DESIGN: SCIENCE: METHOD

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WHY DESIGN RESEARCH?

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This paper considers the nature of scientific knowledge and of scientific research NOT as a paradigm for design but, conversely, as a design activity in which particular (although not necessarily articulate) restrictions are allowed to operate. As such, considerations of science as design have a reflexive character - design is designed - and science/design is subject to all the problems that face such self-referential systems. Recent thought on self-reference is consequently brought in to enlighten understandings of science/design.

Science, it is therefore argued, may not be used as the yardstick against which to measure design. If anything the converse is the case. And research into design should be considered as providing a paradigm for science.

INTRODUCTION

A large part of this paper could be seen as a paper on the philosophy of science. What it intends addressing is the nature of research. Contrary to Bruch Archer (1979), I do not see there being a need for a new and special area of design research: rather, I think that what is called for is a modicum of honesty on the part of the research community.

Put in a nutshell, what I think is this: research, itself, is a design activity. It is a far cry from the popularised misrepresentations of the scientific paper in the learned journal, as Medawar (1963) has shown us. And it is only because of the fraud perpetrated through this form that some people feel the need to argue the specialness of design research, while other people refuse to fund it. Design is a basic human activity, and is also the basic activity in scientific research. So that while I agree (Glanville (1980b)) with Archer about its specialness, I no longer agree about its isolation.

What is being expressed in the title of this paper is not what perhaps appears at first glance. What I want to get at is not so much why we must have a field called "design research" but rather why research must be considered as design. In fact, put this way round, what I'm getting at then supports Archer's position perhaps even more strongly than his own arguments: if research (and experimentation) is design, there can be no excuse for not funding design research! But it also gives design research a rather special position, for, like the philosophy of science, which tries to analyse scientific thinking scientifically, linguistics, which tries to talk about language in language, or cybernetics, which feeds back information about feedback (only a cybernetician should be allowed to talk about his own field in such a jesting manner), design research is, in fact, research into research: a reflexive field of study which will need to learn from and recognise within itself all the theoretical devices developed in those other fields to resolve the circularities of reflexivity and to admit the associated problems of self-reference vis-a-vis meta-levels.

Thus the title of this paper expands:

Why Design Research?
Why Design Design Research?
Why Design Research Research?
Why Design Design Research Research?
-or-
Why not design research - What other options do we have?
-and, of course -
Why Research Design?
etc.

RESEARCH AND EXPERIMENT

The notion of research that I want to attack is not the sophisticated and sensitive one that a philosopher of science, in part because he is interested in theories etc., puts forward. It is the naive belief that science is conventionally predictable and logical activity (the problem is that of induction), and that experiments are step-by-step, causally powered routines.

In applying for a research grant, as any applicant knows, the most important requirements are to explain exactly what you will do, what you will analyse (and which statistical procedures you will use), what the results will be and how they will be useful. These are absurd requirements. Not only is the association of usefulness with knowledge quite arbitrary, it is also banal in the sense that if a new piece of knowledge is to appear, it may be that the insight resulting in fact initiates a chain of new insights that produce all sorts of novelty which is useful. But behind this false utilitarianism there lies another serious misrepresentation. If what is being carried out is really an experiment involving testing and transformation - as opposed to, e.g., meditation - only then the result can only be predicted as a hunch - a potential re-validation of some theory. Then, if the "wrong" result (or even no result) emanates, the experiment must be changed, to find one that gives a result that is acceptable. Now, say that the experiment to be carried out is a complex one in several stages, and something goes wrong early on: several later stages will almost certainly have to be changed, in a manner whereby each depends on the others. (This is a broad characterisation shared by many and perhaps most cogently stated in Lakatos (1969)).

