Design method and scientific method

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A recurring theme in recent design theory has been a desire to relate design method to scientific method: to create the 'science of design' or a 'design science'. There is an inherent paradox in such a desire since design and science are clearly very dissimilar kinds of activities. Further, the concept of 'scientific method' now seems to be in epistemological chaos. For these reasons, attempts to model design method on scientific method seem misplaced. It is proposed that it would be more fruitful to regard design as a technology, rather than as a science. The paper seeks to establish the basis for such a view, drawing especially on the idea that both design and technology involve the application of types of knowledge other than the purely 'scientific' kind.

There is a major concern in design research to relate (in some way or other) design method and scientific method. This concern has been noticeable at many of the design research/design methods conferences that have been held over the past 20 years. It was perhaps most prevalent at the 1966 Birmingham conference on *The Design Method.* In the preface to the published proceedings of that conference[1], Gregory noted that a major aim in setting up the conference had been the hope 'to establish a common basis of agreement about the nature of "the design methods", using this phrase in the same way as "the scientific method".' Such an aim suggests an implicit desire to emulate scientists who were presumably supposed to have a definite method that they practised and which was instrumental in their successes.

However, it was clear even then that designers could not hope simply to *copy* the scientists' method, since designers and scientists have radically different interests and goals. As Gregory himself noted, 'the scientific method is a pattern of problem-solving behaviour employed in finding out the nature of what exists, whereas the design method is a pattern of behaviour employed in inventing things of value which do not yet exist. Science is analytic; design is constructive.'

So from the early days of design research and design methodology there has been a seemingly paradoxical attitude in those seeking to relate design method and scientific method. On the one hand there has been a desire to interpret design in ways similar to those in which science is interpreted; and on the other hand there have been the simultaneous *caveats* or declarations that, of course, design is not like science.

The basic text on which is founded the faith of the would-be 'design scientists' appears to be H A Simon's *The Sciences of the Artificial*.[2] In this slim volume the paradoxical 'design science' attitude is again strikingly evident. 'The natural sciences are concerned with how things are,' Simon said, whereas 'design, on the other hand, is concerned with how things ought to be'.

Despite this openly acknowledged fundamental distinction between science and design, Simon went on to outline a series of elements that would embody 'the science of design', which is something that he wants to be 'a body of intellectually tough, analytic, partly formalizable, partly empirical, teachable doctrine about the design process.' The examples of the elements of this emerging doctrine that Simon offered included several that are now regarded as of dubious value in a design context; for example, methods of optimization borrowed from management science (sic), and methods of problem structuring based on the hierarchical decomposition techniques developed by Manheim and Alexander. (One might also wonder just why design should be 'intellectually tough'? Why shouldn't the pleasure and delight of designing be accessible to everyone?)

If science is not the same as design, why has there been this urge to create a 'design science'? There is obviously some strong attraction in science which has been drawing to it many design theorists. We suspect that this attraction lies not so much in the method of science, but in the values of science. These are the values (apart from 'intellectual toughness') of rationality, neutrality and universalism.

In the early part of this century, the Modern Movement in design aimed to adopt very similar values, and it is these values that still permeate design theory. When, for instance, might the following quotations have been made?

> Our epoch is hostile to every subjective speculation in art, science, technique, etc. The new spirit, which already governs almost all modern life, is opposed to animal spontaneity, to nature's domination, to artistic flummery and cookery. In order to construct a new object we need a method, that is to say, an objective system.

> The use of the house consists of a regular sequence of definite functions. The regular sequence of these functions is a traffic phenomenon. To render that traffic exact, economical and rapid, is the key effort of modern architectural science.

The first quotation, made in 1923, is from Van Doesburg, one of the founders of the influential De Stijl movement in design theory. The second, made in 1929, is from Le Corbusier, one of the founders of the supposedly rational, objective, International Style of modern architecture. Yet both quotations might also have been made by any sufficiently outspoken design theorist or methodologist in the past 10-20 years.

