Re-searching Design and Designing Research[1]

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Prologue[3]

When Design Research began, say in the 1960s, the eventual success of science was assumed. Already, at the notorious 1956 Oxford Conference, architectural education in the UK (and its sphere of influence) accepted architecture was a second class subject: ie not properly scientific. Science (in actuality, technology) was seen as so successful that everything should be scientific: the philosopher's stone! Architects (a significant subdivision of designers) were determined to become scientific. The syllabus was changed and design science was invented. Even the Architectural Association School gave over a third of undergraduate time to design science. Prime Minister Wilson and his Government declared the "White Heat of the Technological Revolution."

It was no wonder design was seen not as a discipline in its own right. Design was deficient: effectively, a defective science. It was flawed. But these flaws could be fixed by the proper application of scientific methods

It did not matter that science as done was not as described in both scientific publications and in the philosophy of science or that the philosophers were debunking these understandings. Design should become scientific, research (scientific) should be done. The results would speak for themselves. The problems of design would be solved, given the application of proper scientific methods. Efforts were made to do this, and some had effect (for instance, early CAD packages constructed around hospital design had a certain success: hospitals may, under certain circumstances, be considered as machines, although nowadays there is a backlash against this view: see Blair 1995).

Research was what was needed. Proper scientific research (research was identified with science) would yield the secrets of the designer, allowing us unsentimentally to find the right answer to problems. Research was central to Science. Research was Science. In shameful contrast, Design was not Scientific. Design should be Scientific. Design therefore needed Research.[4] Since Research should be Scientific, Design Research should be Scientific. And then Design, itself, would be Scientific.

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[1] This paper is developed from my earlier paper, "Why Design Research" (Glanville 1980).

[2] This paper was written while Visiting Fellow at the School of Design, Hong Kong Polytechnic University: thanks to them for that most valuable resource, time.

[3] I accept my account may appear inaccurate and oversimplified. But I believe the argument is worth making.

[4] It also needed theory, for similar reasons. This need continues today, with the consequent import of endless new theoretical structures from outside design itself.

Research gave Science its precedence, even today indicated in the growth in evidence-based studies.

In this paper, I look at research, both experimental and theoretical, as done compared with as reported, and compare this to what is central to the act of design (as I understand it) to throw light on the relationship that could hold between research and design. Thus, the balance may be restored so design is accorded what I consider its proper position. Finally, I consider whether there is an area of knowledge which has already achieved, within its own competence, what design should aim to achieve for itself.[5]

So the purpose of this paper is to construct an argument that gives design back its rightful place in research: that is, shows research to be a (restricted) design act, rather than design being a inadequate research.[6]

Part I: the World of Research

Research

What is the purpose of research? What do we aim to achieve through it?

Research is an undertaking by which we aim to increase our knowledge (of the world).[7] The word (and the word design) is both noun and verb in English. In this paper, I am specially interested in research and design as activities, so generally my use is of the verb.

Research is usually understood to produce extendable and testable social knowledge. A characteristic (Swanson 1997) is that we take our knowledge, extend and test it until it "breaks" and then rebuild it. Thus we extend what we know. The circularity and failure (leading to "rebirth") is central to the research undertaking.

What research produces—the outcome—should be stable to be useful in making knowledge, ie the outcome should be repeatable unambiguous (stable in interpretation).

It should also be coherent: the outcome should fit with (occasionally cause reconsideration of) what is already known. Research is concerned with both individual chunks of knowledge, and their assembly into larger structures. It is important that the chunks stick together within the larger structures. This implies that coherence is deeply connected with consistency: the chunks must be consistent with eachother within the structures.[8]

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[5] The reader, anxious to know how I use the word design may look at the section "What is Design".

[6] I lived in this intellectual environment and believed its simplifications. My (student) sketch books are full of Venn diagrams and directed graphs, rather than sketches of sensitive corners of proposed buildings. I had second chances to study, through teaching and through higher degrees. Otherwise I might still think this way.

[7] I prefer the word knowing to knowledge, because knowing requires an agent to know whereas knowledge appears to be knower-free. But, in this paper, I use knowledge to reduce pedantry: please remember, however, that it needs a knower.

