedures undergoing execution. The extrapolation places the word "coherence" in kilter with physical coherence as, for example, the light from a laser is coherent, in contrast to the incoherent radiation of an electric light bulb. Further, in these kinetic terms, "Coherence Truth" becomes, in all cases "meaningful agreement".

DEGRADATION OF AGREEMENT

The good-in-themselves mechanisms which maintain the social organisation of

a research programme are liable to systematic aberrations.

One, very general, aberration is a degradation of agreement (a coherence between individuals, or ideas) into the most rigid variety of consensus.** In matters of value and action, decision is reduced, by Formal "Decision Theory", to selecting amongst ordained "alternatives" on the basis of a probability distribution (which may or may not image a likelihood) and a static parody of "value" (as many-attributed as you wish utilities to be). Organised research is prone to the same defects, manifested by restrictions upon communication and hypothesis formulation.

A closely related aberration is pre-packaged, with this kind of architecture and with these committee-oriented rules and regulations; in summary "Committees do not decide". There is ample quantitative evidence for that intuitively transparent statement; for example, Atkin's elegant studies of the role that may be played by any kind of committee organisation, scientific, academic, or political. The relations that must exist to bring the organisation into being, and that are perpetuated by its operation, permit only certain kinds of activity (Atkin calls it "traffic"). This activity, vote casting, fussing over details, polemic, is "Noise" which may reproduce or reconstruct a structure of the same kind; as when sub-committees proliferate. But it is irrelevant to change in the status quo.

When structural transformations take place they are due to catalytic subsystems, people, who do not act as committees are meant to act, whatever they are called. Of necessity, these components are distinguished; often, by a private language. Beer calls them "Esoteric Boxes" which strikes me as a happy turn of phrase. The power to influence the structure at all significantly lies, fortunately perhaps, with speakers of an esoteric language, of real dialogue rather than codified utterance, in which consensus means coherence.

Scientific research is considered as a social organisation, (a collection of scientific research programmes that are in progress), the day-to-day routine activity is seen as "Noise". In this model of things, most of the observations made on testing hypotheses are also part of the "Noise". They do not inform the social system in the sense of making a significant structural addition to the system. All affirmative (confirmatory) findings are "Noise" and so are many findings that disconfirm hypotheses. The

exception occurs when the preordained experiment has surprise value; some aspect of nature suggests a novel hypothesis.

In general, a social system is informed by hypothesis creation and agreement (or coherence) regarding novel hypotheses. Occasionally, an unexpected event may spur on the creation of hypotheses, but, more often, novelty arises from a dialogue carried on in an esoteric dialect.

ORGANISATIONAL CLOSURE

If Lakatos' thesis, about "programmes of scientific research", is transliterated to the alphabet of system theory, then a consensual system is organisationally closed. It has the autonomy and stability which belong to systems that are productive and reproductive, or constructive and reconstructive, or, appropriately in the cognitive domain, that learn as well as relearn. Biological systems with this characteristic are known as autopoietic (Maturana and Varela). Other systems, social, conceptual, or mechanical, are just "organisationally closed", thereby, autonomous and stable.

The consensual system of scientific research has several distinct, but interactive, "programmes of research"; schools of thought, or disciplines, which share this property. Their productions are manifest as communication, verbal, through journals, or by special forums, such as conferences, set up in conformity to the same pattern.

OBSERVATION AND EPISTEMOLOGY

Fig 1 is a skeletal picture of the consensual system of science. A, B, ... are scientists, who may (or, in cases of dispute, may not) share common hypotheses, open to revision. They do share complete agreement regarding measuring instruments I_A , I_B , ... by token of which, I_A can be exchanged for I_B , and vice versa. Scientists also share canons of reliability, validity, etc, deployed in evaluating evidence.

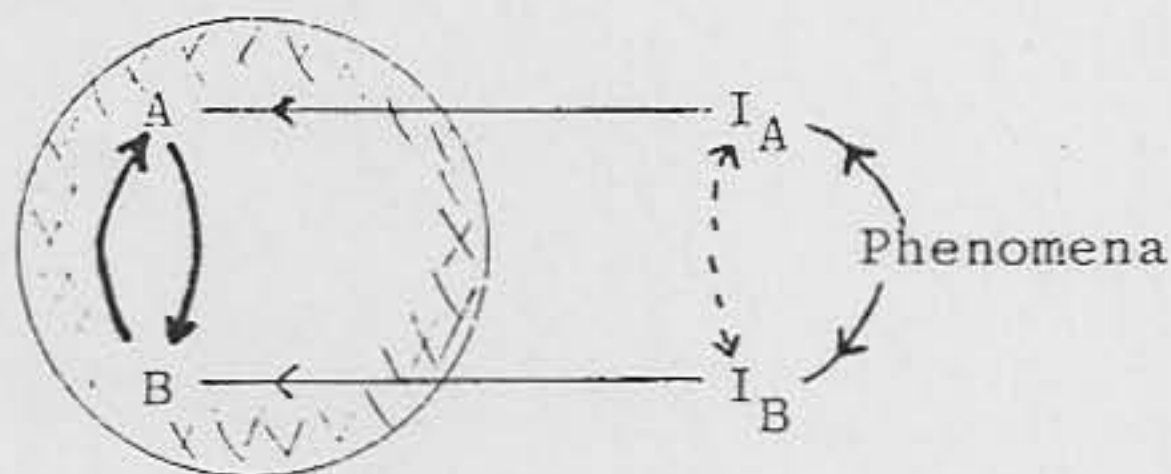


Fig 1
 [Circle with A, B] = Consensual domain; A, B, ... observes I_A , I_B instruments \leftarrow matched readings (factually true propositions). Coherence, or agreement, \rightleftarrows .