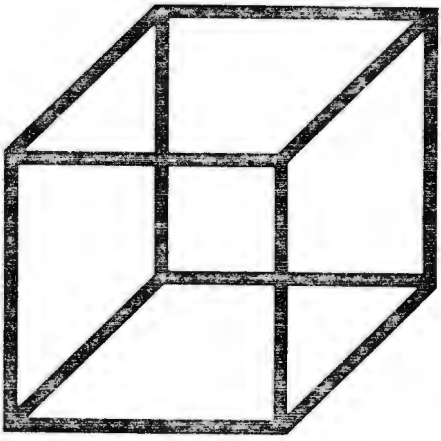
What I'm getting at here is a Kuhnian point (Kuhn (1970)) of the sociological and psychological aspects of science, how we think of experiments. We tend to think of them, unless we are very careful, as they are written up, which, of course, is coloured by the appalling characature of science which we learn in our school experimental apprenticeship: "method; observations; conclusions." Any normal account of an experiment tends to present it as a logical step in the slow development and refinement of the (predetermined and unchanging) corpus of knowledge known as science. (This is exactly what leads to the familiar "problem of induction", the problematic hub of all philosophies of science that assume this inevitability, see.e.g. Chalmers (1978)*.) But this fails to take into account what actually happens in terms of the actions of the experimenter and the effect the new knowledge may have on the corpus.

Only in very rare cases, which I consider as specially restricted and which are usually either very simple or very technological, is an experiment set up without an idea of the outcome to be tested being held in mind, and without some part of the experimental procedure needing to be changed because it doesn't quite work. The actions of an experimenter, from his initial idea of what he might do to the creation of some final, acceptable, working output are no more naively linear than are those of traditional designers - and every attempt to account for and create design processes that function in such a linear, "problem solving" (i.e. simple causalist and flow chartist) manner have, as we have seen, lead to sterility. The reasons for

*See also a major part of Nidditch's (1968) excellent collection of papers.

this have been widely expounded and are to do with such non-deterministic concepts as chance and randomness, (Jones, 1979), creativity, learning (Glanville, Pedretti & Jackson (1979), wholism, circularity and feedback, (Glanville, 1979c, 1979d). Such discoveries and arguments made in the field of Design Research do not need elaboration here! In fact, it is this very activity, design, that lies behind the general ability to create the possibility of new outputs, be they buildings, spoons or knowledge from scientific experiment. This involves the active, creative participation of the agent involved, a fact that is now accepted by virtually every philosopher of science under the title "hypothesis dependency".

(A nice example, probably known to most readers, is the perceptual psychology advocated by Gregory (1970), where he discusses the interpretation of ambiguous figures e.g. the Necker Cube -



in which the image "reverses" in such terms. I have noticed, incidentally, that I have difficulty creating the reversal, no doubt due to the dominance of one hypothesis that comes from an architectural training in the axonometric. Such a cube has, in fact been made.

Furthermore, the creation of fields of scientific knowledge can be looked at in the same way, as being designs. In fact, they are probably closely analogous to cities in both scope, scale, sociality, anonymity, time-span and conservertism (It is tempting to use a building as a simplification of the city, and hence to develop an analogy for science, although it must be remembered that it is a simplification, particularly in that it can be seen as terminating. But this endeavour must await another paper).

The discovery of a new piece of scientific knowledge does not exist in isolation. It stimulates the attempt to create new knowledge (in order to test and refine the knowledge already existing) and affects the local knowledge around it, in much the way that the creation of a new building may "upgrade" an area and cause other new buildings to occur in the city.

This is to say that the purpose of scientific experiment is to produce output which not only increases the scope of science, but also tests the already existing knowledge, and, where it is found lacking, requires its modification: which, of course circulates right through the whole corpus requiring a constant change, just as, in the design process, the mis-fit of one element may require the change in a whole string of decisions. In the extreme, of course, enormous changes may have to be made, completely altering large chunks, or even the whole face of science as we have previously known it, just as, of course, building or social forces may create massive changes in a city. Again, philosophers of science have worked out elegant characterisations of these processes - the falsificationist's Conjectures and Refutations (Popper (1969)), refined into Research Programmes (Lakatos (1969))(and by Roy Landau(1981)considered as a far more general methodology particularly applicable to design), and Kuhn's (1970) Scientific Revolutions. It would be interesting to consider theories of design processes applied as a new theoretical basis in the philosophy of science!