[8] This is not the place for an argument about the relevance of such a view in a Post-Modern World. Whatever the relative truth of different philosophical positions, science continues to work and to be worked in more or less the manner described here. Arguments about interpretation, personal truth etc are close to my heart. As it

An important way of determining that our knowledge is consistent and repeatable (ie complete) is in predicting outcomes. When knowledge does this successfully, we extend our belief in it, leading to a science of conjectures. The fact that we are willing (in theory, at least) to test these conjectures to destruction leads to a science of refutations in Popper's manner (1969).

However, scientific research is not always carried out according to Popper's ideal, which is impossibly ambitious for mere humans. Kuhn (1970) argued from historical observations. He divided science into the revolutionary, which is distinct from the more mundane and technical tasks of conserving the status quo that he calls normal science. Lakatos (1969) has indicated the development around accepted theories of "protective belts" that repel the unconventional.

Science as practised is not the ideal Popper suggests. We may aim for, but are not likely to attain, Popper's ideals. This difference between science as portrayed and done is important.

Research is carried out in two main arenas. The first is experiment; the second, theory. I am concerned with the actuality of what happens, in contrast to the "official presentation."

All this happens against a background of assumptions, for instance that something has always happened does not mean it always will. But we assume that the likelihood of it continuing to happen increases the more it has already happened, until….[9]

Experiment

Experiments are the main means by which scientists extract knowledge of the world we inhabit.[10] They do this by radical simplification.

In the (idealised) scientific experiment we divide systems into distinct, isolated variables. We fix all but one of the variables, and change some factor we believe is influential in the behaviour of the system, realised in the free variable. Changing this factor, we observe any change in the behaviour of the system and attribute it to the response of that variable. We organise the "in and outputs" so that there appears to be a simple relationship, and we determine that this relationship is determined by the variable.[11]

We have devised methods (eg, statistics) for "faking" these conditions in complex systems where we cannot isolate variables, and/or where repeatability is unattainable.

I am sure the reader is familiar with the above picture. What is left out is the experimenter. Yet how could there be an experiment without an experimenter?

The answer (based on simple experience: of doing experiments, and of language use and what that implies for action) is straightforward. We cannot.

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happens, Cybernetics accommodates these arguments—see the last section.

[9] As Wittgenstein (1971) elegantly points out. This is the age-old "Problem" of Induction.

[10] I insist investigative actions must have active agents (I am a constructivist), which are, in the case of science, scientists (who are people). See the Black Box model referred to in the next footnote.

[11] The theory of the Black Box, which has the added advantage we can never talk about "truth" as a result of using it. See Glanville 1979.

The experimenter chooses to do the experiment and sets it up (including determining the variables).[12] The experimenter observes, and determines what the outcomes are. The experimenter carries out the actions.

The experimenter continues until the system begins to perform as desired or required (for instance, moves the light source/screen/lens to get an in-focus image). And the experimenter determines when enough has been done, ie, who breaks the circle.

The experimenter designs the experiment, and if it doesn't work (well enough, in his opinion) redesigns it. The experimenter forms the outcome and assembles different observations into a coherent whole (relating them together). The experimenter may then tie the outcome into theory, modifying that accordingly. Reacting to changes in knowledge resulting from the experiments, the experimenter may rerun the experiment, perhaps with a new arrangement of the variables, or in a different place, to check repeatibility (ie stability). He may make predictions (which always requires a rerun). The experimenter plays with all aspects of the experiment until it produces results of the type desired.

These actions of the experimenter are circular. As a result of their circularity, novelty (the unexpected) may be observed, leading to a rerun under changed circumstances.

There are circularities in setting up and doing the experiment, in valuing what is found and integrating it. There are circularities of repetition. The whole process is deeply embedded in circularity, particularly the greatest of all scientific circularities: the active involvement of the experimenter (the observer).

Account

The traditional relating of (scientific experimental) research is highly formalised—like a Russian Icon! Taught to all school children, this depiction is intended to emphasise certain epistemological claims.

