

## **Chapter 2: Theoretical knowledge in the world of practice**

The literature review revealed a certain amount of difficulty in reconciling different research traditions, and some uncertainty about the form which we might expect knowledge about typography to take. Before following up the specific lines of enquiry suggested at the end of the last chapter, then, it is worth taking some time to focus on some general problems raised there about the conduct of research and the communication of its outcomes.

### **The myth of the two cultures**

Those involved in teaching or researching typography often feel the need to justify the business of theorizing about design. Their critics (for example, Chapman 1978) charge them with irrelevance to real situations, and sometimes imply an irreverence for the mysteries of creativity. Perhaps because of a schooling which forced a choice between arts and sciences at an early age, many of us have inherited what may termed the 'two cultures' attitude, following CP Snow's famous essay (1959). Depending on your bias, either artists are unsystematic and disgracefully ignorant of the basic facts of Science, or else the creativity, mystery and uncertainty of Art is seen as stifled or tainted by the philistine and plodding empiricism of Science. That dichotomy never did stand close examination and, after thirty years of particle physics, molecular biology and artificial intelligence research—areas of science which question the nature of matter, life and thought, and border on philosophy—it seems somewhat quaint. To some degree it is the researchers' own fault that they are caricatured as insensitive to the complexities of design: as the previous chapter showed, it is easy to explain why research is rejected by some

designers and design teachers as unusable.

On close examination it is easy to get the impression that the distinction between the sciences and the arts is dissolving. On one hand, philosophers of science have replaced our confidence in the certainty of Science with a highly relativist view that has gained wide acceptance: where we might once naïvely have thought that scientific theories derive from the observation of facts, we are now told that the facts themselves only exist in terms of theoretical frameworks. Popper (1959), for example, argued that scientific theories (or conjectures), far from representing certainties, are actually characterized by the possibility of refutation. Polanyi (1958) convincingly demonstrated the role of creative intuition in scientific discovery. Kuhn (1962) argued that fact-gathering in the absence of a body of theory or beliefs is a virtually random activity. And Feyerabend (1975) suggested that scientific rationality has been neither the dominant nor the best model, but that progressive science is essentially anarchical. As far as Art is concerned, the twentieth century has seen the Muse replaced by the quasi-linguistic theories of semioticians and structuralists; and we have witnessed the quest for a 'science' of design (Simon 1969), although this has met with difficulty and some disillusionment (reviewed by Cross 1980).

In spite of such developments in the philosophy of science, old attitudes certainly survive in typographic research. Some researchers distance themselves from designers through an exaggerated respect for their 'artistic', 'intuitive' or 'aesthetic' judgement, which is seen as entirely closed to the scientific method. The converse of this is a ritual sneer at the unsystematic, even primitive, intuitive approach. For example, Jonassen (1982: x) proposes a 'technology of text' which he describes as 'the application of a scientific approach to text design.' He continues:

'It exists as a counterpoint to the artistic and unsystematic approach to text design and layout that has prevailed since petroglyphs were first inscribed on walls'.

He goes on to emphasize that it would be a mistake to apply the

technology to anything but expository text: again, the arts are quarantined from the rigours of the scientific approach.

Ravetz (1971) has warned of the dangers of this assumption that practical problems always have technical solutions. Drawing a distinction between the technical and the practical, he cites the inability of the USA and USSR, two great technological nations with the (technical) ability to send people into space, to solve their (practical) social or managerial problems. According to Ravetz, technical problems have simple, easily identified goals (for example, a typical civil engineering problem might be to build a bridge over a certain river to carry a specified volume of traffic), while practical problems are bound up with competing social and historical pressures. Technical problems are indeed practical, but practical problems are not always just technical. Those familiar with the typographic research literature may recognise this consequence of technical solutions:

'If the inquiry avoids "theory" and becomes "empirical", it can encounter the pitfall of simplifying its objects of inquiry to homogeneous populations defined by classes of simple data; then the complexity and contrariness of the situation, which created the problem situation in the first place, is lost from view.' (Ravetz 1971: 355).

Taken to extremes, this can lead to experimental hypotheses which appear obvious, trivial and even self-fulfilling. An example of this is a much-cited paper by Dooling and Lachman (1971) who investigated the effect of titles on the comprehension and recall of text. They prepared a text which was deliberately vague and, depending on the title, was interpreted by subjects as being about Columbus discovering America or men landing on the moon (it referred to unfamiliar rocky landscapes, the ship, the long journey, a new discovery). But no psychologist has yet managed to convince me that this is not a self-fulfilling experiment, since an ambiguous text, by definition, is one which can be assigned alternative interpretations. If it turned out that the text was always understood in one particular way,

whatever the title, it could simply be rewritten until vague enough for the experiment to work.<sup>39</sup>

Although written in a specialized context, Dooling and Lachman's paper is frequently cited in general research reviews on text design, suggesting that reviewers imagine that their readers need proof before they will believe that vague documents need titles. As the educational psychologist John Carroll once remarked, 'it is a poor science that does not improve on common sense'.

Social scientists have the problem that whereas most physicists deal with matters (in both senses of the word) far removed from everyday life, everyday life is what social scientists study. Taylor (1980) has highlighted the difficulty they have in finding scientific ways to define terms, such as 'personality' or 'style', which ordinary people use constantly in conversation without any trouble.