Characteristic of such a way of carrying out research are various behaviours normally considered rather improper and un-scientific, but constantly experienced by honest designers. They include post-rationalisation of whatever happened (which scientists practise in a highly restricted form through the learned paper format); attempts to explain away and brush under the carpet failures by claiming they are not basically relevant (much as scientists incompletely interpret - and, sometimes, as in the allegations against Sir Cyril Burt, actually fix - results, an experience surely every schoolchild repeating a famous experiment and getting a "wrong"*answer will remember); working on hunches and sudden insights (ask a scientist how an idea came to him and his human normalness will become apparent); and the admission of personal learning arising from mistakes. In fact, it is these characteristics that, by and large, are evoked in order to explain the workings of the scientist's mind: "I had a sudden idea so I set about setting up an argument to support it and designing an experiment that would test it. The experiment didn't work, but I didn't give up. I explained away the incorrect data as resulting from poor experimental design, and, learning from my mistakes, I redesigned the experiment. The new experiment gave me the result I wanted at 5% significance, which I judged to be proof of my point." Compare with the standard description!

What I have discussed above, whether in terms of the output of individual experiments or the growth of scientific knowledge, is research. And I have tried to show that experiments are designed and interpreted, and that the inclusion of the output of an experiment into the corpus of scientific knowledge is also a matter of design, and the redesign of the corpus. Thus I am claiming that research is a design activity in just the way any normal designer understands, that research involves and needs design, just as research-as-design needs research. Design Research is, in effect, almost indistinguishable from (at least major aspects of) research into research.

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*What does "wrong" mean, under these circumstances? It means that, according to the knowledge we have it doesn't fit our views (in hypotheses) and must therefore be incorrect. But it may be that view is wrong.

It is in this sense that design research is a reflexive subject in the manner of philosophy of science, linguistics and cybernetics as I pointed out earlier. So, if design has lessons and possibly even provides a paradigm for science, - or at least for scientific research - how might design learn from these subjects, and be reflected in them?

The most important characteristic of these subjects is that they examine themselves in their own terms. For instance, the linguist discusses (natural) language in terms of (natural) language. This highlights a particular "problem" - the problem of levels. In the classical paradigm, the something talked about is on one level and the something in terms of which it is talked about is on another, "meta" level. Much work has gone into exploring the classical paradigm, showing that it is impossible to describe something, with certainty, both completely and consistently in its own terms (Gödel's (1931) Theorum). This gives rise to the "problem" of levels (also known as the "problem" of self-reference). Some subjects, for instance the three mentioned, do and must talk about themselves in their own terms - that is, their meta-levels are the same as their levels (Pedretti (1980)). This leads to the difficulty that they are to be considered as incompletely and/or inconsistently described, in terms of the classical paradigm. But must this be so?

At the extreme limit, a system which is dynamically stable and distinct (such as a living entity) - that is, something which remains independent, must contain its own description, so that it can reproduce itself without reference to others (Maturana (1970) and Varela (1980)). In this case, the level and the meta-level, the thing and its description, together are inseparable constituents of the thing. Thus, it must be that the classical paradigm is inadequate, for it rejects such self-referential systems, which nevertheless clearly exist: as fields of study, as animals, and as, as some of those including myself, who are developing the theoretical consequences of such systems claim, everything about which we can know and think. (Glanville (1975, 1978, 1980a, 1980d), von Foerster (1976)).

There are two ways out of this. The first, conservative way is to attempt to patch-up the classical paradigm in order to reduce the problem. Such attempts (which, of course, follow the design principle of scientific knowledge already shown as the constant modification of the corpus of scientific knowledge) are, however, never likely to succeed completely since they are in essence based on a way of structuring knowledge which specifically excludes self-reference. Indeed, Gödel, who provided the proof of this, worked until his dying day to sort out how human intelligence, which he did not believe could be encompassed in the paradigm, could be accounted for - and failed*! This approach will admit of highly specialised special cases of partial, or limited self-reference (e.g. Loefgren (1968)), but admits it cannot account for complete self-reference and therefore either shrugs its shoulders at, turns it back on it, or says it's really a problem.