So, observations are made in and of experiments. There is no agent, nobody does the observing. The experiment just comes into being (no one thinks of it or messes around until it works—ie, produces the desired type of result (whose desire?)). Results just appear, without anyone adjusting the experiment to gain the outcome required, such as moving optical elements to get a sharp focus, and only then taking measurements. Everything is automatic, a mechanism of great beauty and complexity, true in itself, beyond and not requiring human intervention (see Peter Medawar's charming and witty, "Is the Scientific Paper a Fraud?" 1963).

The undertaking is presented in a manner supporting the view that The Great Scientific Endeavour brings forth truths unsullied by human intervention, awaiting discovery in The Great Reality Out There. Nowadays, this position is unreasonable and untenable: it is hard to pursue the consequences to a logical conclusion, and take onboard the constructivist view that seems to emerge. In science, we have long been drilled otherwise.

[12] That the experimenter is influenced by social factors and epistemological outlook does not reduce his responsibility: he accepts these social factors and acts accordingly.

This manner of accounting is post-rationalisation: the tidy explanation after the event of what was, perhaps, a rather different experience.[13]

In this traditional view, there is a power, a "right". There are solutions to problems because everything fits together in the mechanism. Indeed, it is already fitted together in nature: thus the universe proceeds. Define the problem, execute proper procedures, and the resulting output from the scientific machine must be correct. This view's power is that it works—pretty well, within the assumptions of the framework it is to demonstrate. (This modification provides the basis for studies in Chaos. See Gleick 1987. Also, see the "Error Game" in Pask, Glanville and Robinson 1981) One could, therefore, assume research would lead to the "right" answer, at least until Popper (1969).

This is not the place for the interesting discussion of why we should no longer support this view (assuming it was ever valid).[14] We realise that the picture proposed by the traditional account is neither accurate nor credible—either about how we do research, or the response/output we can reasonably expect. The generally promulgated view held by which we account for our research through scientific experiment is inaccurate. (In fact, it is not nowadays held by major scientists: see, eg, Richard Feynman's "QED" (1985).)

Over the last 30 years, linguistic analysis indicates the aims of the scientific communication and the use of language indicating this have changed. Publications are no longer concerned with "the truth"; they communicate the author's (or the editor's) wish to join or remain in a group of fellow workers (Hunston 1993, Hyland 1997, Glanville, Forey and Sengupta, forthcoming). This finding holds for physicists as for social scientists. The first person has found expression again: papers are written by I's. Designers know they are not dealing with "the truth" except metaphysically, eg truth to materials (the Architectural Association's motto is "Design in Beauty, Build in Truth"): and they know there is no design without them, the designers.

We also nowadays understand that the description is not the experience; the explanation is not the actuality; prediction is not mechanism.[15]

Finally, we fail to mention the actual processes of writing/reporting—as above!

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[13] I argue from this base (Glanville forthcoming) that emergence is a misnomer. Emergence is observed in light of the processes from which emergence is taken to have occurred. And this can only be considered after the event, for the relationship between mechanism and its production can only be determined after the production has occurred, and become (metaphorically) visible to the observer; who may then talk of emergence. To talk of emergence as a property is a nonsense. It must be a post hoc attribution.

[14] My favourite reason (not necessarily the most powerful one) is the transcomputability resulting from Hans J Bremmermann's calculation (1962) of the computing potential of the earth. See Ashby (1964), and Glanville (1997a).

[15] For a rather nice account of the consistent effort not to notice this, and the results of finally realising it cannot be overlooked for ever, see the early parts of James Gleick's account of Chaos (Gleick 1987).

Theory

Theory is what turns the (collections of) observations we build into science. It may not make these collections science, but, if not sufficient, it is necessary. However, this paper is not primarily concerned with what makes science, but the role of theory in research.

In my understanding, theory in research has the following two roles.

> First, to combine, co-ordinate and simplify the findings of experiments by developing generalising concepts.
>
> Second, to examine these concepts in order to further clarify and develop them, reflecting back extended understandings into theory: and, by suggesting experiments that might be performed, into experiment for verification

The relationship between theory and experiment is essentially circular. They might be thought of as partners in a very slow conversation carried out over a very long time.

And the role of theory is to simplify, to generalise.