The values of 'design science', then, are rooted in the notions of objectivity, rationality and universalism that were believed to constitute the 'scientific' ways of seeing the world that were prevalent at the beginning of this century. These philosophical roots were realized by Hillier et al[3], who proposed a significant reorientation of design method to bring it more in line with some of the more recent philosophy of scientific method. The earlier views of design method were based on rationalist and empiricist philosophies of science, they suggested. There had, however, been a 'slow but decisive shift in philosophy and scientific epistemology over the past half-century or so,' that had been overlooked by the design methodologists of the 1960s. One of the results of this shift was a recognition that preconceptions, or 'pre-structures', are an unavoidable

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element in scientific method.

Thus they introduced to design methodology the concept of 'pre-structure'; 'We cannot escape from the fact that designers must, and do, pre-structure their problems in order to solve them, although it appears to have been an article of faith among writers on design method (with a few exceptions) that this was undesirable because unscientific.' What Hillier et al had managed to do was to reclaim the preconception as a perfectly valid, indeed inescapable, element of design method. If scientists themselves 'really operate' on the basis of pre-structures, they argued, 'then why should such a procedure be thought unscientific in design?'

Another important concept brought into design methodology by Hillier et al was that of the 'conjecture'. Again, it was a concept legitimized by its role in science: 'It was once thought that conjecture would have no place in a rigorous scientific method. It was thought to be akin to speculation, and science sought to define itself in contradistinction to such notions. Since Popper we know that science cannot progress without conjecture, in fact that together with rigorous means of testing, conjectures constitute the life blood of science.'

They therefore proposed, following Popper's *Conjectures and Refutations*[4] model of scientific method, a 'conjecture-test' model of design method, in place of the old 'analysis–synthesis' model. This model of design method now seems to predominate in the bastard field of design science. One reason for its success is that, unlike some other design theories and models, it appears to fit in many different areas of design: for example, in engineering[5] as well as in architecture.[6] It is another mark of this model's success that what have been categorized as the first two 'generations' of design methods are now, according to Broadbent,[7] 'giving way to a third which takes a Popperian view of designing.'

However, Popper's 'conjectures and refutations' model of scientific method is not without its critics, as we will show later, and neither is its apparently very successful adaptation to design method. March[8] notes that 'the philosophy of Karl Popper has had some influence' in design theory, but suggests that 'in the main its impact has been pernicious.' March's line of argument is one that, once again, emphasizes the distinctions between design and science. 'To base design theory on inappropriate paradigms of logic and science is to make a bad mistake,' he quite rightly points out. 'Logic has interests in abstract forms. Science investigates extant forms. Design initiates novel forms.'

Some of the basic tenets of the Popperian view of science are actually untenable when applied to design, according to March. For instance, Popper is opposed to the idea of inductive logic operating in scientific method, whereas in design, March says, 'the chief mode of reasoning is inductive in tenor, that is to say, synthetic rather than analytic.' For Popper a good scientific hypothesis must be stated so as to be falsifiable, but March comments that 'a good design hypothesis is chosen in the expectation that it will succeed, not fail.'

We are left, therefore, with the distinct feeling that there are some important flaws in 'the Popperian view of designing', which is not surprising in view of the differences that exist between science and design. Indeed, it seems to us that there is an inherent aberration in attempts to build models of design that are derived from models of science. We believe that this aberration arises because, as we have noted, the attraction of a 'design science' lies not really in scientific method but rather in scientific values. We further believe that the particular value-laden view of science apparently held by the design scientists is no longer a tenable view and this we shall attempt to show in the following section.

SCIENTIFIC METHOD AND ITS PROBLEMS

For some decades now, the philosophy of science has been in a ferment, the origins of which reach back to the early years of this century and to the subsequent development of what has come to be known as logical empiricism. The basic premise of this movement was the belief that scientific theories and progress are amenable to *logical reconstruction*, and its main product was a formalized representation of the structure of scientific theories in axiomatic form. This concept constitutes the basis of what Suppe[9] has designated 'The Received View'.

It has been consistently argued, however, that the Received View suffers from serious deficiencies. Some of these may be described as 'internal', ie stemming from logical problems inherent in the Received View. Others may be termed 'external' in the sense that they stem from considerations outside the Received View, for example reflections on how scientists actually use theories in practice.