The second, radical approach takes the position that self-reference is obvious and must therefore be accepted**, (e.g. Glanville, (1975, 1978, 1980a, 1980d). The problem it then faces is to encompass, in a meaningful way, the sort of level/meta

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*Thus Gödel considered that machine intelligence, necessarily bound by the classical paradigm, could never, in principle, emulate human intelligence. In this view he is not alone.

** An extended debate on these issues occurs in the privately circulated proceedings on the Workshop Conferences on Self-reference (Pedretti, 1980).

level reference of the classical scientific paradigm: that is, to redesign the whole of scientific knowledge to encompass not only the classical view (possibly modified) but also those things which are currently excluded.

This is a call for a scientific revolution, and a lovely exemplification of the operation of Occam's Razor (Glanville, 1980d). It is, in its own way, as profound a revolution as that of Einstein's adumbration of Newton's mechanics, with as many ramifications (as discussed in Chalmers (1978)). And, of course, design and design research are necessarily, as I have argued, in the vanguard of this programme.

In taking this second view, certain basic tenets of the classical paradigm have, of course, to be modified: for instance, the concept of level. Levels in description and in knowledge are assumed to be basic, in the classical paradigm (e.g. Pedretti & Glanville (1980) Glanville (1980c)). But, if self-referential systems, which do not have such levels,are basic, then levels cannot be. Are there, then, any levels at all? Or is the whole of knowledge a sea of little isolated bits, unrelated (for relationships other than identity immediately generate levels) - a deceit. This would obviously be an awful thing were it so,for it would mean that scientific knowledge was an illusion (and, for that matter, the essential cognitive ability to recognise things) - which is as counter-intuitive as the refusal to accept self-reference. It also runs counter to the argument used in support of the radical view, that within it the classical paradigm may survive as a special, if limited, case.

The solution to this lies in remembering how it is that levels come into being. There may or may not be levels in nature - in the real world - but this we may not know because we can never see the real world cold, without interpretation, without hypotheses. The question of levels in nature is, anyhow, irrelevant. Science (and how often do we forget this in our oversimplifications) is a corpus of knowledge, and a corpus of knowledge requires agents to know it. It is not constituted of cold facts, but of working hypotheses. The corpus of knowledge does not, a priori, exist; it is constructed. The relationships in it have to be made through the act of relating, and they have to be expressed linguistically, and stabilised through shared interpretation in shared language (as demonstrated by Pedretti (1978)).

All this is done by the scientist, through reference to self-referential systems as others. And it can easily be shown that the sort of mechanisms that must be assumed for self-referential systems to be observable to others (without which assumption there would be nothing left to talk about!) permit the making of such relationships and thus of levels (Glanville (1978)).

This, of course, re-affirms the centrality of the scientist and his hypotheses in making science - as a design act informed and supported by the very nature of design research as a reflexive system - and it reinforces the concept of science, of levels, of concept formation as being a public endeavour, (as Chalmers (1978) and Kuhn (1970) insist).

But it also provides the theoretical basis for the observer in any experiment - or the designer in any design - as being involved in a circular, feedback process in which the observer's description and the experimental arrangement's behaviour interact and modify each other until they are in apparent agreement, allowing predictions to be made (inductively) without there being the need for any recourse to "truth" (that word particularly inappropriate to design), hence removing much of the "problem" of induction, and allowing a similar circularity to exist between the experimenter, the experiment, the experimental medium and the representational medium (i.e. how to say something) (Glanville (1979d)).

And it provides a neat solution to the problem of absolute knowledge vs basic ignorance. I think it is beyond argument, nowadays, that we only know through interpretation - as has been argued throughout. The problem here is how do we gain and hence <u>have</u> knowledge, in spite of our necessary basic ignorance? And the answer lies in the difference between self and other: we have no absolute know-

ledge outside the self, but we can have some knowledge, through the "design/designer circularity" of others, in which relationships are developed between the various others, which are then assembled and represented in various levels. Any designer knows, of course, that design is a matter of few absolutes (except socially applied absolutes such as regulations), and a lot of interrelated compromises which lead to some new "whole thing" (Glanville (1977)).