The consensual domain (in my own terms the conversational domain) of the consensual system, "science", is its epistemology. This is not open to revision within science. A primary tenet of this epistemology is that impartiality shall be maintained, that scientists regard an observation as an objective, or, literally, an It referenced event. One critically important feature of this situation is shown in Fig 1. Unless something goes amiss, the scientists A, B, ... need never interpret evidence; it is sufficient to compare the instruments, I_A , I_B , ..., for similarity (in the limit, isomorphism) between readings, provided that the instruments belong to some already agreed category, reflecting the consensual system's view of nature.

The readings are deemed factually true subject only to an irreducible experimental error. This expedient provides factual truths about a view of nature, that are, by agreement, prior to the coherence truths of scientists who interpret results and reject, or accept, hypotheses.

The significance of factual truth in the epistemology of science is not (chiefly) to do with any absolute dogma about nature; for we have argued that most observations are "noise" to a social system; the "noise" of a daily round of existence. However, factual truth is very much to do with a basis for accord. In that respect, the criterion is not unique; for example, another is proposed, later on (in Fig 2). But, in its correct place, the idea of factual truth has incalculable worth. The Royal Society of this City was formed alongside the notion that men of different persuasion and loyalty will not wax acrimonious if their debate is centred upon the factual truths of nature. Their accord underlies an aesthetic of reason, just as a reserved and isolated axiomatic agreement underlies the beauty of mathematics and logic.

THE SPECIAL POSITION OF ANALOGY

Due to the correct (but not unique) criterion of Fig 1, the epistemology of science is deliberately partitioned into two compartments. One of these compartments, "working science", operates exclusively with logical consistency and factual truth. It contains all the conceptual apparatus needed for deduction and for induction from instances to generalities under a given rule. It does not contain the mechanism for which Pierce coined the term abduction; the creation of rules, hypotheses or inventions. In contrast, the other compartment, call it "science

philosophy and innovation" contains the entire equipment, including logical coherence and agreement, but is guarded in an "esoteric box" by means of various linguistic devices (for example, it stands as "metascientific"; or "talking about the nature of science").

The existence of two compartments is blurred for several reasons. Any individual scientist can, from time to time, operate in both compartments (though because of the social organisation of science, the majority operate only in "working science"). The hypotheses of working science are numerous and complex enough to generate further hypotheses algorithmically, giving the appearance of invention. For instance, causal or probabilistic inductive inferences are available in working science and seem diverse until they are recognised as mathematically refined ways of arguing by similarity; that the future will be like the past, or that the accretion of evidence from independent sources is witness to a pattern.

The fundamental distinctions or the similarities that give "identity" to particles, places; the principles of independence, order and the conservation of quantity, the critical distinctions, may not arise within "working science". Any change is relegated, (of necessity, if the factual truth of evidence is to be preserved) to the esoteric box of science philosophy. In this compartment the major mode of reasoning is by analogy construction; the abduction of a difference and a similarity of form or process. It is characteristic of all major discoveries; for example, the notion of a field, of quantisation, of special relativity in physics; of the periodic table in chemistry; of genetic recombinations and the codon in biology.

Accounts that equate science with "working science", depict the "ugly face" of science, only. "Science" with a proper structure must include "science philosophy", as well. Apart from serendipitous results (such as radio stars, or observation of a soliton impulse as a wave on a canal), abduction is the only way in which the organisationally closed system of science is informationally open. Abduction or analogy construction is the foundation stone of all discoveries; neglect it, and science is autonomous, but pointless.

ADVANTAGES AND MALADAPTATIONS

The consensual system of scientific research has pretty clear advantages. Methods, notably mathematical, are appli-

cable to all the organisationally closed research programmes participating in the consensus. Programmes of observation and experiment can be continued in this stable framework to achieve otherwise unattainable results, like determining long term trends in astronomy, ecology, or oceanography, the systematic investigation of DNA; or operations as varied as the programme of space exploration, and the development of semiconductors.

The adaptations that exploit these advantages are not so beneficial, although they seem "necessary" if viewed in retrospect. For example, strategies, mostly reductionist in character, are ingrained with remarkable tenacity and are promoted as though mandatory when they are, in fact, just useful tactics. The style of communication most clearly exemplified by journal formats, is tuned to maximise the transmission of messages compatible with one or other of the research programmes; it is generally impersonal, for thereby an individual's responsibility can end at satisfaction of the mainstream rules. Since a rapid turnover of messages is normal, no one of them can occupy too long; the scale is set by the time allowed for speaking at a conference, or the length of a publication. The initial maxim, "no-one takes any notice unless you are against something", appears in a special form. The things you can be intelligibly "against", are partly autonomous research programmes and these are weak alternatives; to be "against" one kind is to be "for" the other kind. Finally, to be understood at all, except as a "philosopher" (who is not quite scientific) or an inventor (who is just a maverick) you must subscribe, in principle at least, to the currently fashionable epistemology, for this shapes all the languages of science, as well as the beliefs entertained as plausible, by scientists. These adaptations degrade the quality of agreement just as formal decision theory degrades real life decision until "committees do not decide".