'Internal' problems include the difficulties of axiomatizing many theories commonly regarded as scientific, the untenability of the Received View's distinction between theoretical and observational terms, and its general treatment of the former. The 'external' problems have been formulated in the writings of various philosophers who objected, in one way or another, to the way the Received View focused exclusively on the finished product of the scientific enterprise – theories – and ignored the process of reasoning through which laws, hypotheses and theories come to be proposed. There are various strands to this line of attack: epistemological, psychological, sociological, and historical.

Popper and Kuhn

Distinguishing between the internal and external criticisms of the Received View provides a convenient framework for considering the specific contributions of two particularly influential philosophers: K R Popper and T S Kuhn. For our purposes, Popper's work may be viewed as an attempt to overcome (or to sidestep) some of the internal deficiencies of the Received View without sacrificing the basic principle of the logical reconstructibility of science. Kuhn, on the other hand, came at the Received View from quite another angle, with an historical study which seemed to cast doubt on the whole principle of logical reconstructability – at least in relation to the most significant cases of scientific progress.

From the outset, Popper's work[4,10] has been removed from the logical empiricist movement, though he is often (and incorrectly) subsumed into it. 'Critical rationalism' is a term sometimes used to describe his position, and it is accurate at least insofar as he maintains that science is a 'rational', logical business in its testing of theories and hypotheses. So although he would not agree with proponents of the Received View that the growth of scientific knowledge can be reduced to the study of artificial languages and logical calculi, Popper would maintain that the justification of scientific discoveries must be amenable to rational reconstruction.

His philosophy may briefly be summarized thus: theories are genuine conjectures which, though not verifiable, can be subjected to severe critical tests. Such tests consist of attempts to falsify the theory in question. Scientific research always starts with a problem, and discoveries are always guided by theory rather than theories being discoveries due to observation. It is not possible to draw a distinction between theoretical and observational terms, and the descriptive language of science is theory-laden. Various competing theories can (indeed must) exist simultaneously and can be compared on the basis of their record in falsifiability tests and their degrees of corroboration. The scientist's obligation is to test his theories ruthlessly by seeking potential falsifiers rather than confirmations. All theories (and therefore all scientific knowledge) are ultimately conjectural.

Popper's account of scientific method has proved astonishingly popular with lay audiences and seems to have been largely accepted as 'the' picture of what science is about. The reasons for this are doubtless complex and beyond the scope of this paper, but two factors are worth noting. The first is the fact that the 'hypothetico-deductive' characterization of science has been ably popularized by distinguished scientists like PB Medawar and John Eccles and, through this, has probably acquired some degree of public credibility. The second factor is that there are strong romantic overtones to the Popperian picture, with its portrayal of scientists as dedicated searchers after truth, endlessly seeking falsifiers for their most cherished hypotheses.

However attractive this picture may be, it has to be said that it has some serious drawbacks as an account of scientific method. The chief difficulty is that the falsification by which Popper sets such store is difficult if not impossible to achieve in practice. The problem is twofold. In the first place, falsification can only work if there exists a neutral observational language in which to formulate the (falsifying) observation statements. But no such language exists: all observation is theory laden. The second difficulty arises because, in real scientific practice, the entity that is being tested is rather more complex than a single hypothesis; it will include, for instance, laws and theories governing the behaviour of any instruments being used, plus specifications of initial conditions and of the experimental set-up, and so on. Popper of course recognized this from the outset, and tried to deal with it by introducing an element of professional judgement into decisions about whether to accept (falsifying) observation statements. But this really amounts only to an evasion of the issue, and to the inevitable conclusion that, since observation statements must always be tentative and fallible, the falsificationist position is drastically undermined.

Perception of these basic flaws at the core of Popper's position has led to various rescue attempts, the most notable of which is Lakatos' *Falsification and Methodology of Scientific Research Programmes.*[11] But this proposal also founders, largely because it cannot provide criteria for determining whether one research programme is 'better' than another. The inevitable conclusion to this line of development seems therefore to be that the attempts which have been made to date to impose rational reconstructions on scientific progress – whether in terms of the Received View of the logical empiricists, or the sophisticated falsification of Popper and Lakatos – appear to have failed. This apparent failure is one of the considerations which have led philosophers like Feyerabend[12] to proclaim that the only general methodological rule which could have universal validity in science is 'anything goes'.