'An approach attempting to adhere completely to a methodological imitation of the natural sciences could never tell us even as much as what we do already know, prescientifically, about behaviour, nor about how we could have arrived at such knowledge.' (p, 4)

Reviewing the educational psychology literature on text design, it is quite common to find what at first sight appears to be a staggeringly naïve view of what counts as knowledge. In effect, a game is being played where a new 'fact' is admitted to the circle of those playing only when an experiment has appeared in the literature to support it. No other knowledge counts. The game is played in code: 'nothing is known about...' or 'we do not know...' means 'no one has published an experiment about...'. My mind seized up for a few seconds when I encountered the following

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<sup>39</sup> In fairness to Dooling and Lachman, at the time of publication their paper was not especially intended to contribute to the practical literature on text design where it is so often cited. Instead it played an influential role in the general reawakening of cognitive psychology. Their purpose was simply to demonstrate the degree to which contextual inference contributes to the comprehension of language, and meaningful comprehension contributes to memory. Even so, it surprises the lay observer that such things needed demonstration.

conclusion to a recent review of classifications of research questions:

‘On the whole, little is known about the kinds of questions that may be posed for research’ (Dillon 1984: 327).

## Pure and applied research

I drew a distinction between the purpose of Dooling and Lachman’s study and the circumstances in which it is often cited. This highlights another dichotomy in addition to one between the arts and science. Within the scientific tradition there is the issue of how pure and applied research relate to each other and to the world of practice.

Pure research in psychology is concerned with the investigation of fundamental and relatively abstract aspects of perception and cognition, isolated from real-life contexts: psychology’s basic task is to explain how people perceive, think, remember and so on. Cognitive psychologists build models which embody hypothesized components and mechanisms of a cognitive system. These must be consistent with known data and testable by experiment. Consequently, many studies of, for example, the reading process address themselves to the theoretical problems of a particular model rather than to the actual problems encountered by readers. In contrast, applied psychology uses the same methodologies to tackle real-life problems more or less directly (‘more or less’, because even applied psychology can seem extremely abstract to lay people).

Wright (1978) has explored the relationship of pure and applied research into language comprehension. She argues that both branches are interested in the connection between ‘factors prior to reading’ (differences in texts, subjects and reading goals) and performance measures (such as retention or comprehension). The difference lies in the nature of the connection: pure psychologists are interested in ‘Theories of HOW’ while applied psychologists are interested in ‘Theories of WHEN’. For example, a ‘pure’ theory might explain how language is represented in memory, while

an 'applied' theory might state that when certain kinds of questions are inserted in a text it is memorized more easily. However, some have doubted that such applied 'theories' are in fact theoretical unless linked by some general framework: Rickards (1977), for example, concluded that most research into inserted questions was atheoretical and thus impossible to integrate and apply. And Anderson and Biddle (1975), reviewing the same literature, dubbed it 'mindless empiricism'. Brian Lewis (personal communication) used the term 'dustbowl empiricism' to describe the endless succession of atheoretical experiments on this and similar educational issues. If there is no foundation for the process of systematic enquiry we are dealing not with science but what Hudson (1972) has called 'the cult of the fact'.

Indeed, Wright goes on to remark that:

'more typically in practice the contents of the Theories of WHEN box [in a diagram she provides] seem to be an accumulation of statements about the conditions in which performance is improved or impaired.' (p. 263)

She cites the legibility research reviews of Tinker (1963) and Spencer (1969b) as examples. We have already reviewed some of the difficulties of applying such research.

A related problem is that it is very easy for research results to become detached from the conditions and qualifications alongside which they are originally presented. Most psychologists are well aware of the multi-dimensional nature of the phenomena they investigate, but there is a trade-off between different purposes of research. Frase (1973) warned users of psychological research on text that they should distinguish between three kinds of problem—theoretical, methodological and practical. There is a danger of attempting to interpret research which is primarily intended to solve methodological or theoretical problems as a source of practical advice.

In an influential book *Cognition and reality*, Neisser (1976) made a plea

for cognitive psychologists to respect what he terms 'ecological validity'. This term describes the extent to which theories of cognition account for pragmatic considerations of the subjects' world view in addition to 'content-free' mechanisms of the mind. He cites Newell (1973) who listed fifty-nine different research paradigms in use at that time, fifty-seven of which were based on artificial laboratory situations. Neisser appeals to cognitive psychologists to

'make a greater effort to understand cognition as it occurs in the ordinary environment and in the context of natural purposeful activity. This would not mean an end to laboratory experiments, but a commitment to the study of variables that are ecologically important rather than those that are easily manageable.' (p. 7)

For an example of the easily manageable being preferred over the ecologically important, we might turn to research on educational texts. Many experiments, including some of those on 'typographic cuing' (reviewed in Chapter 1), continue to use literal recall as a convenient measure, although outside the laboratory the need to recall the exact words of a text is rare. Furthermore, Neisser is talking not about applied but about pure research on cognition:

'A satisfactory theory of human cognition can hardly be established by experiments that provide inexperienced subjects with brief opportunities to perform novel and meaningless tasks.' (p. 8)

If only one or two variables are measured it is possible to produce statistically significant data which is easy to interpret. Although psychometricians have produced methodologies and statistical techniques for handling multi-dimensional issues, they are complex and technically demanding and are less likely to give clear answers; they may also be of doubtful use in the hands of those who only half understand them. And if such studies were to use a wide variety of subjects (age-groups, cognitive styles, educational and socio-economic groups etc) the technical problems of experimentation could be overwhelming. This is not to discount the contribution of psychologists and experimentation, but to argue that the most usable research results are those that show an awareness of their

limitations and their place within a wider theoretical framework.

