making sense of uncertainty, performing artistically, setting problems, and choosing among competing professional paradigms, when these processes seem mysterious in the light of the prevailing model of professional knowledge.

We are bound to an epistemology of practice which leaves us at a loss to explain, or even to describe, the competences to which we now give overriding importance.

2

From Technical Rationality to Reflection-in-Action

The Dominant Epistemology of Practice

According to the model of Technical Rationality—the view of professional knowledge which has most powerfully shaped both our thinking about the professions and the institutional relations of research, education, and practice—professional activity consists in instrumental problem solving made rigorous by the application of scientific theory and technique. Although all occupations are concerned, on this view, with the instrumental adjustment of means to ends, only the professions practice rigorously technical problem solving based on specialized scientific knowledge.

The model of Technical Rationality has exerted as great an influence on scholarly writing about the professions as on critical exposés of the role of the professions in the larger society. In the 1930s, for example, one of the earliest students of the professions asserted that

> it is not difficult to account in general for the emergence of the new professions. Large-scale organization has favored specialization. Specialized occupations have arisen around the new scientific knowledge.[1]

In a major book on the professions, published in 1970, Wilbert Moore embraced Alfred North Whitehead's distinction between a profession and an avocation. An avocation is "the antithesis to a profession" because it is "based upon customary activities and modified by the trial and error of individual practice."[2] In contrast, Moore said, a profession

> involves the application of general principles to specific problems, and it is a feature of modern societies that such general principles are abundant and growing.[3]

The same author argues further that professions are highly specialized occupations, and that

> the two primary bases for specialization within a profession are (1) the substantive field of knowledge that the specialist professes to command and (2) the technique of production or application of knowledge over which the specialist claims mastery.[4]

Finally, a recent critic of professional expertise sees the professional's claim to uniqueness as a " . . . preoccupation with a specialized skill premised on an underlying theory."[5]

The prototypes of professional expertise in this sense are the "learned professions" of medicine and law and, close behind these, business and engineering. These are, in Nathan Glazer's terms, the "major" or "near-major" professions.[6] They are distinct from such "minor" professions as social work, librarianship, education, divinity, and town planning. In the essay from which these terms are drawn, Glazer argues that the schools of the minor professions are hopelessly nonrigorous, dependent on representatives of academic disciplines, such as economics or political science, who are superior in status to the professions themselves. But what is of greatest interest from our point of view, Glazer's distinction between major and minor professions rests on a particularly well-articulated version of the model of Technical Rationality. The major professions are "disciplined by an unambiguous end—health, success in litigation, profit—which settles men's minds,"[7] and they operate in stable institutional contexts. Hence they are grounded in systematic, fundamental knowledge, of which scientific knowledge is the prototype,[8] or else they have "a high component of strictly technological knowledge based on science in the education which they provide."[9] In contrast, the minor professions suffer from shifting, ambiguous ends and from unstable institutional contexts of practice, and are *therefore* unable to develop a base of systematic, scientific professional knowledge. For Glazer, the development of a scientific knowledge base depends on fixed, unambiguous ends because professional practice is an instrumental activity. If applied science consists in cumulative, empirical knowledge about the means best suited to chosen ends, how can a profession ground itself in science when its ends are confused or unstable?

The systematic knowledge base of a profession is thought to have four essential properties. It is specialized, firmly bounded, scientific, and standardized. This last point is particularly impottant, because it bears on the paradigmatic relationship which holds, according to Technical Rationality, between a profession's knowledge base and its practice. In Wilbert Moore's words,

> If every professional problem were in all respects unique, solutions would be at best accidental, and therefore have nothing to do with expert knowledge. What we are suggesting, on the contrary, is that there are sufficient uniformities in problems and in devices for solving them to qualify the solvers as professionals . . . professionals apply very general principles, *standardized* knowledge, to concrete problems . . . [10]

This concept of "application" leads to a view of professional knowledge as a hierarchy in which "general principles" occupy the highest level and "concrete problem solving" the lowest. As Edgar Schein has put it,[11] there are three components to professional knowledge:

1. An *underlying discipline* or *basic science* component upon which the practice rests or from which it is developed.
2. An *applied science* or *"engineering"* component from which many of the day-to-day diagnostic procedures and problem-solutions are derived.
3. A *skills and attitudinal* component that concerns the actual performance of services to the client, using the underlying basic and applied knowledge.[12]

The application of basic science yields applied science. Applied science yields diagnostic and problem-solving techniques which are applied in turn to the actual delivery of services. The order of application is also an order of derivation and dependence. Applied science is said to "rest on" the foundation of basic science. And the more basic and general the knowledge, the higher the status of its producer.

When the representatives of aspiring professions consider

the problem of rising to full professional status, they often ask whether their knowledge base has the requisite properties and whether it is regularly applied to the everyday problems of practice. Thus, in an article entitled "The Librarian: From Occupation to Profession,"[13] the author states that

> the central gap is of course the failure to develop a general body of scientific knowledge bearing precisely on this problem, in the way that the medical profession with its auxiliary scientific fields has developed an immense body of knowledge with which to cure human diseases.