Chalmers[13] has also noted that 'An embarrassing historical fact for falsificationists is that if their methodology had been strictly adhered to by scientists then those theories generally regarded as being among the best examples of scientific theories would never have been developed because they would have been rejected in their infancy'. Study of the history of science, therefore, would seem to offer little consolation to logical reconstructionists.

Certainly that seems to have been the general conclusion drawn by many of them from T S Kuhn's celebrated historical study.[14] In this he presented a picture of science as a process involving two very distinct phases. The first (and in a temporal sense the dominant phase) is 'normal science', ie a phase in which a community of scientists who subscribe to a shared 'paradigm' engage in routine puzzle-solving by seeking to apply the agreed-upon theories within the paradigm to anomalies of various sorts which are observed in the physical world.

The second phase of scientific activity Kuhn called 'revolutions'. These are periods of frenetic activity during which the failure of a paradigm to resolve anomalies throws a particular discipline into a state of crisis, from which it is rescued by the overthrow of the old paradigm and its replacement by another which can then serve as the basis for the next phase of 'normal science'. Kuhn's picture of science was profoundly disturbing to logical reconstructivists for a variety of reasons. In the first place they disliked his portrayal of 'normal' scientific practice, carrying as it did the implication that scientists working within such a tradition are unwilling to contemplate the abandonment of their paradigm even in the face of its apparent falsification. Failure to reconcile a paradigm with experimental or observational evidence, Kuhn claimed, will be seen as a failure on the part of the 'normal' scientist rather than as a reason for rejecting the specific theories in question.

Secondly, Kuhn's description of scientifis revolutions – the phases during which paradigms are overthrown – was distressing because of the implication that the revolutionary process cannot be 'rational'. This is because rival paradigms are 'incommensurable' – ther exists no metalanguage which would enable their merits and demerits to be compared and evaluated – and because a pardigm is never abandoned until another is available to replace it. The process of pardigm-switch is therefore a matter of faith rather than of rational judgement – something akin to a religious conversion perhaps. Some of Kuhn's critics even went so far as to accuse him of characterizing scientific revolutions as outbreaks of 'mob psychology'.

Like Popper's 'logic of scientific discovery', Kuhn's historically-inspired picture of science has also achieved widespread popular currency. The terms 'paradigm' and 'scientific revolution' have found their way into methodological discussions in hundreds of disciplines, though often with interpretations that their originator would be unlikely to countenance. They have even found their way into writings on design. And there are good reasons for their popularity. For Kuhn's picture of science has seemed intuitively plausible to many practitioners; many of his ideas, in other words, resonated with scientists' experience. For example, his concept of a paradigm highlighted the importance of shared intellectual values, 'craft' knowledge, apprenticeship, etc among a community of researchers and teachers. And his concept of a 'revolution' squared with developments in a variety of disciplines, eg plate tectonics in geophysics and the quantum theory in physics.

Just as Popper's position has been eroded by criticism, so has Kuhn's. His central notion of a 'paradigm', for example, has been challenged so effectively that Kuhn himself seems virtually to have retracted it, retreating to a concept of what he calls 'disciplinary matrices' and shared 'exemplars'.[15] Other critics have argued that the demarcation between normal and revolutionary science is by no means as sharp as he claimed. And, of course, there has been the overriding criticism that his emphasis on the incommensurability of paradigms leads in the nihilstic direction of a portrayal of scientific progress as a non-rational process.

Present position

The picture we have been assembling is one of highly sophisticated epistemological chaos. Various powerful attempts have been made to arrive at generalized descriptions of 'science'. But each in turn has collapsed under criticism. None of the alternative views – whether those of Kuhn, Feyerabend, Hanson,[16] Toulmin,[17] Lakatos or others – has managed to attract majority support among philosophers of science. The most recent attempts at deriving some form of workable synthesis has been the work of Sneed[18] and Stegmuller[19] who have argued that a set-theoretic representation of theories allows for a bridge between Kuhn and the logical reconstructivists. But it is still too early to say whether this bridge has in fact, been successfully constructed.