This view is echoed by applied linguists (eg Widdowson, 1979; Brumfit, 1980) who have similarly had to address the relationship between theoretical and applied linguistics. The way forward for theoretical linguistics was established by de Saussure's distinctions between *langue* & *parole* and diachronic & synchronic. By studying language as a symbolic system frozen in time, practical issues to do with translation or second languages could be ignored. Language could be studied in a 'pure' form undistracted by its social context. However, as soon as we need to study language as a communication system, it is complicated by human motivations and conversational roles.

This is not to say that research or scholarship must be unselective. All scholars obviously have to sort out that which is relevant from that which is irrelevant to the problem in hand. Popper (1957: 145) remarks in the course of a discussion of the nature of historical study:

'If we say that the cause of death of Giordano Bruno was being burnt at the stake, we do not need to mention the universal law that all living things die when exposed to intense heat. But such a law was tacitly assumed in our causal explanation.'

A crudely expressed 'law' like that might be disputed by biologists who might conceivably know different, or who might have a better way of putting it, but it is good enough for the historian's purpose. Putnam (1978) calls this 'interest relativity'.

At one time it was possible to talk of the 'unity of science', whereby sociology was reducible to psychology, psychology to biology, biology to chemistry and chemistry to physics. Individual fields of study are often themselves divided into levels of analysis. Linguistics, for example, is sometimes divided between various applied fields which are reducible, through semantics, syntax and phonology, to phonetics (as Figure 2.1 illustrates).



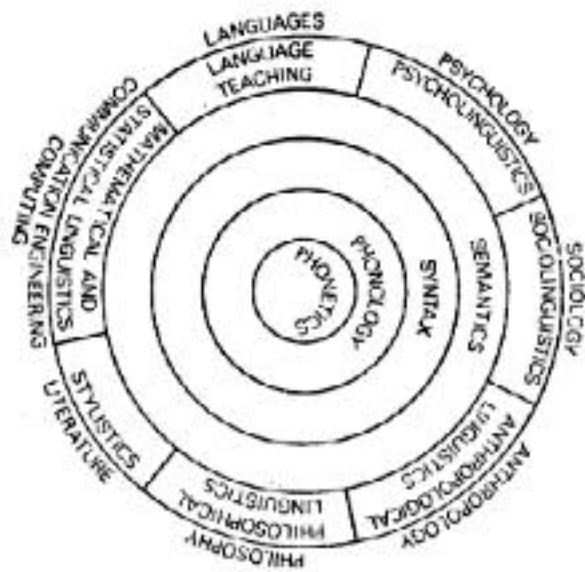


Figure 2.1 The linguistic sciences, as diagrammed by Aitchinson (1978).

There are two problems here. The first is that in order to focus their efforts on a particular level of analysis, scholars have to simplify their assumptions about peripheral matters. Those simplified assumptions may be made from a state of ignorance about other levels of analysis. That risk will always be present and calls for good interpreters or popularizers. The second problem is more fundamental: we have to distinguish between different levels of analysis within a unified world view, and different, incompatible, world views.

The unity of science view has been rejected by most philosophers of science, who talk instead about the incommensurability (which may be roughly translated as incompatibility) of scientific paradigms. Feyerabend (1975) likens this concept to the gestalt switching which we experience when looking at well-known 'impossible' figures (Figure 2.2). In both the figure and in a scientific theory, apparently immutable things may change their function or disappear altogether when viewed in a different way.

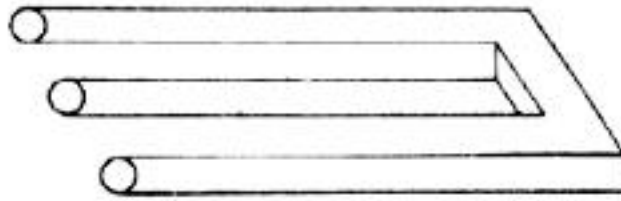


Figure 2.2 An impossible figure used by Feyerabend (1975) to demonstrate the principle of incommensurability.

The clash of incommensurable paradigms may explain the tone of the primacy of speech debate among linguists (Chapter 1), symptomatic, perhaps, of the fundamental incompatibility of the structuralist and functionalist linguistic paradigms. Indeed, similarly intemperate language can be found in well-known paradigm clashes in other fields: when evolutionists discuss creationism, for example, or when monetarist economists discuss green politics. Where modernist graphic designers condemn symmetrical layouts as irrational, or where applied psychologists appear to dismiss intuitive knowledge, there, too, is evidence of paradigm clashes.