SOME FINDINGS

At the meeting I shall take the opportunity to present some anecdotal data from an informal survey of research workers, carried out over 10 or 15 years. Their responses, sometimes noted at conferences, where they presented papers, support the point of view expressed in the last paragraph. It seems, for example that researchers seldom know much about the epistemology to which they "subscribe". It is often noted that "science" is so large that "any one investigator cannot possibly know all of it"; this is "an age

of specialists". There is a pervasive feeling that "science advances by small, cautious steps; we are simply testing hypotheses, small in themselves, a contribution to the great goal"; however, there is a strong finding that researchers who feel this way have not the remotest idea of what the goal may be, and it is usually defined tautologically, as "science" or "the progress of science". Between them, these factors are conducive to a studied irresponsibility. In ignorance of why they do science, researchers do it like production-line work in a factory, without much commitment or conviction.

Since little time is permitted, little can be said. Hypotheses that deviate a trifle from the norm have a chance of being understood, less pedestrian conjectures, almost none. For, if there is anything of consequence to say, it does take time and effort. Rutherford's reported comment, "you should be able to explain your theory to the barmaid ...", is entirely valid. But, it may be necessary to spend a great many drinking hours in her company, before she will understand. The mainstream cult of science is simply not a lady; it will not listen, to begin with.

CONSEQUENCES IN DIFFERENT KINDS OF SCIENCE

If a compartment of science is a mainstream juggernaut, it does require technicians; manifestly, there are plenty of them, all the self effacing backroom boys. It does little harm, if they like to image themselves as "scientists", providing they do not pretend to be a Newton or a Boyle, and provided their conceit in the matter does not preclude the activity of real scientists.

In physics, biology, or where the component research programmes do have a firm foundation, innovative science is still possible. The innovator will be damned as a heretic, but with enough persistence, can win out as a hero, by appeal to the criterion of Fig 1.

Science, as it bears upon the human condition is generally a different matter. There are a few areas in which the method of Fig 1 and the allied epistemology, work well; few enough to be enumerated.

First the study of mental disorders in physiological terms is of value; so, also, is the psychophysiology of brain function as a whole (the field of enquiry delightfully summarised in Blakemore's Mechanisms of the mind). Next, the examination of perceptual and cognitive phenomena in the spirit of Fechner or Helmholtz has parity with physics or biology, for the

experimental subject connives with the experimenter, quite explicitly pitting his (unexplained) awareness against external and objective standards. Finally, studies of behaviour pure and simple, are genuinely scientific, whether the behaviours stand by the criteria in Skinner's methodological essay, or whether the experimentation is relevant for pragmatic reasons, as in ergonomics.

With these exceptions, the sciences relevant to the human condition, or the quality and stability of social systems, have a curious status; "the Social or Psychological Sciences", or, for brevity, "the Sop-Sciences". They are peculiar because the arrangement in Fig 1 is not really compatible with experiments or observations likely to yield relevant data.

The reason is quite clear. Fig 1 secures precisely objective (it referenced) observations, by disallowing the observation of observers (self-or-other referenced) and at the cost of placing abduction and analogical reasoning outside the consensual system of science.*** With the exceptions just noted, it happens that all the phenomena, relevant to the theme of this gathering, are self-and-other referential, (as, for example, the present discussion of consensual systems). Most of these phenomena also involve abduction, analogy, and social evolution, which must be adumbrated by a satisfactory Sop-Science. Nearly all relevant enquiries refer to the consciousness of one individual with another regarding some concrete or intellectual thing. A satisfactory Sop-Science should accommodate the phenomenon of consciousness within its scheme.

Mainstream Sop-Science, satisfies none of these desiderata, nor, in principle, can it do so.

MAINSTREAM SOP-SCIENCE

Mainstream Sop-Science consists of two closely related research programmes: one is a naive behaviourism, the other is an ego-oriented discipline, compatible with behaviourism, in which some neurones are added to provide motive and a surrogate for mind.

'Sop-Science is built in the image of physical science, and is revered as though it really had the content of physical science. The ethos of reductionism and the maladaptive attitudes are carried over piecemeal from physical science. The researchers are, at least, as purposeless (in that sense, irresponsible).

An organisation with proven success in one field is likely to be successful

in another. In our culture the dominant organisation is physical and biological science. Its least troublesome formulation (roughly classical mechanics) has been transferred, as a gross copy, to the domain of S.O.P. phenomena and, regarded only as a social organisation, it thrives very well, as an organisationally closed but not informationally open system; it looks like science, it tastes like science, but it is ... a pretty arid husk. This particular carapace is a quirk, perhaps, of Anglo Saxon tradition (elsewhere, there are probably systems built to imitate Yin or Yang, or even the occult)

Because of reverence for a content-free imitation there is an odd inversion of the emancipation of scientia, of knowledge, from the weight of opinion which took place, in physical science, during and after the Renaissance (recounted, for example, in Hacking's monograph). In mainstream Sop-Science, findings are supported by the authority of "Science"; the opinion of its social-organisation. Since there are no relevant facts behind the pretension, the authority is borrowed. The invocation of "science" as a kudos word is as painfully familiar as it is pervasive. For example, "Objectively Marked Mental Tests or Examinations" (meaning, tests with response formats that can be inexpensively, or computer, scored) is converted, under consensual pressure, to read "Objective Mental Tests or Examinations" with special scientific dignity (though the connotation is obscure).

THE POVERTY OF SOP-SCIENCE

In both of the two major programmes, observations are referred to the paradigm of Fig 1, and an indiscriminating gaggle of events (stimuli, responses etc... designating reactions, changes of attention, etc ...). It is generally conceded that Fig 1 does not work "very well" in the S.O.P. domain. All results are "statistical" and a great deal of effort is concentrated upon reducing the variability of observations. It is much more important to notice that Fig 1 is an altogether misplaced paradigm. The conditions of measurement do not admit the observation of Sop-relevant events. For example, consciousness is either relegated to a meta-theory and placed outside science or dismissed as an epiphenomenon. There is plenty of data with some statistical regularity, but few results relevant to the human condition or social stability.