Transferring this discussion back into the realms of design theory, the important lesson to be drawn seems to be as follows. Attempts to equate 'design' with 'science' must logically be predicated upon a concept of science that is epistemologically coherent and historically valid. The history of the twentieth century debate in the philosophy of science suggests that such a concept does not yet exist. It would therefore seem prudent for writers on design method to back away from this particular line of argument, at least for the time being.

In the meantime it may be worth seeking an alternative model for 'design'. We propose that it will be more fruitful to view design as a technological rather than a scientific activity.

DESIGN AS A TECHNOLOGICAL ACTIVITY

> This scientific age too readily assumes that whatever knowledge may be incorporated in the artifacts of technology must be derived from science. This assumption is a bit of modern folklore that ignores the many nonscientific decisions, both large and small, made by technologists as they design the world we inhabit. Many objects of daily use have clearly been influenced by science, but their form and function, their dimensions and appearance, were determined by technologists – craftsmen, designers, inventors and engineers – using nonscientific modes of thought. (Ferguson[20])

There is, as yet, no developed 'philosophy of technology' analogous to the philosophy of science. It is therefore not yet possible to construct a 'technological' model of design in the same sense as 'scientific' (eg Popperian) models of design have been constructed. The best we can do is to begin to construct a view of design which draws upon some of the acknowledged features of technological activity. In particular we shall draw upon the acknowledged role of 'nonscientific modes of thought' in technology.

Concept of technology

Everyday usage of the term 'technology' includes the connotations of machinery, applied science and engineering. Each of these interpretations, however, suffers from deficiencies. 'Technology' (as for instance in the context of 'control of technology' or 'technology assessment') clearly denotes more than just hardware, and involves, at the very least, consideration of the organizational systems within which machinery is designed, commissioned, operated and paid for. 'Technological' achievements, whether those of building a major bridge or putting a man on the moon, are as much organizational feats as technical ones. Likewise the widespread tendency to equate technology with applied science seems to us inadequate – if only because practising technologists make use of some knowledge which is clearly *not* scientific. Indeed this has always been the case; the great cathedrals of Europe for example, were successfully completed long before the modern science of materials or the theory of structures were developed (Pacey[21]).

These considerations have led to the view that any satisfactory definition of 'technology' must highlight the following characteristics:

- The goal of practical tasks. Unlike science, technology, is oriented not towards understanding but towards action or solutions to defined problems.
- Use of different kinds of organized knowledge, of which scientific knowledge is only one. Technology also relies on craft knowledge, design knowledge and organizational and managerial skill.
- The fact that much technological activity takes place in an organizational context.

We offer the following as a definition which meets these considerations: 'Technology is the application of scientific and other organized knowledge to practical tasks by social systems involving people and machines'.

It would seem that many of the observed features of design-as-practised map neatly onto such a concept of technology. It is strongly directed, for example, towards practical tasks, solutions, or action. Designers make use of a variety of kinds of knowledge, from scientific knowledge of the properties of materials to the ineffable craft knowledge (derived from apprenticeship, experience, trial and error etc.) which enables a skilled practitioner to say that a given design solution 'feels' right (or wrong). And, finally, design usually takes place within a commercial or organizational framework. Indeed organizational constraints are sometimes cited as explanations of why ingenious designs fail to be implemented or effective.

A 'technological' view of design therefore seems to be a reasonable proposition. We also believe that it will bear fruit in terms of furnishing insights into the design activity.

Use of nonscientific knowledge

We find useful the acknowledgement within a 'technological' model that a variety of kinds of knowledge can legitimately be drawn upon. Designing relies heavily on modes of thought and ways of knowing which (so far) are incompletely defined and poorly understood, but which are characterized by being neither 'scientific' nor 'literary'. This argument has been made by Archer[22] in the context of defining design as a neglected 'third area' of general education. A related argument has also been made by Ferguson[20] in the context of the history of technological development. Ferguson emphasizes the role of 'nonverbal thought' in technology, especially the role of images, visualizations, and pictorial modes of thought.