Theory from Experiment

The first aspect of theory, theory from experiment, involves pattern finding. Humans look for patterns. Piaget (1955) insists the child develops a view of the world as he/she becomes able to distinguish objects: that is, create constancy between separate perceptions on separate occasions ("object constancy").[16] Pattern finding, the making of one concept from many distinct perceptions, is an intensely human activity. Theories are patterns given widespread credence and accepted as accounting for a part of our experience.

Since, in Popper's characterisation, theories are not provable, they remain only temporarily valid, awaiting disproval. They fall into a category that includes Occam's Razor. The criterion—relative simplicity—of Occam's Razor is no more provable than "randomness" (see Chaitin (1975), Glanville (1977, 1981)). To assert something is random is to assert that no pattern has been found, yet. There is no absolute truth in simplicity: rather, there is convenience, coherence and consistency.[17] Occam's criterion can be neither proved (it is a matter of taste) nor properly tested (although it has intuitive validity and we like it to hold—it is the means by which, for example, Newton's universe is subsumed in Einstein's).

Why do we want to simplify?

To make the "continuum" of our experience de-finite, handleable within limited (finite) resources (see Ashby 1964, Glanville 1994, 1997a).

If we did not simplify by (constantly) making (constant) objects constant, we would never be able to recognise them: nor would we "cognise" "perceptions" as being of "objects". The

[16] George Spencer Brown's "Logic of Distinction" (1969) is based on this concept. My first PhD (Glanville 1975) was concerned with how, although we perceive differently we can still believe we see the same "Object".

[17] This is the problem facing those wishing to demonstrate absolute "scientific" certainty in, for instance, the non-transmission of BSE, or, more recently, of H5N2 (Hong Kong bird flu) to humans. Popper's point is that science attempts to disprove, so validity is temporary.

world we lived in would have no object, and we would not be able to conceive, let alone speak, of our own I's. To "cognise" would be beyond inconceivable. We would live in the continuum, the void: about which we cannot speak, for to speak is to distinguish and make objects. Nirvana. In a word, .

Nor could we generalise, finding similarities in behaviour and learning from repetition so we can venture the belief that because some (observed) behaviour of some object has always held, it will always hold (see footnotes 7 and 9)!

So strongly do we believe in such simplification that, when we find discrepancies, we explain them away as errors, rather than a demonstration that simplification necessarily omits something. By this device we maintain our theories.

Theory formalises the significance and necessity of pattern. Pattern gives us objects and recognisable behaviours, allowing us to predict, and risk living by our predictions.[18]

Prediction is a means for extending the range of our observations and the patterns we have constructed—of pre-forming our worlds. Living by the assertion that pattern X exists and because X has always happened it will always happen, we extend the range of application of the pattern to become a prediction, taking control of the future so we pursue certain courses of action.

Believing something is constant leads us to stop thinking about it: it becomes habit. For instance, if I have a route I regularly use, it forms (and severely limits) my actions and I treat it as causative. An accident (or similar) leads to confusion and loss of control. My chain of causes has been broken.

Similarly, we make simplifications forming the base of our science (and our personal knowledge (science—as in Kelly's Personal Scientist 1955)). We determine that objects fall to earth unless constrained and we generalise. Examining the generalisation, we extract a simple principle. We use the principle to cover other areas of observation: objects whirling around on strings do not fall to earth as long as they whirl fast enough, which we extend as an expanded understanding to the planets, as if whirling on invisible strings. There we move to Theory from Theory.

Theory from Theory (from Experiment)

The second type of theory is the examination of concepts to clarify (hence, develop) these concepts further, reflecting the extended understandings back on the theory and suggesting experiments to be carried out.

While science thought itself essentially empirical, there was always a theoretical area. In some accounts, mathematics (or logic—Russell and Whitehead 1927) is the queen of the sciences. Theoretical science abounds today. For instance, particle physics inhabits a universe

[18] I will examine the connection between how we think and what design is in another paper. But I believe this account indicates my belief: that design constitutes our way of thinking.

of theoretical discourse and is essentially theory driven.[19] Sometimes such areas return to experiment, but not always. Science depends not only on theory based in collecting and organising evidence (simplifying it to form patterns), but also on theory based in examining the consequences of that evidence and its simplification through the logical examination of, for instance, both a particular pattern, and patterns in general.[20]

In building theory from (and of) theory, we use the same devices we use to build theory: simplification, pattern finding. As well as the objects we have found, we treat relations and the patterns they are held to pertain to also as objects. Using the devices of theory on theory, we act self-referentially: and self-reference is, necessarily, circular. We make theory about theory just as we make theory: we find the pattern of pattern.

