

S-sequences are 'teleological'¹ in the weak, Taylor sense (1964) in that they are ordered by such concepts as 'reason for' and 'intention' and 'following a rule', which P-sequences are non-teleological, in that they are organized by the concept 'physical cause of ...'. S-, and P-sequences are not, *in general*, mutually convertible, because of the degree of mismatch of their respective organisation.

The structure of the S-system determines the structure which is sought in the P-system when we are seeking an explanation of the S-system. Since it is a logical point that the criteria of identity for entities and systems on the P side must derive from the S side, in order to be relevant to the explanation of performance, psychology must necessarily impose its form upon physiological investigations. If the S-sequence is not only grammatically ordered, but is also seen to involve modelling and monitoring feedback, then it will impose a system-theoretical structure upon physiological hypotheses, since the neurological system must contain the necessary mechanisms for the performance of the higher-order functions.

And, insofar as we inherit those mechanisms there will be deep structures in grammar in the rules of social life. We have seen preliminary steps in the discovery of these structures for language by Chomsky and for certain aspects of the social behaviour of men by Lévi-Strauss (1968). If the society of men is essentially a linguistic phenomenon, then there should be 'social universals', and Lévi-Strauss has opened up one way of seeking for them. But it remains an empirical question whether there is an underlying deep structure to meanings, and this would still be an empirical question even if the Chomskyan grammatical thesis for languages was finally established.

Rom Harré

Review of the Paradigm Shift

Having already mentioned the difficulty I had with various models of scientific research encountered at the University of Wisconsin from 1963-66, it will not be difficult for readers to understand why I was excited about Harré's lecture. The idea that *new ways were available* to approach the subject of human movement studies was essential, but it wasn't just "new ways of approach" that I sought. I now realize that it was a new climate of thinking.

It was rapidly becoming apparent that ideas I had held for a long time about dancing were incompatible with firmly entrenched, existing notions about [a] the nature of human movement itself and [b] the nature of the human beings who generated the movements. The 'Old Paradigm' in any of its forms was radically opposed to the nature and character of human movement as well as to the nature and character of the creatures who produced the movements. My thought was already compatible with the New Paradigm.

I discovered that a colleague, Malcolm Crick (1976), attended Harré's lectures in philosophy of science. Asking to accompany him, I, too, attended

¹ **Teleology.** *Philosophy.* The explanation of phenomena by the purpose they serve rather than by postulated causes. Keen students will want also to consult Harré (1970: 33-62- *teleiomorphs*) [The Editor].

lectures once a week, continuing over a period of three years. Starting in 1972, the lectures were accompanied by occasional tutorials with Harré where I could ask questions, test ideas, etc.

In 1971, the substance of Harré's lectures consisted of the chapters of a book he was writing with Peter Secord: *The Explanation of Social Behaviour* (1972). In fact, I soon realized that the essay printed in *JASO* appears in slightly modified form in the *Explanation* (see pp. 19-25). One of the most important ideas for semasiology in this book reads thus:

A person is an active agent in much of his social life. He is, if you like, the efficient cause of his own actions. He monitors his performances and controls the manner in which he presents himself to others. He takes care of the meaning of his acts.

Action of this kind has significance and meaning, and it occurs in a social and not a physiological context. It is inextricably bound up with the nature and limits of language and with the fabric of society. *There is no way of reducing action to movement, and so of setting it within a physiological context. To try to do so is to transform what was action into something else* (italics added). Compare the following pairs of statements:

- 1a. His arm extended straight out through the car window.
- 1b. He signalled a left turn.
- 2a. Her arm moved rapidly forward and made contact with his face.
- 2b. She slapped him angrily.

These examples, though trivial in themselves, make clear that *when we describe actions in terms of movement, we lose the real significance of the action as a part of human social life* (italics added). The legacy of behaviourism is such that ... [we] have too often failed to focus on human action in devising experimental studies and empirical investigations, and concentrated instead on the sounds or movements which are merely the vehicles of action. ... The adoption of the concept of action, rather than movement, as the basic empirical concept, carries with it another idea that is very important in the search for appropriate forms of explanation for social phenomena. This is the idea that a person often does things for a reason. ... In many contexts, a man's action is adequately explained by references to his reason or reasons for doing it. And this may direct us to quite a different structuring of reality from that revealed by the application of the concept of cause, in its mechanistic signification. It directs us to consider such items as the plan according to which the man acted, or the impression he was trying to create and so on (Harré and Secord 1972: 39-40).