Balchin[23] has used the term 'graphicacy' to summarize the human intellectual and practical abilities concerned with graphic and other nonverbal forms of understanding and communicating. Graphicacy is an equivalent concept to the other three, more generally-acknowledged, areas of human ability: articulacy, numeracy, and literacy. It encompasses the ability to perceive and operate on the world by means of models which are neither verbal, nor numerical, nor literary. These 'graphical' modes of thought are central to designing and making. Yet they are consistently ignored or undervalued by those articulate theorists of cognitive processes who are so deeply immersed in the numerate–literate subculture of the scientific–academic world.

The activity of designing also relies heavily on the skilled performance of the designer. This again is something which is openly and legitimately recognized in technology, but tends not to be in science. It also happens to be something which is difficult to analyse in the classic reductionist ways of science. Skilled behaviour, according to Singleton,[24] 'can only be understood in terms of organized patterns and directed series and any attempt to consider a particular stimulus-response combination in isolation is a simplification at the expense of losing the essence of the whole business.'

Like other skilled behaviour, designing is a 'whole business'. A designer attends simultaneously to many levels of detail as he designs. The level of attention encompasses the range of design considerations from overall concept to small particulars such as materials and dimensions, and a skilled designer is adept at recognizing when concept and particulars clash. A naive designer characteristically allows particulars to intrude or to dominate, to the detriment of the overall design quality.

Many of the small particulars of a design actually appear to be dealt with, or 'attended to', by the skilled designer in a subconscious way. They only surface to be dealt with consciously when they become critical. This is typical of all kinds of skilled behaviour; if the small particulars become dominant then the 'organized patterns' of the skill are lost.

The impossibility of 'attending to' all the details of the skill by the skilled performer is recognized by Polanyi[25] in his concept of 'tacit knowing'. 'There are things that we know but cannot tell,' he argues. These things relate to skills and to qualities which can only be learned by ostensive definitions, ie learning from example. The knowledge which is conveyed by ostensive definition is *know-how*.

Knowing how and knowing that

In his book *The Concept of Mind*, Ryle[26] offered a useful distinction between two categories of knowledge: *knowing how* and *knowing that*. The philosophical tradition behind this distinction stretches back a long way. For our purposes it surfaces in English philosophy in Russell's[27] distinction between 'knowledge by acquaintance and knowledge by description'. We will stick to Ryle's terms of 'knowing how' and 'knowing that', which seem particularly relevant to our present discussion.

Knowing that is the kind of knowledge which can be made explicit, which can be formulated into advice, into procedures or into organized rules of conventional wisdom. For example, an architect knows that so many square metres of space are necessary (by convention) to a four person house. Similarly he knows that a minimum sized bathroom will occupy a floor area of 2 m x 2 m using standard sanitary ware. He knows that drains should fall at a minimum gradient of 1:40, and so on.

Knowing how cannot be made explicit. It is that tacit knowledge which 'we know but cannot tell'. The architect's know-how derives from the experience of planning and designing and constructing many houses, in discovering subtle tactics within the rules, of finding incidental, spontaneous ways of subverting the rules to greater benefit.

The differences between knowing that and knowing how may become easier to understand if we think of an example outside design. For instance, knowing that is the kind of knowledge possessed by a football spectator or football coach. He knows that the way to play football is such-and-such. He understands the rules of the game, he can cite chapter and verse. He has ideas about strategy in play and methods of scoring and so on. His knowledge can be made explicit.

The game clearly cannot proceed without this kind of knowledge; knowledge of the rules, which forms a public currency between the participants and observers of the game. Yet it is not the same as the incommunicable know-how of the players. A football player knows how to play football. His know-how is embodied and embedded in the tiny interrelated details of performance. He cannot say how he does it. The knowledge cannot be transferred in talk or on pieces of paper. This is not because the player is inarticulate, but rather that his form of knowledge is intrinsically non-verbal.

The corollary of this is that knowing that is to do with explicit descriptions, to do with rules and procedures. Knowing that determines competence. If you do not know the off-side rule, then you are likely to make mistakes. If you do not know the minimum size of bathrooms, then the WC pedestal may interfere with the opening of the bathroom door, and so on. You are incompetent. Knowing that seeks to avoid mistakes, seeks competence.

