Researching Design and Designing Research[1]

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This is a slightly modified version of an essay published in the proceedings of the "No Guru-No Method?" conference held in Helsinki in 1997.

Prologue

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: i.e., 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 more than a third of undergraduate time to design science. Prime Minister Wilson and his Government declared the "White Heat of the Technological Revolution."

It is 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 practiced, 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, and 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 an 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).[2]

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 answers 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.[3] Since research should be scientific, design research should be scientific. And then design, itself, would be scientific. Research gave science its precedence, even today indicated in the growth in evidence-based studies.

1 This paper is developed from my earlier paper, "Why Design Research," in R. Jacques and J. Powell, eds., *Design: Science: Method* (Guildford, UK: Westbury House, 1980). It was written while I was a Visiting Fellow at the School of Design, Hong Kong Polytechnic University.

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

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

Design Issues: Volume 15, Number 2 Summer 1999

In this paper, I look at research, both experimental and theoretical, as done, compared with that as reported, and compare this to what is central to the act of design (as I understand it) in order to throw light on the relationship that could hold between research and design. Thus, the balance may be restored so that design is accorded what I consider to be 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.

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 inadequate research.[4]

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 through which we strive to increase our knowledge (of the world).[5] 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 usage is that of the verb.

Research usually is understood to produce extendable and testable social knowledge. A characteristic[6] 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, i.e., the outcome should be repeatable and unambiguous (stable in interpretation).

It should also be coherent: the outcome should fit with (occasionally cause reconsideration of) what already is known. Research is concerned with both individual chunks of knowledge, as well as 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 each other within the structures.

An important way of determining that our knowledge is consistent and repeatable (i.e., 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.[7]

4 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. If I hadn't had the opportunity for additional study through teaching and higher degrees, I might still think this way.

5 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.

6 G. Swanson, "Building ISSS Success—One Failure at a Time," Incoming 1998 ISSS Presidential Address (Website:www.isss.org).

7 K. Popper, *Conjectures and Refutations*, 3rd ed. (London: Routledge and Kegan Paul, 1969).

However, scientific research is not always carried out according to Popper's ideal, which is impossibly ambitious for mere humans. Kuhn[8] argued from historical observations about how scientific knowledge builds up and collapses, being modified to support accepted theories until such theories become so top-heavy in riders that they cannot survive. He divided science into the revolutionary, which is distinct from the more mundane and technical tasks of conserving the status quo which he calls "normal science." Lakatos[9] has indicated the development around accepted theories of "protective belts" that repel the unconventional.

Science as practiced 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 here with the actuality of what happens, in contrast to the "official presentation."

All this takes place 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....[10]

Experiment

Experiments are the main means by which scientists extract knowledge of the world we inhabit.[11] They do this by radical simplification. In the (idealized) 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 behavior of the system, realized in the free variable. Changing this factor, we observe any change in the behavior of the system and attribute it to the response of that variable. We organize the "inputs and outputs" so that there appears to be a simple relationship, and we determine that this relationship is determined by the variable.[12]

We have devised methods (e.g., 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.

The experimenter chooses to do the experiment and sets it up (including determining the variables). He or she observes and determines what the outcomes are, and carries out the actions. That the experimenter is influenced by social factors and epistemological outlook does not reduce his or her responsibility: the experimenter accepts these social factors, and acts accordingly.

8 T. Kuhn, *The Nature of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970).

9 I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes" in I. Lakatos and A. Musgrave *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970).

10 L. Wittgenstein, *Tractatus Logico-Philosophicus*, 2nd ed., translated D. Pears and B. McGuinness (London: Routledge and Kegan Paul, 1971). Wittgenstein elegantly points out this age-old "problem" of induction.

11 I insist investigative actions must have active agents (I am a constructivist), which are, in the case of science, scientists (who are people).

12 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, "Inside Every White Box There Are Two Black Boxes Trying to Get Out," *Behavioral Science* 12:1 (1982).

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, i.e., who breaks the circle. The experimenter designs the experiment and, if it doesn't work (well enough, in his or her 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 repeatability (i.e., stability). He or she 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 evaluating what is found, and in 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 formalized—like a Russian icon! Taught to all school children, this depiction is intended to emphasize 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—i.e., 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.[13]

The undertaking is presented in a manner supporting the view that the Great Scientific Endeavor 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.

This manner of accounting is post-rationalization: the tidy explanation after the event of what was, perhaps, a rather different experience. Emergence is a misnomer, observed in light of the processes from which emergence is taken to have occurred. And

13 P. Medawar, "Is the Scientific Paper a Fraud?" *The Listener* (September 12, 1963): 377–8.

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 nonsense; it must be a post hoc attribution.