We use these understandings (devices)—simplification, pattern finding—to develop our understandings, especially how we understand these understandings (the understanding of understanding). Thus, our understandings help us develop our understandings, but also restrict them. When we find contradictions, we either modify or reject these understandings and start again: from the original, where necessary. Ie, we return to our initial simplifications.

This circular process is, I argue, a design process: of continuous modification and unification, the inclusion of more and more in a coherent whole; occasional re-start, extension and revolution; the increase in range and of simplification ("Less is More").

From our (re-)new(ed) understanding we suggest how experiment allows us to test our simplification through an interaction in which both we and the personal reality we make for ourselves find confirmation, extension and modification (and renewal).

Note how all of this requires an agent. We do it. It does not happen by itself. We do not even uncover: we make and we test, and where necessary, we modify. We are always present, as active agents. What we do is circular because that is the way we do it.

One form of doing theory from theory is criticism. This paper is a form of criticism, reconsidering theory from a position of (and as) theory. The act of reading (or writing) this paper is making theory from theory (about the theory from which theory is made!). So it is part of a design process: it reflects the theory (of science) back on itself. It focuses (simplifies) reflecting certain aspects, finding a pattern: of circularity and of the observer's involvement. In this paper, the criticism is essentially self-referential. But criticism not deriving from theory itself is, nevertheless, theory applied to theory, within a wider setting. For criticism involves an abstracting from theorising, to permit and encourage a (general) theoretical overview.

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[19] The machinations in constructing evidence from photographic "evidence" is astonishing. But not as astonishing as image enhancement creating patterns telling us "truths" in, for instance, space exploration!

[20] I do not necessarily mean formal, mathematical logic.

Part II: the World of Design

Reflex

I contend what I described in Part I of this paper is design, and is design at many levels. And, therefore, (scientific) research is a form of design—a specifically restricted form. If this is so, it is inappropriate to require design to be "scientific": for scientific research is a subset (a restricted form) of design, and we do not generally require the set of a subset to act as the sub subset to that subset any more than we require the basement of the building is its attic.

That (scientific) research is a hidden branch of design leads to peculiarities! It is strange an area for so long claiming the uncovering of truth as its purpose itself seems dishonest about what it does and how it does it.

To indicate (scientific) research is a variety of design as forcefully as possible, I shall explain what I mean by "design", reminding the reader how the qualities of that characterisation are found in my earlier description of (scientific) research.[21]

What is Design

There have been many answer to the question of what design is. The characterisation that is used in this paper concentrates on design as a means of exercising our creativity.[22]

Recapitulating, design is a word used in several ways which has, in English, the form both of noun and verb. In this paper, design is mainly thought of as a verb, indicating action. Central to the act of design is circularity. Here, in my view, creativity enters (Glanville 1980a, 1995a), which is at the centre of my interest. Other aspects (eg solving a stated problem), although often understood as crucial, are not, I maintain, central to the study of the design act, no matter how important. Problem solving is its own discipline. I am happy to leave it to those interested.[23]

I characterise design (after Pask 1969) as a conversation, usually held via a medium such a paper and pencil, with an other (either an "actual" other or oneself acting as an other) as the conversational partner. The word conversation is used in a recognisable and everyday manner.[24] (Pask eventually developed notions of the conversation into a highly refined technical theory of sophistication and some difficulty (Pask 1975, 1976, Glanville 1993, 1998b)).

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[21] I like to believe this holds for all research, because for research to be distinct from assertion requires validation: it is not enough to assemble a few ideas in whatever way we fancy; we must test these ideas (honestly and fairly) for consistency, correspondence with experience (reality), and communion. I shall not pursue this argument here.

[22] See Glanville 1980a, 1994, 1995a, 1997b, 1998c. This is my normal characterisation.

[23] Some postulate primitive problem solving as a first venture towards design. History is as much a construction as any other account. I do not deny problem solving and design co-incide. But I insist design takes a space of its own.