## Paradigms

The term 'paradigm', used in this way to refer to systems of thought, is associated with Thomas Kuhn (1962). Kuhn defines science primarily in social terms—for him, a mature scientific discipline is a community of scholars who share a common paradigm. A paradigm is a model or pattern. It may be a theoretical statement or law, agreed within a scientific community, that anchors a field of enquiry and suggests a firm course for further study. The essence of Kuhn's theory is that science progresses not by the steady accumulation of facts but by a succession of revolutions (or 'paradigm-shifts'), where paradigms (and many of the 'facts' attached to them) are discarded in favour of new ones that are seen as better able to resolve key problems. Together with other influential

post-war philosophers of science, he effectively laid to rest the popular ideal of the ultimate unity of science, and placed science firmly in the context of human activity rather than a quest for Universal Truth.

Kuhn emphasizes that paradigms rarely start out as complete systems of thinking. In fact it seems to be important that paradigms are not complete. According to Kuhn, most scientific work (what he calls 'normal science') consists of trying to fit previously known observations into the paradigm's framework, or trying to make new observations which it predicts. Paradigms are valuable because they define the agenda for a discipline: they pose a series of puzzles that fascinate individual scientists and motivate them to do detailed work of the sort that would be impossible if each saw it as his or her task to build the field from scratch.

Kuhn's ideas, concisely and persuasively written, have proved attractive to members of relatively new specialisms who are struggling for consensus. I have found him cited not only by other philosophers of science but by writers on research areas relevant to this study—for example, discourse analysis (Coulthard, Montgomery & Brazil 1981), linguistic history (Cohen 1977) and reading research (Venezky 1984). Members of such young disciplines perhaps feel challenged by Kuhn's remark that

'...it remains an open question what parts of social science have yet acquired any paradigms at all. History suggests that the road to a firm research consensus is extraordinarily arduous.' (p. 15)

While not explicitly condemning the social sciences, he goes on to point out that in the absence of the focusing and unifying power of a paradigm, fact-gathering is restricted to readily available data and commonsense observation.

It is not altogether accurate, though, to suggest that the social sciences have no paradigms; cognitive psychology, Chomskian linguistics, and structuralist criticism, for example, all replaced previous paradigms in a revolutionary manner—not in the sense of sweeping them off the face of the earth, but by being incompatible with their predecessors they forced

individual scholars to choose between them. Having said that, though, it should be noted that Kuhn's remark on the paradigm-less state of the social sciences was something of an aside. Elsewhere he refers to them only obliquely and as a rhetorical contrast to the established sciences. For example (of broader interest to typographers, perhaps), he notes that technical articles are preferred to books as the communication medium within an established science, since they can assume agreement within the shared paradigm and knowledge of its facts. But:

'only in the earlier, pre-paradigm stages of the development of the various sciences did the book ordinarily possess the same relation to professional achievement that it still retains in other creative fields.'  
(p. 20)

In a postscript to the 1969 edition of *The structure of scientific revolutions*, Kuhn expresses puzzlement at the extension of his ideas to areas outside science, since he confesses that they were themselves borrowed from those fields:

'Historians of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way. Periodization in terms of revolutionary breaks in style, taste, and institutional structure have been among their standard tools.' (p. 208)

This is certainly the way that design is taught: while a dominant paradigm is hard to discern, we do have distinct schools and periods—the Arts and Crafts, the Bauhaus, Swiss typography, and the historical tradition, for example. In certain places and at certain times a particular paradigm has dominated, and the way has been made clear for a flurry of activity within its boundaries.

Whereas for Kuhn the contrast between science and 'immature' social sciences is relatively incidental, Ravetz (1971) has taken up the issue in more detail. Like Kuhn, Ravetz examines science as a social activity. He seems particularly fascinated by the edges of science: pseudo-science, folk

science and immature disciplines. As well as offering a detailed and, to students of typography, recognizable description of immature fields of inquiry, Ravetz also offers positive advice to those engaged in such fields, whose ‘model of genuine science is a very specialized one, which may be quite inappropriate to their own tasks’. He points us to

‘the history of scientific inquiry in the period before the rise to dominance of “positive science”. Then, there was a clear distinction between two sorts of inquiry, “history” and “philosophy”; and they were in turn distinguished from “art”.’ (p. 372)

The terms survived well into the nineteenth century: although ‘natural philosophy’ is now ‘physics’ or possibly ‘science’, ‘natural history’ has been kept alive as the name of a museum, and the older meaning of ‘arts’ lives on in the title of the Royal Society of Arts which promotes practical skills, not poetry or painting.

Ravetz especially warns of the temptation to assume the outward pretence of the positive sciences before it is fully justified: elaborate mathematical or symbolic systems may become grotesque parodies of the realities they claim to describe. This view can be found in many discussions of applied fields of study: a typical example is Brumfit (1980) who discusses applied linguistics in these terms:

‘...a great deal of harm has been done by the enthusiasm of practitioners for inappropriate statistically-based experimental work, when discussion of a synthetic rather than analytic nature may have much greater value: there are academic dangers in formalism and practical risks in the adoption of inappropriate ritual.’

Similar views are to be found among sociologists, educational theorists and psychologists (some of whom I cited earlier in the chapter), so much so that it is fair to talk of a consensus of opinion running across discipline boundaries—although still a consensus of the minority.