I could easily handle the idea that a dancer (a signer, a martial artist) is an active agent -- an "efficient cause" of his or her own actions. What I couldn't handle (then or now) is the idea that human movement is meaningless -- or, at best, any semantic content is merely 'window-dressing' somehow grafted on to 'real' movement which consists of reductionist physiological notions of what human movement amounts to. Think about it: is a dancer's dance or a signer's language simply a response to some stimulus, *sans* intention, *sans* reasons, or much of anything else human? Of what value is 'scientific objectivity' if it means cutting away everything human in order to achieve it?

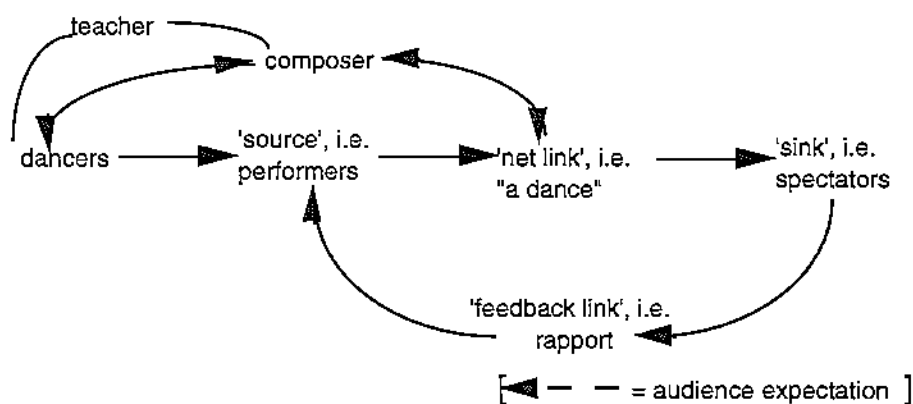
It is tempting to go through the *Explanation*, dealing with all of the ideas contained therein -- I filled two notebooks during the first year I attended Harré's lectures -- but in the end, I think going over these would not be fruit-

ful for present-day students. Suffice it to say that apart from discoveries of the kind I've so far discussed, from 1970-72 there were difficulties.² For example, everything Harré and Secord say in the *Explanation* is tailor-made, not for social anthropology, but for social *psychology*: the method ('Ethogenics') advocated in the book is for social psychologists. Important though the lectures and book were for a beginning, I was later to move away from Ethogenics to Harré's "nature, powers and capacities" arguments, subsequently published in Harré and Madden (1975) -- the same year I completed the doctoral thesis (Williams 1975).

The biggest difficulty pertained to the mathematics involved: I was not convinced that Harré's choice of "Systems Theory -- the mathematics of the New Paradigm" (p. 127) for Ethogenics was appropriate for an accurate meta-description of the human expressive body and its moves.

Before going on, it must be emphasized that we are not working towards mathematical systems theory nor trying to appropriate the intellectual respectability of that theory by setting up what may appear to be some kind of 'block diagram' ... The conceptualization of the relationship between dancers and spectators is not meant to be a control diagram. ... [M]athematical equations cannot be written in connection with [the relationship] to show various 'rate variables', etc. with regard to theater and dance (Williams 1972: 228-29).

The disclaimer was made in an argument about the *continuity* that exists between performers and spectators in or out of theaters.³ I claim that neither stimulus-response theory nor communication theory tell us anything about the *intersubjective processes* that occur between spectator and performer (see Williams 1995: 44-81). To illustrate my case that changes of actions and understanding are *contingent on the outcome of relational, interactional situations mediated by a symbolic system*, I used the concepts of 'source', 'net links', 'sink' and 'positive feedback', i.e.



"H₁ impinges on H₂ in such a way that H₁ or H₂ or both, are altered."