[24] A conversation is a circular form of communication, in which understandings are exchanged. In a conversation, participants build meanings through the conversational form, rather than trying to communicate a predetermined meaning through coding. In conversation, words do not hold meaning—we do. See Glanville 1995b.

Design-as-conversation will be familiar from the doodle on the back from the envelope upwards. I believe the value of the doodle is an instance of creativity firing the doodler's enthusiasm, personal research and commitment.

Creativity may also be found elsewhere. But this circular process is certainly one in which novelty—a distinguishing feature of design and so typical of creativity—can be generated.[25]

Design and Research

(Scientific) research (whether experiment or theory) is a design activity. We design experiments, but we also act as designers in how we act in these experiments. We design the experiences and objects we find through experiment by finding commonalities (simplification): and we design how we assemble them into patterns (explanatory principles, theories). Looking at these patterns, we make further patterns from them—the theories of our theories. Thus, in doing science, we learn.

The manner in which we do this is circular—conversational (in Pask's sense): we act iteratively, until reaching self-re-inforcing stability or misfit. We test, until we arrive at something satisfying our desires—for stability/recognisability/repeatibility/etc. Thus we arrive at our understandings. We test and test again, repeat with refinement and extend, and when driving to extremes we find our patterns no longer hold, we rejig them or start again from scratch. We adumbrate the special within the more general, coming to resting points where we say (as in design) "this is ok, I can get no further just now".

It is we who do it: we act. The role of observer-as-participant, in making knowledge, abstracting it to theory, theorising about theory; and in constructing the way we obtain this knowledge, then obtaining it accordingly, is central/essential/unavoidable/inevitable and completely desirable. Without the active participation of this actor, there would be nothing that we would know. At every step, in every action, the observer/participant is actively designing. There is nothing passive, automatic or without person (agent, scientist, designer), here.

No matter how regrettable or distasteful this may appear to traditional scientists and others drilled in the convention (the distortion) of presentation by which science puts forward its discoveries and the claims it makes for them, it is a consequence of this examination of how we do science and what we do with what we learn from doing it.

(Scientific) research is a branch of design, in which the designer is central, and through which we construct the world of (and according to) the scientific knowledge we design.

So the act of design, as we understand and value it, has much to offer as an example of how science and scientific research might be in a new era: an era that designer-readers will recognise as their contemporary paradigm and which is how scientists, when we talk to them, recognise and characterise their own activity. Design, being the more general case, satisfies Occam's razor for simplicity: as Einstein is to Newton, design is to science and scientific research.

[25] Whether the novelty is global, or only to the person designing, at that instant.

Conclusion: Research and Design

There are differences between design and (scientific) research: otherwise they would be indistinguishable and we would only need one word (Glanville 1980b). The differences, traditionally emphasised, are not my concern. My intention has been to show that (scientific) research, as it is and must be practised, is properly considered a branch of design: (scientific) research is a subset of design, not the other way round. This is the reason for the potted history in the Prologue. We who are interested in design and in researching into it are still inclined to insist we should prosecute our research according to the old and no longer sustainable view of (scientific) research: which view removes from design—and from how we consider and present it—which makes it central, important and valuable, exactly that which characterises it. Even while scientists come to realise their creative involvement in their processes.

We, in design research, should redress this imbalance, indicating the primacy and centrality of design both as an object of study and a means of carrying out that study; insisting on the impropriety of demands that design perform according to criteria of (scientific) research when design is that which encapsulates and embodies this. (Scientific) research should be judged by design criteria, not the other way round. We need to learn to believe in design, to live this, no longer apologising, but refusing to downplay what we do, kowtowing to an old and falsely elevated view.

We should not let the misrepresentations of (scientific) research be forced on us as an insensitive straitjacket. This does not mean we should not glory in the successes (and beauty) of (scientific) research. We should learn from what it offers us, including the lessons of this paper. There are qualities essential (and all too often forgotten) in design which are remembered and given primacy in (scientific) research, such as rigour, honesty, clarification and testing, and the relative strength of argument over assertion. Especially now, when design researchers are again asking about the benefits available from other disciplines, we should look for disciplines that study circularity and the included observer-participant for the insights they may afford us into the operation and consequences of those processes in our research, and what that might mean to us. That is, disciplines that are, at their base, sympathetic to design. Otherwise we forsake our primacy and dance to the wrong tune played by the wrong fiddler, who scarcely believes in the tune any more but who will, nevertheless, call the tune when we ask him to because to do so retains his primacy.