Those who seek support from Kuhn’s work for the establishment of a unifying paradigm sometimes appear to assume that although the ‘immature’ social sciences may not yet have recognizable paradigms or are

not yet mature sciences, the attainment of scientific status is nevertheless an ideal to be pursued. An important distinction, though, between the sciences and the social sciences is embodied in the term itself: the social sciences recognise the social context in which their subject of study is found. While scientists are typically removed from their subject, which may be viewable only through an apparatus or methodology, social scientists are participants in theirs. A single paradigm may be essential to scientists trying to make sense of a reality mediated by an oscilloscope, but is unlikely to satisfy the social scientist who, as a human being with complex social, spiritual and physical needs, can sense the futility of pretending the world is one-dimensional.

Ravetz proposes a substitution of terms that might help young areas of study (such as typography) avoid changing from immature sciences to mere pseudo-sciences. He suggests that 'research' might be replaced by the less formalized concept of 'history' and 'theory' by 'philosophy'.

'It would then not be a cause of surprise or shame that effective new insights come only very rarely...and it could be recognized that an essential part of a genuine education in the discipline is a dialogue with its great masters.' (p. 374)

This view is of some comfort to teachers of typography who traditionally engage in just such a 'dialogue with its great masters', focusing on key personalities whose work is catalogued and style analysed. Typographic philosophies may thus be viewable through typographic history, and may be embodied in the work of 'masters' (whether Aldus Manutius or Herb Lubalin) whose work is representative of a particular coherent and distinctive approach accessible through criticism and, like fine artists who copy paintings in galleries, through exercises in pastiche.<sup>40</sup>

But while attention to the past is common among typographers and

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<sup>40</sup> Macdonald-Ross (1977) has argued for the study of 'master performers' as a means of arriving at generalizable principles of good design; indeed, the currently fashionable quest for 'expert systems' is based on the study of high-performing individuals.

especially artists, it is rare among scientists, who typically see progress as cumulative. Feyarabend (1975) suggests that scientists, too, would do well to study the past:

‘No idea is ever examined in all its ramifications and no view is ever given all the chances it deserves. Theories are abandoned and superseded by more fashionable accounts long before they have had an opportunity to show their virtues.’ (p 49)

As if to prove his point, the same idea was expressed seventy years earlier:

‘Do you want to get at new ideas? read old books, do you want to find old ideas? read new books.’ (attributed to Robert, Earl Lytton by the Times Literary Supplement, October 19th, 1906; quoted by Dobson 1917)

Ravetz’s third component, ‘arts’, is also fully compatible with traditional typographic study. The term is used to describe ‘the set of principles defining the methods of any class of tasks’. The linking of history, philosophy and arts, for Ravetz, gives theorizing a goal and retains its links with the real world. It also recognizes the value of craft skills, expressed aphoristically rather in the form of ‘laws’ or theories, as providing a first generation of facts for the new discipline.

## Guidelines and slogans

The communication of aphoristic, practical knowledge presents certain problems. Practical books and articles often present advice or research findings as simple guidelines. In their more general form, guidelines have been termed slogan language (‘form is function’ is an example of a frequently repeated typographic slogan).

Wright (1985) has been particularly critical of low-level (ie, detailed) guidelines which, applied without sensitivity to their inevitably numerous exceptions, can do more harm than good. She further points to the sheer number of guidelines needed to cover the range of problems encountered

in text design, citing Hartley whose 'Fifty guidelines for improving instructional text' (Hartley and Burnhill, 1977b) were subsequently expanded to eighty (Hartley 1981). She remarks, 'Why stop there?'.

In his reply Hartley (1982) suggests that guidelines are helpful to novices if not treated as inflexible rules. Indeed, his papers and books are enthusiastically sought by educators wishing to know how they can write and present good textbooks. My own concern is that guidelines should not become detached from supporting evidence. A typical guideline might say 'Use simple language (Some name, 1979)', without detailing those circumstances under which simple language might be misleading, or what constitutes simple language. More seriously, on following up the reference given one can find that the cited author has simply remarked, say, 'Use simple language'. Research references have sometimes been used for persuasive purposes to lend authority to the guideline—indeed, when non-experts seek research references it is frequently for this reason.

Since guidelines are often neither detailed enough for exact application nor generalizable through a theory, they can appear to offer contradictory advice. For example, Winn & Holliday (1982) offer research-based guidelines for diagramming which separately suggest that the components of diagrams should be arranged in a left-right, top-bottom reading sequence, should be arranged to reflect the physical arrangement of the system described, and should be arranged so that graphic proximity reflects conceptual relatedness. In reality these are all good alternatives to consider when faced with a diagramming problem. Presented in guideline form, though, they can too easily become detached from their supporting argument.

In the field of typography an example of well-presented guidelines is provided by Felker and his colleagues at the Document Design Center, Washington DC (1981). Twenty five principles for clear writing and design are presented together with generous examples, a critical review of relevant research, a bibliography, and, most important, qualifications



which emphasize the exceptions to the rule. When guidelines are presented as the conclusions to a sound argument or a well-documented case study, then they can indeed be useful and effective. However, there is still no unifying framework for the advice. The guidelines do not exemplify or embody a practical theory, mastery of which would enable the reader to build insight in order to tackle problems not anticipated by the guideline writers.