Perhaps the most damaging effect of this arrangement is an unintentional, though unavoidable, suppression of innovative ideas. A heretic working in physical

science has recourse to the paradigm of Fig 1 which, in that case, is appropriate. In mainstream Sop-Science, his path is blocked because observations of the type permitted by Fig 1 are not appropriate, or even applicable, to relevant phenomena. Hence, a seriously radical hypothesis is utterly untestable against the criterion of Fig 1 and will be rejected by the consensus of "mainstream" opinion. For an unbiased perspective, let me quote from the last few lines of Beloff's sympathetic review of Popper and Eccles' The Self and its Brain; to speak of consciousness and question some hoary presuppositions of identity is a major heresy. Beloff "hopes the book will succeed ...", but doubts whether it will in "the present climate of opinion ...". The heresies are better concealed in Jason Brown's Consciousness as a result of which the book received a more general welcome but, in fact, there are many similarities. The thrust of this paper is to hope, in Beloff's words, that "... the whole intellectual climate will be so radically transformed...."

The entrenchment of a "climate of opinion" is exacerbated by the statistical character of Sop-Scientific evidence which makes it easy to advance an hypothesis which deviates only slightly from the status quo, provided enough (dubiously independent) data samples are available. The logistics of sampling and data processing clearly favour institutional subsystems that are already part of the "Mainstream" and there is, of course, an audience immersed in this "climate", familiar with ANOVA passes and significance levels.

The more fundamental criticism of Fig 1 as an altogether inappropriate paradigm of observation in the Sop-domain, is sturdy enough to stand up against a wide variety of changes upon what is, or is not, relevant. For example, Fig 1 does not fit data from structured interviews; it does not fit the work of Luria, or (quite distinctly) the Piagetian School. At another extreme, Fig 1 does not accommodate Bartlett's work on memory, or Duncker and Wertheimer's work on problem solving, or the recent and detailed studies of thinking and learning of Entwistle and Hounsell or Gilbert.

In the present climate of opinion, these sources of genuine data are regarded as providing "only clinical", evidence, or are discounted altogether, or else Fig 1 is "made to fit" by ranks, scales and so forth, that are statisticised into a numerical sludge.

A RELEVANT AND SATISFACTORY SOP-SCIENCE

Fig 2 shows a paradigm for the pre- precise observation of subjective events (like agreements, questions, from A to B, answers from B to A, etc). The observer is OB and the phenomena observed are agreements between A and B. These phenomena mark the appearance of organisational closure between A and B who may, depending upon the context, be individuals, points of view, (perspectives) social groups, or even the scientists of Fig 1. The A,B, agreement observed by OB is perceived by A and B, the participant individuals (or groups) as a coherence truth, relative to a topic under debate.

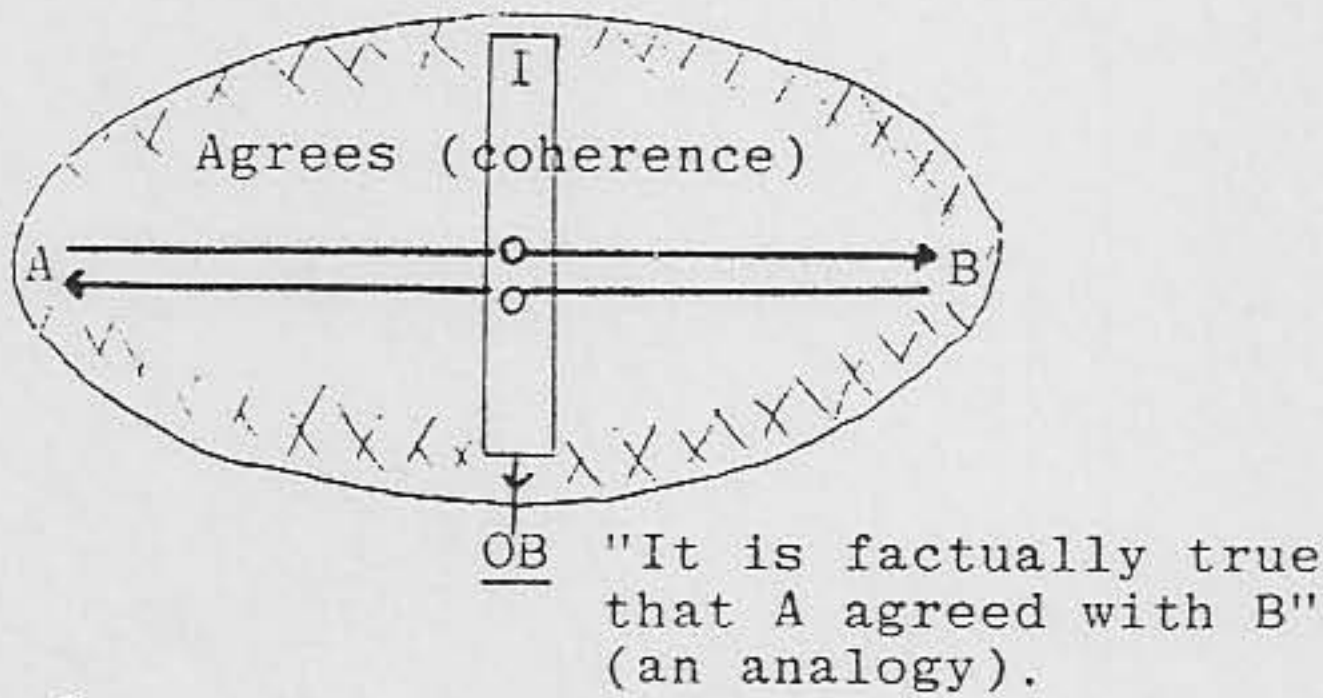
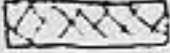


Fig 2

OB observes A, B,... participants (may also be observers) I = Interface (equivalent, in a language, to I_A, I_B of Fig 1)  Consensual domain.