Design is the key to research. Research has to be designed. Considering design carefully (making theory from or even researching it) can reveal how better to act, do research—to design research. And how better to acknowledge design in research: as a way of understanding, acting, looking, searching.

But design should be studied on design's terms. For, design is the form, the basis. And research is a design act. Perhaps that is why it is beautiful?

Design's Secret Partner in Research.

As it happens, there is one subject that is concerned with the philosophical, psychological and mechanical examination of just these issues: cybernetics.

Over the last 30 years, and visible largely through application in other areas, it has (in the form of “second order cybernetics” or the “cybernetics of cybernetics”, the “new cybernetics”) explored the nature of circular systems and those actions in which the observer (in the most general sense) is a participant. Cybernetics has elucidated conversation, creativity and the invention of the new; multiple viewpoints and their implications for their objects of attention; self-generation and “the emergence” of stability; post rationalisation, representation and experience; constructivism; and distinction drawing and the theory of boundaries.

In this, cybernetics has been explicitly concerned with the qualities we have found to invest research and which are designerly.

This recent manifestation of cybernetics is not to be confused with that for which such large and absurd claims were made at much the same time that the early and determinist (“scientific”) approaches in design research were being pushed as the powerful way forward for design. It is a much gentler and more introspective subject, although its approach can be clearly derived from the original (Glanville 1987b 1998a, von Foerster 1974).

Given this similarity of concern and of formation, it is no surprise that, over these last 30 years, cybernetics has learnt much from design, nor that many of those most intimately involved in the development of this new cybernetics have come from or been closely involved with design.[26]

It is, in my biassed opinion, time that design redressed the balance and examined its Secret Partner in Research, the subject that, learning much from design, has clarified our understandings of designerly qualities. I hope to undertake this in a general manner in a later paper, but, for the meanwhile, the reader is referred to Glanville 1997b 1998a.

References

Ashby, R (1964) “Introductory Remarks at a Panel Discussion” in Mesarovic, M (ed) “Views in General Systems Theory” Chichester, John Wileys and Sons

Blair, K (1995) “Cubal Grids: Invariable Civilizational Assumptions, Variable Human Values” in Glanville, R and de Zeeuw, G (eds) “Problems of Values and Invariants”, Thesis Publishers, Amsterdam

Bremmermann, H (1962) “Optimisation Through Evolution and Re-Combination” in Yovits, M, Sawbi, G and Goldstein, G (eds) “Self-Organising Systems” Washington DC, Spartan Books

Chaitin, G (1975) “Randomness and Mathematical Proof” Scientific American, May

Feynman, R (1985) “QED: the Strange Theory of Light and Matter” Princeton, Princeton University Press

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[26] Particularly, Gordon Pask became a staff member at the Architectural Association, from which school many of his successful doctoral students came, and where many architecture students and teachers learnt quite unwittingly to do second-order cybernetics.

Foerster, H von (1974) “Cybernetics of Cybernetics”, Biological Computer Laboratory Univeristy of Illinois, Champaign-Urbana

Glanville, R (1975) “A Cybernetic Development of Theories of Epistemology and Observation, with reference to Space and Time, as seen in Architecture” (Ph D Thesis, unpublished) Brunel University, also known as “The Object of Objects, the Point of Points,—or Something about Things”)

Glanville, R (1977) “The Nature of Fundamentals, applied to the Fundamentals of Nature” in Klir, G (ed) Proceedings 1 International Conference on Applied General Systems: Recent Developments & Trends, Plenum, New York

Glanville, R (1979) “Inside Every White Box there are two Black Boxes trying to get out” Behavioural Science vol 12, no 1, 1982

Glanville, R (1980a) “The Architecture of the Computable” Design Studies, vol 1, No. 4

Glanville, R (1980b) “The Same is Different” in Zeleny M (ed) “Autopoiesis” Elsevier, New York