In this traditional view, there is a power, a "right." There are solutions to problems because everything fits together in the mechanism. Indeed, it already is 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, at least within the assumptions of the framework it is to demonstrate. This modification provides the basis for studies in chaos.[14] One could, therefore, assume research would lead to the "right" answer, at least until Popper.[15]

This is not the place for the interesting discussion of why we should no longer support this view (assuming it was ever valid). One reason, and not necessarily the most powerful one, is the transcomputability resulting from Hans J. Bremmermann's calculation of the earth's computing potential.[16] We realize 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.[17] The generally promulgated view held by which we account for our research through scientific experiment is inaccurate.[18] In fact, it is no longer held by major scientists.[19]

Over the last thirty years, linguistic analysis has shown 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.[20] This finding holds for physicists as well as for social scientists. The first person style has become popular again in writing papers. Designers know they are not dealing with "the truth," except metaphysically, e.g., 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 understand that the description is not the experience; the explanation is not the actuality; prediction is not mechanism.[21] Finally, we fail to mention the actual processes of writing/reporting—as above!

Theory

Theory is what turns the (collection of) observations we build into science. It may not make these collections science, but, even insufficient, 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:

14 J. Gleick, *Chaos: Making a New Science* (London: Penguin, 1987).See also G. Pask, R. Glanville, and M. Robinson, *Calculator Saturnalia* (London: Wildwood House, 1981, and New York: Random House, 1981).

15 K. Popper, *Conjectures and Refutations.*

16 H. Bremmermann, "Optimization Through Evolution and Re-Combination" in M. Yovits, G. Sawbi, and G. Goldstein, eds., *Self-Organizing Systems* (Washington, DC: Spartan Books, 1962).

17 R. Ashby, "Introductory Remarks at a Panel Discussion" in M. Mesarovic, ed., *Views in General Systems Theory* (Chichester, UK: John Wiley and Sons, 1964).

18 R. Glanville, "The Value of Being Unmanageable: Variety and Creativity in CyberSpace," Procs. of the Conference "Global Village '97" (Vienna, 1997).

19 R. Feynman, *QED: the Strange Theory of Light and Matter* (Princeton, NJ: Princeton University Press, 1985).

20 S. Hunston, "Evaluation and Ideology in Scientific Writing" in M. Ghadessy, ed., *Register Analysis in Theory and Practice* (London: Pinter Press, 1993). Also, K. Hyland, "Scientific Claims and Community Values: Articulating and Academic Culture," *Language and Communication* 17:1 (1997). And R. Glanville, G. Forey, and S. Sengupta, "A (Cybernetic) Musing 9: The Language of Science," to be published in *Cybernetics and Human Learning.*

21 For a rather nice account of the consistent effort not to notice this, and the results of finally realizing it cannot be overlooked for ever, see the early parts of James Gleick's account of chaos.

First, to combine, coordinate, and simplify the findings of experiments by developing generalizing concepts; and,

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 experiments for verification. The relationship between theory and experiment essentially is 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 generalize.

Theory From Experiment

The first aspect of theory, theory from experiment, involves pattern finding. Humans look for patterns. Piaget insists that 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").[22] 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. George Spencer Brown's "logic of distinction" is based on this concept.[23] My first Ph.D. thesis was concerned with how, although we perceive differently, we can still believe we see the same "object."[24]

Since, in Popper's characterization, theories are not provable, they remain only temporarily valid, awaiting disapproval. They fall into a category that includes Occam's razor. The criterion—relative simplicity—of Occam's razor is no more provable than "randomness."[25] 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.[26] 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.[27]

If we did not simplify by (constantly) making (constant) objects constant, we would never be able to recognize them: nor would we "cognize" "perceptions" as being of "objects." The world we live in would have no object, and we would not be able to conceive, let alone speak, of our own "I"'s. To "cognize" 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. In a word, Nirvana.

Nor could we generalize, finding similarities in behavior and learning from repetition so we can venture the belief that, because some (observed) behavior of some object has always held, it will

22 J. Piaget, *The Child's Conception of Reality* (New York: Basic Books, 1955).

23 G. Spencer-Brown, *Laws of Form* (London: George Allen and Unwin, 1969).

24 R. Glanville, "A Cybernetic Development of Theories of Epistemology and Observation, With Reference to Space and Time, as Seen in Architecture" (Ph.D. Thesis, 1975, unpublished, Brunel University). Also known as "The Object of Objects, the Point of Points—or Something About Things."

25 G. Chaitin, "Randomness and Mathematical Proof," *Scientific American* (May, 1975). See also, R. Glanville, "The Nature of Fundamentals, Applied to the Fundamentals of Nature" in G. Klir, ed., *Proceedings 1 International Conference on Applied General Systems: Recent Developments & Trends* (New York: Plenum, 1977). Also see R. Glanville, "Occam's Adventures in the Black Box" in G. Lasker, ed., *Applied Systems & Cybernetics*, Vol. II (Oxford: Pergamon, 1981).

26 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.

27 R. Ashby, "Introductory Remarks at a Panel Discussion." See also, R. Glanville, "Variety in Design," 11:3 *Systems Research* (1994). And, R. Glanville, "The Value of Being Unmanageable."

always hold. 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 formalizes the significance and necessity of pattern. Pattern gives us objects and recognizable behaviors, allowing us to predict, and risk living by our predictions. 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.