A scientific observer is concerned with establishing factual truths. Now, what kinds of statement are said to be true or false? In Fig 1, for example, propositional statements are said to be true or false.

The minimal statements of Fig 2 have the form "A and B agree or disagree about a topic". Such a statement draws a distinction between A and B, as different organisms, different roles, factions, societies or perspectives. This distinction of A from B supports a similarity, perhaps an isomorphism, between A's and B's concepts marked by the A,B, ... agreement. It follows that all of OB's factually true statements are analogical statements; this is one defining characteristic of Fig 2.

Under the kinetic interpretation of agreement, the coherence obtained is an information transfer which marks an A,B,.. consciousness of whatever they agree. This is another defining characteristic of Fig 2.

There is more to the matter than that; A and B may be observed to agree, amongst other things, about analogies which they construct themselves; both the distinctions and the domain of similarity. Hence,

A or B could act (if desired) as OB, or vice versa. The epistemology of a relevant Sop-Science, must countenance participation. In it, the impartial or numinous stance of OB evaporates, since he may be a participant in the system he observes.

The participatory scheme frequently provokes the criticism that this is not science, a point of view adopted by the "mainstream" movement. But, unless these preconceptions are entertained, Fig 2 is just as scientific a paradigm as Fig 1.

If it is possible to clothe the bare observational paradigm of Fig 2 with a fitting methodology and to embed the whole construction in an appropriate epistemology, then there is a genuine**** Sop-Science, which is one kind of operationalised and interpreted general system theory.

For several years, it has been possible to give an adequate account of meaningful agreement, to operationalise (and quantify) the process of organisational closure, how autonomy is achieved and how, without losing their autonomy, organisationally closed units may become part of a more inclusive closure.

More recent epistemological developments reveal a class of essential bifurcations. They resemble the "Catastrophes" of Thom and Zeeman, but generate distinctions; first, between participants, A,B,.. so that further agreements may take place; next, between areas of knowledge or behaviour which are reunited by abduction and analogy construction. These predicative distinguishing operations show how it is that some organisationally closed systems, of paramount importance in the S.O.P. domain, are also informationally open.

This type of theory, which encompasses self and other reference, as required, is relativistic and reflective. It is not, and does not look like, Mainstream Sop-Science; however, it should be familiar enough (though possibly not in the S.O.P. domain of interpretation) to General System Theorists.

I shall thus employ some results and derivations; (germane, given a relativistic and reflective stance) to contradict the conventional wisdom of the Mainstream Movement; more particularly, to submit that various Sacred Cows, which graze upon this fertile pasture are no more nor less than false idols.

SACRED COWS; THEIR NATURE AND NURTURE

These creatures are reductionist principles, perjorative phrases, and

tricks of the trade (as, for example, the trick of partitioning a problem into sub-problems that are more readily solved).

(a) of Some assertion: "it is a mere analogy" (which means that the assertion is suspect, as not respectable in scientific circles). Well now, from Fig 2 all observations in Sop-Science are analogies; or, even if you opt for Fig 1, the information gained by science is abductive, part and parcel of analogy construction. Bachelard's critique of analogical reasoning in science comes into a different ball park; it is quite fair to say, "that is a mere similitude".

(b) "Leave out the epistemology ..." a favourite gambit amongst hard-nosed data-getters. The fact is, epistemology cannot be excluded and attempts to exclude the stuff give rise to silly and mostly spurious conclusions (see, for example, the collection Alternatives to Piaget, and its references, not, incidentally, to discover "alternatives" but to illustrate, in practical terms, the result of dissecting out minute portions of a thesis, here, Piagetian) and testing them out of context, by the canons of a different and incompatible epistemological framework.

(c) Since Gagné's pioneering work, at least there is widespread belief in an "Hierarchy of Knowledge"; for example, that numbers must be learned before arithmetic operations make sense. Over the past five or six years, we have made detailed records of, in aggregate, more than 10,000 hours worth of learning and I can see no evidence in favour of this notion, quite the reverse is generally evident. Knowledge is more like the labyrinth inside a sponge which different individuals may enter by different routes. Its substance corresponds to ignorance whittled away by learning until there is no obstacle. Surely, when people think, or act, they adopt a perspective (several, simultaneously, in creative thinking) and under a perspective thought and action are hierarchic-
alised. So Gagné's results, and those obtained later, are (obviously) valid. But the hierarchy is not in knowledge as such, nor incidentally, in organisations, plans, or prescriptions, as such.