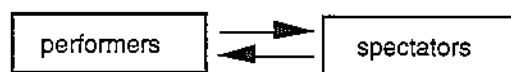
(N.B. 'H' = Human being)

² Often, the road was anything but smooth. I feel constrained to make a point of this, because in the telling, it may seem as if everything was straightforward and easy-going.

³ The feature of continuity is discussed in further detail beginning on p. 134.

All of the borrowed terms from systems theory are important, but the notion of 'feedback' is most important for the reason Harré points out: "[M]odels ... must contain some form of 'feedback', by which the various orders of monitoring of performance can be achieved" (p. 127).

I offered the diagrammed representation in contrast to a much simpler formulation expressing the *lack of continuity* that communications theory and stimulus-response theory provide:



The 'Black Box' diagram, where there is a blank, inexplicable void between H_1 and H_2 "full of sound and fury, signifying nothing."

In the 'Old Paradigm' most of what happens to performers and spectators (even speakers and hearers) is unexplainable because the 'boxes' are sealed. Science (in terms of the Old Paradigm) can only study the boxes (i.e. human bodies). In the "black box" diagram, *there is no distinction made* between, say, dancer and dance or between signer and language. This distinction is critical, just as the Saussurean distinction between *la langue* and *la parole* is crucial.

"What?" I asked myself, "is the equivalent of *la langue* and *la parole* with regard to dancing?"

Initially, I reasoned that '*the dance*' is *not* the product of any one person, although '*a dance*' may be. 'A dance' is a result of a structured system of moves (*la langue*). 'Dancing' is an individual act; the individual 'utterances' of movements constitute *la parole*. Dancers are at liberty to choose their idioms of dance movements within their own society (or that of another), just as they may choose what they will individually do *with* the idiom or *in* it, *but they cannot exceed the limitations of the theoretically possible movements of their bodies* -- nor can they change a whole idiom of dancing, although some one (or a small group) of them may well be innovators.⁴ Likewise, choreographers cannot change an entire society's notions of the values of up and down (U/D), front and back (F/B), right and left (R/L) and inside and outside (I/O), although they, too, may well be innovators. Both dancer and choreographer are constrained by the spatial and sociolinguistic fields in which they operate, but I now digress.

The Issue of Continuity

Momentarily, we must return to the problem of mathematics: if the maths of Systems Theory was inappropriate for the kind of theoretical structures I envisioned for the dance and human movement, then what *was* appropriate? I was encouraged to read Leach's *Rethinking Anthropology* (1961) in

⁴ They may, like Martha Graham, develop their own idiom and the technique to support it (Hart-Johnson 1997).

1970. Ardener refers to this work in a short essay entitled *Galileo and the Topological Space*:

Item 1: Its title *Rethinking Anthropology* was of striking symbolic value: the date of its publication, or perhaps the earlier date of the public address (1959) upon which it was based, mark in retrospect a boundary time between the immediate post-Malinowskian period in British social anthropology, and that phase (however it be characterized) in which it is now ... The comparison of the state of the social sciences with that of the natural sciences at some earlier period has become commonplace. More precisely, there has been the expectation of a revolution in which a figure of the stature of one of the great innovators will appear: 'we are told this revolution has not yet taken place in the social sciences, or at least it is only now in process of taking place. Perhaps social science has not yet found its Newton but the conditions are being created in which such a genius could arise' (Winch 1958: 1, cited in Ardener 1970: 125).

Item 2: Kurt Lewin was responsible for the first important discussion of topology in relation to social studies so it is worth citing him at some length. His major work was the *Principles of Topological Psychology* (1936). Elsewhere he has this to say about the concept of the topological space in psychology and sociology: Psychology has to deal with a multitude of coexisting facts which are interrelated and have a relative position to each other; in mathematical terms, it has to deal with a "space."