But playing football or designing bathrooms is not judged to be successful merely by the avoidance of mistakes. Knowing how is to do with standards of performance that go beyond competence. Knowing how determines quality.

There are three main points to be made about these epistemological categories.

First, for very many ordinary human activities knowing that is unnecessary. We learn how to see, how to talk, walk, eat, drink, etc, without explicit instructions. After trials and errors we know how to do it. We acquire the knowledge as children and the skills become locked into us. It is conceivable that even sophisticated rule-bound skills, such as playing chess, can be developed without explicit instruction. (See Ryle[26]).

Second, knowing that depends upon knowing how. Our ability to perceive, assess, and relate to the world of particulars depends on rudimentary know-how. The accumulated content of knowing that is composed from records of those individual acts of perception, measurement and so on. The one kind of knowledge is prior to the other.

Polanyi[25] goes further. His use of the term 'tacit knowledge' embraces know-how and what he calls 'knowing by relying on'. 'Tacit knowing can indeed be identified with understanding, if understanding is taken to include the kind of practical comprehension which is achieved in the successful performance of a skill. This kind of tacit understanding Polanyi calls the 'central act of knowing'.

Third, under many conditions of practice the two types of knowledge are mutually exclusive: they actually interfere with one another. In the employment of know-how, in the practice of a skill (such as playing football), the rules of play, the instructions of the coach (knowing that), the concentration upon the explicit, inhibit the smooth operation of the skills.

When actually performing skillfully it is necessary that the rules, the advice, the instructions, should be internalized and 'forgotten'. The exercise of know-how is unselfconscious – if not without thought. It is inhibiting to concentrate on rules and explicit procedures when engaged in skilled performance. The centipede knows how to walk: if he thinks about it too much, the centipede stumbles!

What does all this mean for designers? Well, the three lessons are reiterated in a more specific form.

Firstly, knowing that is not of necessity part of design. Theory may not be all that helpful. Tacit knowledge embodied in craft seems quite capable of producing objects which are well made, fitting to their context, appropriate to the users, and rich in significance, in short 'good' designs.

Secondly, knowing how, ie the inexplicit, manipulative nonverbal acts of skill, lies at the core of design. Historically design has arisen from craft, and it contains very many skills and embodiments of know-how. But also, no artefact exists just because of theory. As Pye[28] notes, 'If there had been no inventions, there would be no theory of mechanics. Invention comes first.'

Thirdly, knowing that, ie knowledge of the explicit 'rules' of design, can actually inhibit practice. The focus of attention can be in the wrong domain, in the explicit procedures rather than the subtle details of performance.

We can now see that the activity of science is directed by knowing that, towards error-free explanation, towards scientific 'truth'. Design and technology, on the other hand, are directed by knowing how, towards seeking performances and products of skill and quality.

CONCLUSIONS

We have argued that the commonly-adopted 'scientific' models of design are not really tenable in view of the current epistemological chaos surrounding the concept of 'scientific method'. Further, we wholeheartedly agree with the 'design scientists' themselves that design simply is not like science. As an alternative, we have proposed that design should be viewed as a technological activity, and that a 'technological' model of design would be more fruitful than continued attempts to construct a 'scientific' model.

However, models (of whatever kind) of the design process have no value in themselves. Their utility derives from the extent to which they enable us to understand and improve teaching and practice. In this connection, a 'technological' model such as we propose does seem to have something to offer. For example, it encourages us to include and develop within design theory concepts of other forms of knowledge than the simply 'scientific' forms, and leads us to recognize the importance of craft knowledge in the acquisition of design skills.

To some people it may seem that we have simply (or perhaps not so simply) stated the obvious, in the sense that many designers have always subscribed to an implicit model of design which resembles the one we propose. If that is indeed the case, our attempts to make the foundations of such a model more explicit may at least serve to reassure these designers that their basic instincts about the teaching and practice of design are sound. And those who disagree, or who wish to continue their advocacy of a 'scientific' model, will at least have an opposing conjecture to refute.

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