Prediction is a means for extending the range of our observations and the patterns we have constructed—of preforming 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 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).[28] We determine that objects fall to earth unless constrained, and we generalize. Examining the generalization, 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) is the queen of the sciences.[29] Theoretical science abounds today. For instance, particle physics inhabits a universe of theoretical discourse and essentially is theory driven.[30] Sometimes such areas return to experiment, but not always. Science depends not only on theory based in collecting and organizing 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. This does not necessarily involve formal, mathematical logic.

In building theory from (and of) theory, we use the same devices we use to build theory: simplification and pattern finding. As well as the objects we have found, we treat relations and the

28 G. Kelly, *A Theory of Personality* (New York: Norton, 1955).

29 B. Russell and A. Whitehead, *Principia Mathematica*, 2nd ed. (Cambridge: Cambridge University Press, 1927).

30 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!

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 and 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; i.e., 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 essentially is self-referential. But criticism not deriving from theory itself is, nevertheless, theory applied to theory, within a wider setting. Criticism involves an abstracting from theorizing, to permit and encourage a (general) theoretical overview.

Part II: The World of Design

Reflex

I contend that 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 that (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 characterization are found in my earlier description of (scientific) research. I 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.

What Is Design?

There have been many answers to the question of what design is. The characterization that is used in this paper concentrates on design as a means of exercising our creativity.

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, which is at the center of my interest.[31] Other aspects (e.g. 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.[32]

I characterize design 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.[33] The word "conversation" is used in a recognizable and everyday manner.[34] (Pask eventually developed notions of the conversation into a highly refined technical theory of sophistication and some difficulty.)[35]

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 also may be found elsewhere. But this circular process certainly is one in which novelty—a distinguishing feature of design and so typical of creativity—can be generated, whether the novelty is global, or only to the person designing, at that instant.

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

31 R. Glanville, "The Architecture of the Computable," *Design Studies* 1:4 (1980). Also Glanville, "Architecture and Computing: A Medium Approach" in *Procs. 15th Meeting of Association for Computing in Architectural Design in America* (Seattle: University of Washington Press, 1995).

32 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 coincide. But I insist design takes a space of its own.

33 G. Pask, "The Architectural Relevance of Cybernetics," *Architectural Design* (9/1969).

34 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 footnote 35, Glanville, "Communication Without Coding."

35 G. Pask, *Conversation Theory* (London: Hutchinson, 1975). Also see G. Pask, *Conversation, Cognition and Learning* (New York: Elsevier, 1976). See also, R. Glanville, "Pask: A Slight Primer" in Glanville, ed., "Gordon Pask, a Festschrift," *Systems Research* 10:3 (1993). And, R. Glanville, "Communication Without Coding: Cybernetics, Meaning, and Language (How Language, Becoming a System, Betrays Itself)," *Modern Language Notes* 111:3 (1995).

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-reinforcing stability or misfit. We test, until we arrive at something satisfying our desires—for stability/recognizability/repeatability/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 right now."

It is we who do it: we act. The role of observer-as-participant, in making knowledge, abstracting it to theory, theorizing 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, or 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 recognize as their contemporary paradigm, and which is how scientists, when we talk to them, recognize and characterize 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.

Conclusion: Research and Design

There are differences between design and (scientific) research: otherwise they would be indistinguishable and we would only need one word.[36] The differences, traditionally emphasized, are not my concern. My intention has been to show that (scientific) research, as it is and must be practiced, 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

36 R. Glanville, "The Same is Different" in M. Zeleny, ed., *Autopoiesis* (New York: Elsevier, 1980).

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 characterizes it. Even while scientists come to realize 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 apologizing, 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 rigor, 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 anymore but who will, nevertheless, call the tune when we ask him to because to do so retains his or her 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, and 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 thirty 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-rationalization, 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.[37]

Given this similarity of concern and of formation, it is no surprise that, over these last thirty years, cybernetics has learned 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.[38]

It is, in my biased opinion, time that design redressed the balance and examined its "secret partner" in research, the subject that, learning much from design, has clarified our understanding of designerly qualities. I hope to undertake this in a general manner in a later paper.[39]

37 R. Glanville, "The Question of Cybernetics," *Cybernetics, An International Journal* 18 (1987); republished in the *General Systems Yearbook* (Louisville, KY: Society for General Systems Research, 1988). Also see R. Glanville, *Cybernetic Realities* (Bialystok: Technical University, 1998). See also, H. von Foerster, "Cybernetics of Cybernetics" (Biological Computer Laboratory, University of Illinois, Champaign-Urbana, 1974).

38 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 learned, quite unwittingly, to do second-order cybernetics.

39 R. Glanville, "The Value When Cybernetics Is Added to CAAD" in K. Nys, T. Provoost, J. Verbeke, and J. Verleye, eds., *The Added Value of Computer Aided Architectural Design* (Brussels: Hogeschool voor Wetenschap en Kunst Sint-Lucas, 1997). Also see footnote 37, Glanville, *Cybernetic Realities.*

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