(d) "There are always simple things; units or atoms". For example, it is said that individuals are units, personalities are units that "an economic man" is a unit, a city is a unit; that there are "most elementary" topics or a simplest canonical state description. If these statements voice locally, contextually or momentarily useful beliefs, there is no difficulty. But, if taken to signify absolute or universal dogmas, then they

are all flatly denied. From Fig 2, integrity or autonomy (hence, unity relative to some participant), is a characteristic of a process, not only of an observer.

a personality (as in a profile). Using the Mainstream test instruments, a human being, retested, has (with rather wide statistical limits) one personality. In an ingenious experiment by Prescott, the familiar test instruments were used, but responses were elicited in various contexts of daily life, home, work, in transit, at a restaurant, a theatre, etc. Under these conditions, one human being has several personalities. Conversely, it can be demonstrated that a unitary "personality" is usually a role played by several human beings, often acting in concert. Do you prefer persons or personalities as units? That depends upon circumstances, perhaps; but neither is a universal unit. Laurillard found much the same for learning; perspectives exist, but are (in this case) attached to people in the context of a task.

(e) "Proceed gradually", or a closely related notion, that tried and tested theories are tenable even though they happen to be irrelevant.

For example, in their scholarly work, on the acquisition and use of natural language, Miller and Johnson Laird deploy the strategy of breaking a problem into parts, showing by minimal deviations from "learning theory", that "learning theory" will not account for natural language phenomena, nor will a host of increasingly radical variants. Some 600 pages lead to very reasonable conclusions, for example, that language has a procedural core. The discussion is illuminating, but what is the assumed "learning theory"?

Von Foerster incisively demonstrated, some years ago, that it is a theory about the behaviour of two or more finite state machines (with storage) coupled back to back, the contrivances discussed in the 2nd volume of an authoritative work, the 1963, *Handbook of Mathematical Psychology* (eds, Luce, Bush and Gallanter). The application domain of this theory is chiefly animal management, though it is usefully deployed for human conditioning, and has more general value in regulating severely handicapped human beings. Whatever else, "learning theory" in the intended sense, is irrelevant to the "human condition" or "social stability".

(f) Stability (especially in society) is commonly associated with regimes in which the state trajectories of an en-

semble of systems are approaching static or dynamic (cyclic) equilibrium. The idea is sensible if a state description is given and in no way influenced by the system behaviour. But if Sop-Science is rooted in Fig 2 these requirements are not satisfied. At most, there is a relative, observer-and-participant contingent, demarcation of behaviour from structure (stability, autonomy, is organisational closure... the productions construct the structure...) and, as a rule, any tentative state description which may be adopted must change (what else is intended by the "Essential Bifurcations" that render the system conscious and "informationally open" or just "Self Organising", either in Nicolis and Prigogine's sense, Von Foerster's or my own?).

Under these circumstances, it would be positively hazardous, as witnessed by innumerable global simulations, to pursue the usual control theoretic expedients. Moscovici suggests as much in the social context, as does Bateson in anthropology. At this point, I am prepared to demonstrate; for the price of some technical jargon, that nearly all stable systems are up against a catastrophic boundary; most of them one step away from a catastrophe which, however, they recognise.

This is asserted, in particular, of stable concepts and stable coalitions, in a social system. But it is a general idea, commonplace in practical politics. If it were mistaken, there would be no conferences at all or any place to hold them.

SOP SCIENCE AND POP SYSTEMS

The views expressed in the last sections are at odds with the mainstream perspective, and they are claimed to be more closely related than it is, to common sense.

The points "against" mainstream Sop-Science are certainly phrased in General Systemic terminology; if these two disciplines are weak alternatives then presumably I am "for" General Systems. Clearly, I do have a strong commitment to this science or metascience, or philosophy. Because of that, it is particularly important to air some consequences of its nature as a science.

An institution, like a living organism, moves through the euphoria of commencement, into an apathy of ossified operation until it meets the agony of death. Are these stages in development essential to a collection of scientific research programmes, also; for example, to General System Theory? The question is poignant for System Theory

is approaching its middle age, the efficient but automatic repetition of hoary themes. It could become a popular-theory; Pop-Systems, diluted to cause no offence, but slick enough to furnish its quota of useful methods. If that were so, then the innovators of the field might well take the advice that all of us, I imagine, would furnish if they formed part of an institution; namely, to move out sideways and on elsewhere.

Probably, five years ago, the only honest General Systemic answer to the question was that some such fate is unavoidable. In those days, it would have been appropriate to cite organisational closure alone; uneasily, because we are participants in the closure and know of variations on the theme: Kuhnian revolutions in science, for instance, only as random events. Nowadays, a different answer is possible. General System Theorists are surely wise enough to disavow "randomness" (excepting for the purely technical usage) and to recognise that information transfer is not just the measure of selection but is that many faceted commodity which Ashby termed "Variety". Rephrased, and made to capture his intention more precisely, it is a "becoming"; of local synchronicity between systems otherwise asynchronous; of local dependency between systems distinguished as independent. In the idiom of this paper, information transfer is coherence amongst the otherwise incoherent, and it is quite non-trivial to say that systems can be organisationally closed and informationally-open; moreover, some systems are of this type. For these, a "catastrophe" is not an incommensurable case of singularity, but rather a creation of autonomy, an essential bifurcation that forms independence (not just "parts"), which is the promise of coherence in the old autonomy. No accident or chance is entailed by this mode of evolution; the aleatory, "noise" to the system, is a different matter, (more like the "noise" of regular observation).

Are the Scientific Research Programmes of General Systems of this type, so that heretics can be honestly advised to stay in, and take advantage of the bifurcations, rather than get out as soon as the era of Pop-Systems is visible on the horizon?