Mathematics knows a variety of different types of spaces. It is an empirical question as to what kind of geometry is best suited to represent the dynamic interdependence of that realm of facts which is treated in a particular science. Since Einstein it has been known that Euclidean geometry, which previously was the only geometry applied in physics, is not best fitted for representing the empirical physical space. For psychology, a recently developed non-quantitative geometry, called 'topology', can be used satisfactorily in dealing with problems of structure and position in a psychological field. This space permits representation of the position inside or outside of a certain region, the relation between parts and whole, and a great number of structural characteristics. All of this is done in a mathematically exact way but does not presuppose the quantitative determination of size, which is generally not possible in a psychological field. . . . (Lewin 1952[1939]: 150-51).

Ardener said much more, but enough has been cited for readers to understand that lively discussion of these issues occurred at the time.

Lévi-Strauss, in the same essay in which he specifically uses Lewin's 'Galilean' concept (1964[1953]: 283) refers to topology as one of the fields in which it has been possible "to develop a rigorous approach to problems which do not admit of a metrical solution" (1953: 532 and 1963: 290, cited in Ardener 1970: 129).

Regarding Leach's ideas, Ardener remarks: "Leach's presentation of topology through the rubber-sheet analogy was possibly the more evocative one to use to introduce the matter to a group of functionalist anthropologists in 1959" (Ardener 1970: 129), and finally, a few of his concluding remarks are relevant:

The formal systems of science and the images of science seem to form co-existent and interrelated semiotics. The search for a new synthesis, and for a non-mensurational view of systematic relationships, could be apprehended only symbolically in the 'fifties by most social anthropologists ... Leach's paper, as he no doubt would be the first to agree, is brilliant myth rather than mathematics. Yet the great interest of mathematicians in

topology is itself part of the general intellectual movement of our time, of which the structuralist or 'new-anthropological' trends in social anthropology are another expression. Topology was for Leach as the phoneme was for Lévi-Strauss - something good to think with (Ardener 1970: 130).

Such was the climate of thinking into which I entered at the Institute of Social Anthropology in 1970. As things turned out, I did not use topological spaces for reasons explained in Volume II of the D. Phil. Thesis (1975) in connection with the analysis of the Tridentine Mass:

The important contribution which acquaintance with topological ideas made to the study of action sign systems is connected with 'distance' ... one of the [useful] characteristics of a topological space is that the existence of a metric need not be assumed for it ... In a metric space, continuity depends upon distance. In the case of the Mass, we are at the outset dealing with a structured, semantic space in which continuity *does not* depend upon the function of distance either in time or in space (Williams 1975: 89 -Vol. II; also see Williams 1994: 64-65).

In the end, I only used certain lines of reasoning and suggestive ideas from the mathematical language of topology. I re-interpreted topological ideas so that they were consistent with groups and sets and the rest of the theoretical apparatus that informs semasiology. Thus it is important to know that

[H]istorically, the subject of topology arose because of some abstruse problems and counter-examples in calculus which displayed difficulty with the intuitive notion of continuity of functions with which Newton, Cauchy and others had worked for many years. These counter-examples took a long time to turn up and much of the actual vocabulary of topology is in response to these problems. The language does not, therefore, provide the best framework for the research in hand ... for it cannot compare in power and flexibility with group and set theoretical terms. *The reason for this turns around the notion of continuity* ... The continuity involved [in topology] is mainly the continuity of *fixed points*, as in the "rubber-sheet" cases or as in a projection, say, of a [photographic] slide of some *situ* which is then projected onto a draped piece of cloth which hangs in folds.⁵ It would not be going too far to say that one runs into the same problems here as one might if one were restricted to a geometry of *planes*. As always, it is [multi-dimensionality] which haunts us in considering human structured spaces. (Williams 1975: 89 - also see Williams 1994: 76-77, Note #5).

The Introductory Essay: *Social Anthropology and Language* (Excerpts from Ardener -- 1971)

At the beginning of the D. Phil. Thesis, I say, "Finally I should like to acknowledge my greatest debt of gratitude, owed to my tutor, Mr. Edwin Ardener, of St. John's College, Oxford, who has maintained unflagging interest in this research since it began and without whose patience, subtlety, wit, wisdom and concern, it could never have been written" -- a statement of

⁵ Topological spaces were essentially two-dimensional and they could not accommodate the feature of *agency*. They were incompatible with notions of human beings as efficient causes of their own actions.