In a different context (a discussion of the practical curriculum advice offered to schoolteachers), the problem of guidelines has been attributed by Anderson (1981) to a false equation of practicality with simplicity. He argues that features often associated with good practical manuals, such as simplicity, readability and the use of familiar examples, do not necessarily make researchers' advice usable. Good practical manuals, he suggests, must be sincere in their practicality: too many curriculum researchers use a rhetorically contrived appearance of practicality, either because it is expected of them, or simply to ease standards of academic criticism. In addition, Anderson argues, researchers usually underestimate both the complexity and the orderliness that lies behind the apparently chaotic (to the outside researcher) classroom situation. Practicality is not the same as simplified theory, but is related to the context of a manual's use as it is perceived by its users. The question is, of course: how are writers of guidelines to get at such perceptions? Anderson (a sociologist) suggests ethnomethodological techniques to model the intentions and actions of people in practical contexts. Such techniques are briefly reviewed in Chapters 5 and 8.

Earlier I discussed the relationship between pure and applied research (mostly in relation to psychology). But although psychologists might see the debate about the pure/applied distinction in terms of their real-world relatedness, to most designers both are in the realm of 'theory'. Far from being at opposite ends of a continuum of practicality, both pure and applied psychology are dimensions of scientific investigation, itself distinct in most designers' minds from 'commonsense' or intuitive knowledge.

When psychologists conclude research reports related to language with practical recommendations or guidelines, they switch from a mode of reasoning with whose rules they are familiar to one they often underestimate. While following methodological conventions with precision, they risk cutting across the accumulated and, often widely shared, practical knowledge of their audience. Whereas most scientists seek universal laws and dislike exceptions, practical knowledge is value-laden and context-sensitive. Because it is very often tacit rather than articulated, it is easy to underrate.

Stefan Körner (1970), a philosopher of science, sees the distinction between science and commonsense as a matter of degree, arguing that it is not the case

‘that the aims of science—prediction, explanation and mastery of natural phenomena—are foreign to commonsense, but rather that science pursues them in a more methodical manner.’ (p. 39)

He goes on to suggest that

‘Commonsense thinking and the logic underlying it do not satisfy the requirement of the exactness of all attributes. Many, possibly all, of its classifications rely on the recognition of similarities of objects to standard examples and to standard counterexamples’ (p. 43; my italics)

Returning to the context of curriculum advice, Ilene Harris (1981) has similarly argued for the replacement of ‘slogan language’ with what she terms a ‘case rhetoric’ in which theoretical precepts are interleaved with practical examples which build personal insight alongside intellectual understanding.

## Explicit and tacit knowledge

Harris quotes Dreeben’s (1970) analysis of the problems of teacher training, with which typographic teachers and researchers might identify:

‘...there probably exists enough individual knowledge and experience stored in individual heads to provide the basis for sophisticated technologies—were that knowledge and experience ever brought together, codified, tested for efficiency and communicated to teachers both in training programs and on the job.’ (p.212)

This wistful longing to encapsulate knowledge in objective form is at the heart of all applications of the scientific method to practical areas, and is exemplified by current proposals to apply computer expert systems to typography (Hewson & Lefrere 1986; Rivlin 1987). But it could be argued that all this ‘individual knowledge and experience stored in individual heads’ is what constitutes a technology. The scientific method is a system of knowledge which, even if it started as a creative intuition or hunch, is publicly demonstrable by reasoning and experiment: the method as well as its outcomes are supposedly open to question and debate (‘supposedly’ since the extreme technicality of many scientific processes places them out of the lay person’s reach). But although designers can draw on scientific theories from time to time, the solving of complex problems stems from a different kind of knowledge that, although possibly beyond analysis, can nevertheless be effective.

Michael Polanyi, himself a scientist, developed this notion of ‘tacit’ knowledge in a series of books and lectures. He argues that it is not only as real and valid as explicit knowledge but that it forms the root of all knowledge, citing numerous examples of the intuitive nature of scientific discovery:

‘Let us recognise that tacit knowing is the fundamental power of the mind, which creates explicit knowing, lends meaning to it and controls its uses. Formalization of tacit knowing immensely expands the powers of the mind, by creating a machinery of precise thought, but it also opens up new paths to intuition; any attempt to gain complete control of thought by explicit rules is self-contradictory, systematically misleading and culturally destructive. The pursuit of formalization will find its true place in a tacit framework.’ (Polanyi 1969: 156).