I do not know, but my conviction that our science does evolve, a conviction carried over into the Sop-Science dogmas of this paper, rests chiefly in some survey data. Researchers in the Sop-Scientific interpretation of General System Theory are innovative, but also, com-

pared to other scientists in the sample, ready to accept responsibility for the purpose they announce. In place of the response, "I might do that research if it is funded", they reply, "I shall do this research, and obtain the funds required to do it". This is a fitting attitude if they are not post haste towards the exit door. That is what I believe, myself, as one participant observer.

Edward Cainiello made the comment whilst we were overlooking Naples: "It would be quite indecent not to die; but inexcusable to make an ugly mess of it". Well, I agree with that. So let me substitute the Pop-Systemic unavoidable: the apathy of age, the agony of death, by this, another heresy; an elixir of evolution latent in the art we have. It is a calculus of decadence, where beauty lies; within that calculus the promise of transcendence, and thereby, metamorphosis.

FOOTNOTES

** Out of deference to the most common usage in sociology, I relinquish the habit of other publications and mean, by "consensus", either the genuine (coherence) agreement, born of meaningful communication, or the stultified message-passing which takes place when rigidity sets in.

*** Everett's and De Witt's elegant resolution of the "many observer" paradox is consonant with Fig 1, but renders the "observers" as simply servo-mechanisms, unable, by definition, to theorise or think.

****A genuine Sop-Science depends upon forging the methods and the epistemology proper to Fig 2. A great deal of progress in this direction has been made (in alphabetical not chronological, order) by Beer, Braten, Daniel, Dirkzwager, Flores, Von Foerster, Gergely, Glanville, Kallikourdis, Maturana, Melitis, Midoro, Nemeti, Nicolis, Nowakowska, Pedretti, Robinson, Shaw, Thomas, Valach, Varela, Winograd. The list is incomplete and limited to schemes with formal foundations. There is a great deal of relevant sociological work (Geyer and Van der Zouwen) and the Conversation Theory which I have developed also offers a comprehensive candidate scheme, which in major respects, is congruent with the others (Pask).

REFERENCES

- Atkin, R. H.
1977. Combinatorial Connectivities in Social Systems, London, Heinemann
- Atkin, R. H.
1978. "Decision Making as an Event Search Traffic on a Multidimensional Structure", Proceedings 3rd ARI Richmond Conference on Decision Making in Complex Systems
- Bachelard, G.
1970 La Formation de l'esprit scientifique, Paris, J. Vrin.
- Bartlett, F. C.
1932 Remembering, Cambridge, Cambridge University Press.
- Bateson, G.
1973 Steps Towards an Ecology of Mind, New York, Paladin
- Beer, S.
1979 The Heart of Enterprise, London Wiley
- Beloff, J.
1978 "Is Mind Autonomous", British Journal for the Philosophy of Science, Vol 20, No 3, 265-283
- Blakemore, C.
1977 Mechanisms of the Mind, London Cambridge University Press
- Bradley, F. H.
1914 Essays on Truth and Reality, Oxford, Oxford University Press
- Braten, S.
1978. "Competing Modes of Cognition and Communication in Simulated and Self Reflective Systems", Proceedings, 3rd ARI Conference on Decision Making in Complex Systems, Richmond, August. (in press).
- Brown, J.
1977 Mind Brain Consciousness, London, Academic Press
- Daniel, J. S.
1975 "Conversations, Individuals and Knowables: toward a theory of learning", Engineering Education February, 415-420.
- Dirkzwager, A.
1978. "Human Behaviour in Information Processing Systems", in Trappl, R and Pask, G. (eds) Progress in Cybernetics and Systems Research, Volume IV, New York, London, Halsted Press, 14-21.
- Duncker, K.
1945. On Problem Solving, reprinted 1972 West Port, Connecticut, Greenwood
- Entwistle, N. J. and Hounsell, D. J.
1975. How Students Learn, University of Lancaster.
- Festinger, L.
1972. Cognitive Dissonance, Stanford, Stanford University Press.
- Flores, C. F.
1978. "Action Conversation and Organisations", Stanford, Stanford University (in press).
- Flores, C. F. and Winograd, T.
1978. "Understanding cognition as understanding", Stanford, University of Stanford, (in press)
- von Foerster, H.
1976. "An Epistemology for Living Things" L'Unité de l'Homme, Morin, E (ed), Paris, Editions de Sevel.
- Gagné, R. M.
1965. The Conditions of Knowing, New York Holt, Reinhardt and Winston.
- Gaines, B. R.
1976. "Foundations of Fuzzy Reasoning", University of Essex.
- Gergely, T. and Nemeti, I.
1977. Various publications, Institute of Applied Computer Science, Hungarian Academy of Science, Budapest.
- Geyer, F and Van der Zouwen, J.(eds)
1979 Sociocybernetics, An Actor oriented Social Systems Theory, Amsterdam, Martinus Nijhoff.
- Glanville, R.
1976. "What is Memory that it can remember what it is?", Proceedings 3rd EMCSR, Washington, Hemisphere.
- Glanville, R.
1978. "The Concept of an Object and the Object of a Concept", Proceedings 4th EMCSR, Washington, Hemisphere.
- Glanville, R.
1979. "The Same is Different", in Zeleny, M, (ed), Autopoiesis, (in press)
- Goguen, J. A.
1975. "Objects", International Journal General Systems, Volume 1, 237-243
- Hacking, I.
1975. The Emergence of Probability, London, Cambridge University Press.