Glanville, R (1980c) “Why Design Research” in Jacques, R and Powell, J (eds) “Design: Science: Method”,,Westbury House , Guildford

Glanville, R (1981) “Occam’s Adventures in the Black Box” in Lasker, G (ed), “Applied Systems & Cybernetics” vol II, Pergamon, Oxford

Glanville, R (1987) “The Question of Cybernetics” Cybernetics, an International Journal, vol 18, republished in the General Systems Yearbook, Society for General Systems Research, Louisville, 1988

Glanville, R (1993) “Pask: a Slight Primer” in Glanville, R (ed), “Gordon Pask, a Festschrift”, Systems Research vol 10, no 3

Glanville, R (1994) “Variety in Design” Systems Research, vol 11, no 3

Glanville, R (1995a) “Architecture and Computing: a Medium Approach” in Procs 15th Meeting of Association for Computing in Architectural Design in America, University of Washington, Seattle

Glanville, R (1995b) “Communication without Coding: Cybernetics, Meaning and Language (How Language, becoming a System, Betrays itself)” Modern Language Notes, Vol 111, no 3

Glanville, R (1997a) “The Value of being Unmanageable: Variety and Creativity in CyberSpace” Procs of the Conference “Global Village ’97”, Vienna

Glanville, R (1997b) “The Value when Cybernetics is Added to CAAD” in Nys, K, Provoost, T, Verbeke, J and Verleye, J (eds) “The Added Value of Computer Aided Architectural Design” Brussels, Hogeschool voor Wetenschap en Kunst Sint-Lucas

Glanville, R (1998a) “Cybernetic Realities”, Bialystok, Technical University

Glanville, R (1998b) “Gordon Pask” Luminaries section, web site, www.isss.org

Glanville, R (1998c) “Keeping Faith with the Design in Design Research”, Design Research Society conference (ed Robertson, A), web site www.dmu.ac.uk/ln/4dd/drs9.html

Glanville, R (forthcoming) “Emergence” in review for special issue of Systems Research

Glanville, R, Forey, G and Sengupta, S (forthcoming) “A (Cybernetic) Musing 9: the Language of Science”, to be published in Cybernetics and Human Learning

Gleick, J (1987) “Chaos: Making a New Science”, Penguin, London

Hunston, S (1993) “Evaluation and Ideology in Scientific Writing”, in Ghadessy, M (ed) “Register Analysis in Theory and Practice”, Pinter Press , London

Hyland, K (1997) “Scientific Claims and Community Values: Articulating an Academic Culture”, Language and Communication Volume 17 no 1

Kelly, G (1955) “A Theory Of Personality”, Norton, New York

Kuhn, T (1970) “The Nature of Scientific Revolutions” 2nd ed, Chicago University Press, Chicago

Lakatos, I (1969) “Falsification and the Methodology of Scientific Research Programmes” in Lakatos, I and Musgrave, A (1970) “Criticism and the Growth of Knowledge” Cambridge University Press, Cambridge

Medawar, P (1963) “Is the Scientific Paper a Fraud?” The Listener September 12 pp 377-8

Pask, G (1969) “The Architectural Relevance of Cybernetics” Architectural Design 9/1969

Pask, G (1975) “Conversation Theory” Hutchinson, London

Pask, G (1976) “Conversation, Cognition and Learning” Elsevier, New York

Pask, G, Glanville, R and Robinson, M (1981) “Calculator Saturnalia” Wildwood House, London and Random House, New York

Piaget, J (1955) “The Child’s Conception of Reality’, Basic Books, New York

Popper, K (1969) “Conjectures and Refutations” 3rd ed, Routledge and Kegan Paul, London

Russell, B and Whitehead, A (1927) “Principia Mathematica” 2nd ed, Cambridge University Press, Cambridge

Spencer Brown, G (1969) “Laws of Form”, George Allen and Unwin, London

Swanson, G (1997) “Building ISSS Success—One Failure at a Time” Incoming ISSS Presidential Address 1998, web site www.isss.org

Wittgenstein, L (1971) “Tractatus Logico-Philosophicus” 2nd ed, translated Pears, D and McGuinness, B, Routledge and Kegan Paul, London