Another important discussion of the problem of theory and practice is by Ryle (1949), who addressed the distinction between 'knowing that' and 'knowing how'. Ryle's ultimate concern is rather more fundamentally epistemological than our present one, to destroy what he terms 'the dogma of the ghost in the machine': that is, the mind/body distinction of Descartes. In doing so he argues that practical skills and actions exhibit intelligence in exactly the same way as mental skills. According to Ryle, Cartesian dualism implies that thinking (by the mind) and doing (by the body) are separate operations, that thinking must precede doing and that intelligent practice exhibits a prior mental process. He argues instead that when we describe an action as intelligent we are not discussing a mental process of which the action is just an outcome, but we are discussing the action itself. Indeed, turning the tables on the 'intellectualist tradition', Ryle suggests that arguing logically is itself a practical skill:

'Rules for correct reasoning were first extracted by Aristotle, yet men knew how to avoid and detect fallacies before they learned his lessons...They do not plan their arguments before constructing them. Indeed if they had to plan what to think before thinking it they would never think at all.' (p. 30)

Ryle's polemic against the intellectualist tradition is attractively democratic—each of us ('the boxer, the surgeon, the poet and the salesman') applies similar intelligence to our particular tasks. But what does this intelligence consist of? It does not simply mean performing well—clocks and performing seals do that. Ryle argues that intelligence is instead to do with responsibility for effective performance:

'To be intelligent is not merely to satisfy criteria, but to apply them; to regulate one's actions and not merely to be well-regulated. A person's performance is described as careful or skilful, if in his operations he is ready to detect and correct lapses, to repeat and improve upon successes, to profit from the examples of others and so forth. He applies criteria in performing critically, that is, in trying to get things right.' (p. 29)

In effect, Ryle is suggesting here that the critical process is the essential mark of intelligent performance. Indeed, it could be said that criticism (or feedback) is at the heart of any successful performance. It is at the heart of behaviourist Stimulus-Response psychology, and it is a basic concept in systems theory where unintelligent systems are seen to maintain stability through feedback mechanisms (a thermostat is a typical example). In fields closer to the present enquiry, Kulhavy (1977) has discussed the role of feedback in instruction, and Eco (1976) applies a feedback model to semiological communication. And students of graphic design will confirm from experience that design is easier to criticize (in the non-pejorative sense of the word) than to prescribe.

Whether or not there is a dualism of thought and action is not the issue to us, but there is surely a sense in which the criteria referred to by Ryle can be asked to stand independently from particular actions. To detect lapses or to profit from the example of others implies the existence of some sort of paradigm performance, or set of rules, with which actual performances are compared, although this may remain in the tacit domain. The critical loop (or feedback) may not be articulated in everyday practice by the boxer, surgeon or designer, but that is not to say that it cannot be articulated or might not benefit from being articulated.

### Holistic thinking

Polanyi, from his particular perspective, also recognizes the critical process as central to the growth of both tacit and explicit knowledge. He uses the term 'physiognomy' to refer to situations which can be recognized but not described; the metaphor is that of the infinitely varied instances of the human face which we can identify without being able to articulate. Polanyi argues that defining a physiognomy (which in our own terms might be a design problem, a magazine page or a typeface) will involve two stages: a focal awareness of its particulars and a subsidiary awareness

of those particulars in relation to their participation in the whole. Using a series of examples from ordinary life and science, he argues that most 'knowing' involves an alternation of focal and subsidiary awareness, analysis and integration.<sup>41</sup> Interestingly, Polanyi cites the use of the term 'aesthetic recognition' by Pantin (1954) in relation to the recognition of species by zoologists. Pantin's fascinating paper describes how biologists in the field

'cannot help being struck by the contrast between the way one identifies...animals in the museum and the way it is done in the field'.

In the case of his own speciality (a species of small worm), it is not possible in the field to analyse specimens feature-by-feature against a recognition checklist. Instead,

'if, when we are collecting *Rhynchodemus bilineatus* together, I say "Bring me any worms that sneer at you," the probability of your collecting the right species is high.' (p. 593)

Among the more theoretically inclined of graphic designers the term 'aesthetic' has become somewhat discredited, with its implication that one personal preference is as good as any other. Pantin's use of the term, though, suggests that it is more sensibly used to describe the feeling of recognizing the physiognomy of, in the typographic context, a well-formed letter or a problem solved. Unaccompanied by an articulated critical process though, designers can be easily deceived by what we might term aesthetic fallacies—false symmetries and alignments, for example.

This holistic style of thinking need not be accepted uncritically. Although the context was somewhat different to the present one, in the course of an attack on historicist and Utopian social policy Popper (1957) makes an important distinction between two kinds of 'wholes': first, the literal totality of all the components of a system and their relations; and, second, certain properties of a system which make it more than the sum of its parts—the *gestalts* of that school of psychology. The fact that the second

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<sup>41</sup> Polanyi carefully distances himself from Ryle's 'absurd' conclusion to 'his [Ryle's] powerful arguments'. Instead of dismissing dualism out of hand, Polanyi prefers to 'dispose of the Cartesian dilemma by acknowledging two mutually exclusive ways of being aware of our body'.

sort of whole can be studied scientifically does not mean that the same can be said of the first sort. For Popper, science—indeed, all description—is inherently selective. Gestalts, although holistic properties, are not the only properties, or even the only holistic properties, of systems. Popper cites melody and rhythm as examples of co-existing holistic properties in music. The lesson for typographic theory is that we can expect to find similarly co-existent holistic properties of typographic displays which, although they may be hard to reconcile in terms of explicit theory, may be as easy as music for readers to discern and designers to create, using tacit knowledge.

### The specialization of scholarship

We may reduce many of the issues raised in this chapter to two key problem areas: one is the specialization of scholarship, with its distinctions between disciplines, between pure and applied research, and between systems and components; the second is the communication of practical advice, with its associated problems of balancing theory, guidelines and examples, and establishing a critical method.