- Kallikourdis, D, Pask, G. and Scott, B C E
1975. "The Representation of Knowables"
International Journal of Man
Machine Studies, 7, 15-134.
- Lakatos, I.
1967. "Criticism and the Methodology of
Scientific Research Programmes",
Proceedings of the Aristotelian
Society, 69, 149-186
- Laurillard, D.
1978. "The Processes of Student Learn-
ing", Proceedings 4th Internat-
ional Conference on Higher Ed-
ucation, University of Lancaster.
- Luce, R.D., Bush, R.T. and Gallanter, E.R.
1963. Handbook of Mathematical Psychol-
ogy, Volumes, I and II, New
York, Wiley.
- Luria, G. R.
1968. The Mind of a Mnemonist, New
York, Basic Books.
- Maturana, H and Varela, F.
1976. "Autopoietic Systems: A
Characterisation of the Living
Organisation", BCL Publications
on Microfishes, 821 Bond, Peoria,
Illinois 61603.
- Melitis, H.
1977. Concepts and Analogies in Cyber-
netics, Thesis, Uxbridge, Brunel
University.
- Miller, G. A and Johnson-Laird, P. N.
1976. Language and Perception, London,
Cambridge University Press.
- Moscovici, S.
1976. Social Influence and Social
Change, London, Academic Press.
- Nicolis, J. S.
1978. "On the Spontaneous Emergence of
Decision Making Constraints in
Communicating Hierarchical
Systems", Proceedings, 3rd ARI
Richmond Conference on Decision
Making in Complex Systems.
- Nowakowska, M.
1979. "Foundations of Formal Semiotic.
Objects and their verbal copies".
Institute of Philosophy and Soc-
iology, Polish Academy of Sci-
ences, Warsaw.
- Osborne, R. J. and Gilbert, J. K.
1978. An Approach to Student Under-
standing of Basic Concepts in
Physics", University of Surrey.
- Pask, G.
1961. An Approach to Cybernetics,
London, Hutchinson.
- Pask, G.
1975. The Cybernetics of Human Learn-
ing and Performance, London,
Hutchinson.
- Pask, G.
1975. Conversation Cognition and
Learning, Amsterdam and New
York, Elsevier.
- Pask, G.
1976. Conversation Theory: Applicat-
ions in Education and Epistem-
ology, Amsterdam and New York,
Elsevier.
- Pask, G.
1977. "Organisational Closure of Pot-
entially Conscious Systems and
Notes", Proceedings NATO Con-
gress on Applied General Systems
Research Recent Developments and
Trends, Binghamton, New York and
Realities Conference, est Found-
ation, San Francisco.
- Pask, G.
1978. "Observable Components of the
Decision Process and a revised
theoretical position", Proceed-
ings, 3rd ARI Richmond Confer-
ence on Decision Making in Com-
plex Systems
- Pask, G.
1975-
1977. "The Influence of Learning Strat-
egy and Performance Strategy
upon Engineering Design", Rep-
orts, USAF F44620.
- Pask, G.
1979. "Consciousness", Proceedings of
4th EMCSR, Linz, Austria, Journ-
al of Cybernetics. Washington,
Hemisphere, 211-258.
- Pask, G.
1979. "Learning Styles, Educational
Strategies and Representations
of Knowledge: Methods and App-
lications" Final Report,
SSRC HR 2708.
- Pask, G.
1979. "L_p a Protolanguage", Report
System Research Ltd.
- Pask, G and Lewis, B. N.
1969. "The Self-Organisation of a
Three Person Task Oriented Group"
Proceedings NATO Symposium. The
Simulation of Human Behaviour.
Paris, Dunod, 291-311
- Pedretti, A.
1979. A Conversation Theoretic parad-

- sgm for Semantic Representation
(contains 4 papers , 2 previously published, 1977, 1978), University of London
- Pierce, J. R.
1962. Signal, Symbols and Noise, London Hutchinson.
- Popper, W. R.
1959. The Logic of Scientific Discovery London, Hutchinson.
- Popper, K. R. and Eccles, J. C.
1977. The Self and Its Brain. An Argument for interactionism, Amsterdam, Springer.
- Prescott, S., et al
1978. Environmental Effects on Cognitive and Affective Studies, University of Chicago.
- Rescher, N.
1973. The Coherence Theory of Truth, London, Oxford University Press.
- Robinson, M.
1975. "Prisoner's Dilemma: Metagames and other Solutions", Behavioural Science, 20, 3, 201-205.
- Shaw, M. L. and Thomas, L. F.
1977. "FCCUS on Education: An Interactive Computer System for the Development and Analysis of Repertory Grids", International Journal of Man Machine Studies, 10, 2, 139-174.
- Skinner, B. F.
1959. "A Case History in Scientific Method", in Koch, S. (ed) Psychology. The Study of a Science, Vol 2, London, Wiley.
- Skinner, B. F.
1969. Contingencies of Reinforcement. A Theoretical Analysis, Appleton-Century-Crofts.
- Thom, R.
1973. Models Mathematiques de la Morphogenese a collection of essays, Paris, ChristianBoigois.
- Varela, F and Goguen, J.
1976. "The Arithmetic of Closure", Proceedings, 3rd European Meeting on Cybernetics and Systems Research Vienna, Trappl R, (ed). New York, Hemisphere.
- Wertheimer, M.
1961. Productive Thinking, London, Social Science Paperbacks.
- De Witt, B. S and Graham, N.
1973. (eds) The Many Worlds Interpretation of Quantum Mechanics, Princeton, Princeton University Press.
- Zadeh, L. A.
1978. "PRUF-a meaning representation language for natural languages", International Journal Man Machine Studies, 10, 4, 395-461.
- Zeeman, E. C.
1976. "Catastrophe Theory", Scientific American, April, 65-83.
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