Finally, it is worth mentioning in passing a few further consequences of this point of view (which may be pursued in the literature). Designers know, from experience, that designing is a matter of keeping several things in mind (both consciously and sub-consciously) at once, and gradually getting them to fit together (often in quite surprising ways). Theories handling self-referential systems require a similar performance. Rather than handling determinate elements in sequence, they attempt to synchronise several distinct elements to occur together (Glanville (1978), Pask (1978)). The implications for this in computing terms, and CAD, are particularly important. Furthermore, the conventional views of representation (as a sort of coding) have to be changed to be both more subjective and more conversational again something designers recognise in creating and discussing (both verbally and non-verbally) their work. This whole field is being exhaustively researched by Pedretti (in progress), who has also worked, with me, on aspects of elicitation and representation of spatial perception and architectural design (Glanville, Pedretti & Jackson (1979), Glanville (1979a)). Using the self-referential paradigm we can, in fact, relate coding, communication, conversation, consciousness and intelligence as being manifested behaviour of groupings of synchronised self-referential systems working together. Finally, the ideas of inside and out, of boundaries, of inherent properties become modified in such a way that properties of things are seen as being inserted by the designer, rather than being present as inherent in the first instance (Glanville (1979a)).

It can thus be seen that the theoretical developments with regard to reflexive systems are not only fuelled by the nature of design research, but also account clearly for much of design as an activity and experience.

WHY DESIGN RESEARCH?

There remains one point to be clarified: the question of the title. "Why Design Research?" Why should we research into research, design design, and so on?

The answer to this is, I think, rather shocking. There is, in principle, no reason at all. It is merely a matter of faith. The faith is rather that of the patient attending psychotherapy, of the person who believes that by becoming more aware of himself he will learn to perform better. This is a common educational belief, one to which I subscribe, but which can never be conclusively proved and which, in as far as my knowledge goes, has never yet been even circumstantially demonstrated. (For instance, some people appear to benefit from psychotherapy while others don't, but who knows if those who benefited might not have done so without therapy, or if those who didn't would have, if left alone?). This, of course, parallels the whole business of usefulness and knowledge - we may know, but that does not necessarily mean our knowledge is useful, any more than knowing about how we perform means we can improve our performance.

Nevertheless, this is a position widely held in our Western Culture, that knowledge brings, as it were, power, and that through this power we can control, manipulate, change things (e.g. espionage). In a sense, this is a justification for science - and speaking materialistically we may claim that the technological embodiments of the advances made by science do indeed provide some justification for the pursuit. Then, since scientific knowledge comes through the medium of experimental research, research into such research would be seen to be not only justifiable but highly desirable since it should lead to improvement in the activity of research. Certainly, given virtually the only available experimental training of today, which designers will recognise as the articling of an apprentice to an attelier, one wonders whether a new and more "efficient" way of teaching experimenters might not emerge, as we believe (but are we right to do so?) has happened with designers. But it is also

conceivable that doing research on research, design on design, could give us a scale for comparison: for the body of research (for example) could be compared to the generalised view of research which might act as a (temporary?) norm.

Thus, given the (hard to justify but generally held) view that knowledge improves things, if our knowledge derives from research, it is surely sensible to acquire knowledge of research in order to improve research. Since research is a design activity, this, of course, means knowledge of design and design research.

Under these circumstances, the beautiful activity that is science will no longer be seen as mechanistic, except in retrospect. It will truly be understood honestly, as a great creative and social design activity, one of the true social arts. And its paradigm will be recognised as being design.

Thus design will take its true place as the basis for the activities that create scientific (as well as other) knowledge, and will no longer be sneeringly and trivially dismissed by those who adjudicate without creating, and who are fooled into believing that science is as she is writ. There will be no need for a special area of design research, for all research will be seen to be part of design research, with that which we call, now, design research being the most basic of all.

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