The scholar's job is to analyse and categorize messy real world situations into sub-problems that can be handled by detailed methodologies. Conversely, the user of scholarship has to synthesize the different views and theories into his or her tacit understanding. So although an analytic approach may be necessary, frequent reference must be made to the holistic context of each sub-problem if the research is to be useful.

Holistic overviews are themselves selective, though, in their view of the world, representing not the totality of all sense-data in a system, but only those data relevant to a particular theme or paradigm. Thus the psychologist, the philosopher and the linguist all claim to study the structure of knowledge at various different levels of analysis, but each takes a distinctive holistic quality as primary.

A problem for the typographer seeking a rationale is that commitment to a single scholarly paradigm, whether psychological, linguistic or otherwise, seems unduly restrictive. A design problem (or a design product) derives constraints from a range of sources and so is likely to have more than one physiognomy—like the impossible figure in Figure 2.2. While a goal of this investigation is to point towards a critical method, we must not expect to find a single paradigm. Instead, I shall be discussing a range of alternative critical paradigms (which I shall term ‘structures’) each deriving from a different source of design constraints. It will be suggested that the nature of designing, and interpreting design, is to attain the skill of multiple (as distinct from selective) perceptions, and to balance or harmonize their competing demands.

Here it may be objected that I am drawing an inappropriate parallel between the way that scientists view communication and the perceptions of ordinary people trying to communicate. After all, while scientists construct formal languages to make ideas publicly accessible, in everyday conversation we use natural language, simultaneously drawing on linguistic, logical, social and aesthetic resources in an automatic way that does not call for an explicit critical method. However, the relationship between writer and reader is asymmetric in a way that the relationship between speaker and hearer is generally not. In a typical spoken conversation, both speaker and hearer can use natural language. Written language, though, differs from spoken language in its provenance as well as its channel. In its printed form it is typically mediated by editors, designers, printers and others. Although the result may be interpreted ‘naturally’ by readers who are unaware of the production process, the assembly of printed texts is a deliberate, planned formal process in which a range of competing demands and constraints must be carefully balanced.

But it is not just at the producers of text that this multi-faceted critical method would be aimed. Classical rhetoric not only taught orators to make persuasive speeches, but it also enabled listeners to spot logical tricks and



biased arguments. In the same way, a critical method for typography is not just for the refinement of technique, but might also represent an extension of literacy for readers. The greater the critical awareness of all aspects of text, the greater chance readers have of exercising control over their reading in terms of both strategy and outcomes. Ivins (1943), with typical insight, linked the critical awareness of media (printed illustrations, in his case) to a more general critical awareness:

‘In view of all this [evidence he has just presented] the importance of being able to recognise the technique or process by which a printed or otherwise precisely duplicable image was made becomes obvious—for this knowledge enables us to discount or make allowances for the limitations, the blind spots, the distortions, implicitly and unknowingly introduced by techniques and processes into duplicate images and their testimony about the world. These implicit distortions are a most important part of the unconscious, unphrased, common assumptions of any society, which basically determines its ideas and action. Very few people ever realize the extent to which “objective facts” as known by us are actually no more than peculiarities of our instruments of observation and record.’ (p. 143)

In any case, there is something to be said for treating scientists and lay persons, users of formal and natural language respectively, in the same way. The psychologist George Kelly (1955) has recounted how he developed the concept of ‘man the scientist’, central to his theory of personal constructs. His appointments during a typical working day would alternate between his psychotherapy patients and his post-graduate students. He came to realize that the patients were asking much the same sort of analytical questions about their personal relationships as the students were asking about their projects. By providing them with the Repertory Grid technique with which to analyse their perception of personal relationships (the same analytical tool he recommended to his students), Kelly enabled his patients to come to a more objective understanding of their problems from the scientist’s critical distance.<sup>42</sup>

I have suggested that, in this context at least, the same Kuhnian paradigms might serve practical as well as scholarly purposes. But where do paradigms come from? According to Kuhn:

‘Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognise as acute.’ (p. 23, my italics)

Although Kuhn means ‘practitioners’ to refer to scientists, in the absence of a science we might adapt his argument to mean those who are most deeply engaged in the field of enquiry, or indeed the field of practice. In effect, Kuhn is suggesting that paradigms grow from a critical tradition.

The term ‘critical tradition’ is apt, implying both a historical dimension and an evaluative approach. The historical dimension informs about possible connotations of candidate solutions to a design problem, and provides models for particular genres of text. Evaluation is suggestive both of minimum standards (catastrophe avoidance) and an ideal or typical model against which design solutions may be measured, and towards which guidelines might point. The model of typographic communication proposed in Chapter 5 is directed towards this end. First, however, I shall return to consider linguistic aspects of typographic study in more detail, since, as became apparent in Chapter 1, it appears to be the most likely source of insight. Whatever else typography is, it is a quality of language.

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<sup>42</sup> Kelly’s technique has been applied across a very wide range of subject areas, including several relevant to this study—for example, study methods (Thomas & Harri-Augstein 1980), and the expressive properties of type design (Bartram 1982).