The sciences in which he proposes to ground his profession are "communications theory, the sociology or psychology of mass communications, or the psychology of learning as it applies to reading."[14] Unfortunately, however, he finds that

> most day-to-day professional work utilizes rather concrete rule-of-thumb local regulations and rules and major catalog systems . . . The problems of selection and organization are dealt with on a highly empiricist basis, concretely, with little reference to general scientific principles.[15]

And a social worker, considering the same sort of question, concludes that "social work is already a profession" because it has a basis in

> theory construction via systematic research. To generate valid theory that will provide a solid base for professional techniques requires the application of the scientific method to the service-related problems of the profession. Continued employment of the scientific method is nurtured by and in turn reinforces the element of *rationality* . . . [16]

It is by progressing along this route that social work seeks to rise within the professional hierarchy so that it, too, might enjoy maximum prestige, authority, and monopoly which presently belong to a few top professions."[17]

If the model of Technical Rationality appeared only in such statements of intent, or in programmatic descriptions of professional knowledge, we might have some doubts about its dominance. But the model is also embedded in the institutional context of professional life. It is implicit in the institutionalized relations of research and practice, and in the normative curricula of professional education. Even when practitioners, educators, and researchers question the model of technical rationality, they are party to institutions that perpetuate it.

As one would expect from the hierarchical model of professional knowledge, research is institutionally separate from practice, connected to it by carefully defined relationships of exchange. Researchers are supposed to provide the basic and applied science from which to derive techniques for diagnosing and solving the problems of practice. Practitioners are supposed to furnish researchers with problems for study and with tests of the utility of research results. The researcher's role is distinct from, and usually considered superior to, the role of the practitioner.

> In the evolution of every profession there emerges the researcher-theoretician whose role is that of scientific investigation and theoretical systematization. In technological professions, a division of labor thereby evolves between the theory-oriented and the practice-oriented person. Witness the physician who prefers to attach himself to a medical research center rather than to enter private practice . . . [18]

In a similar vein, Nathan Glazer speaks of the sociologist, political scientist, or economist who, when he is invited to bring his discipline to the school of a minor profession, manifests a level

of status disturbingly superior to that of the resident practitioners. And in schools of engineering, which have been transformed into schools of engineering science, the engineering scientist tends to place his superior status in the service of values different from those of the engineering profession.[19]

The hierarchical separation of research and practice is also reflected in the normative curriculum of the professional school. Here the order of the curriculum parallels the order in which the components of professional knowledge are "applied." The rule is: first, the relevant basic and applied science; then, the skills of application to real-world problems of practice. Edgar Schein's study of professional education led him to describe the dominant curricular pattern as follows:

> Most professional school curricula can be analyzed in terms of the form and timing of these three elements [of professional knowledge]. Usually the professional curriculum starts with a common science core followed by the applied science elements. The attitudinal and skill components are usually labelled "practicum" or "clinical work" and may be provided simultaneously with the applied science components or they may occur even later in the professional education, depending upon the availability of clients or the ease of simulating the realities that the professional will have to face.[20]

Schein's use of the term "skill" is of more than passing interest. From the point of view of the model of Technical Rationality institutionalized in the professional curriculum, real knowledge lies in the theories and techniques of basic and applied science. Hence, these disciplines should come first. "Skills" in the use of theory and technique to solve concrete problems should come later on, when the student has learned the relevant science—first, because he cannot learn skills of application until he has learned applicable knowledge; and secondly, because skills are an ambiguous, secondary kind of knowledge. There is something disturbing about calling them "knowledge" at all.

Again, medicine is the prototypical example. Ever since the Flexner Report, which revolutionized medical education in the early decades of this century, medical schools have devoted the first two years of study to the basic sciences—chemistry, physiology, pathology—as "the appropriate foundation for later clinical training."[21] Even the physical arrangement of the curriculum reflects the basic division among the elements of professional knowledge:

> The separation of the medical school curriculum into two disjunctive stages, the preclinical and the clinical, reflects the division between theory and practice. The division also appears in the location of training and in medical school facilities. The sciences of biochemistry, physiology, pathology and pharmacology are learned from classrooms and laboratories, that is, in formal academic settings. More practical training, in clinical arts such as internal medicine, obstetrics and pediatrics, takes place in hospital clinics, within actual institutions of delivery.[22]

And teaching roles tend to reflect the same division:

> Medical school faculties tend to be divided between the PhD's and MD's, between teachers of basic science and those in clinical programs.[23]

Even though the law might be thought to have a dubious basis in science, the introduction of the still-dominant pattern of legal education—by Christopher Columbus Langdell at Harvard University in the 1880s and 1890s—followed the normative curricular model. In his address before the Harvard Law School in 1886, Langdell argued that "first, law is a science and secondly . . . all available materials of that science are con-

tained in printed books."[24] Langdell claimed that legal education is better conducted in a law school than in a lawyer's office because legal study is based upon broad, scientifically determined principles which cut across state lines.

> For Langdell claimed law was a science . . . this meant that its principles could be developed from analysis of prior court decisions and could be used to predict subsequent ones. Just as Charles William Eliot was introducing the experimental laboratory into the study of natural sciences at Harvard, so it was Langdell's claim, with the study of previously decided cases.[25]

Even the famous "case method" was originally grounded in the belief that the teaching of scientific principles should precede the development of skills in their application.

In his recent review of the Harvard School of Business Administration, the school which first adapted Langdell's method to management education, Derek Bok, the current president of Harvard University, argues against case method. His argument reveals both his implicit belief in the normative curriculum of professional education and his adherence to the model of technical rationality.

Bok begins by noting that case teaching has certainly helped to keep professors "closely involved with the activities of real corporations" and has "forced them to work continuously at their teaching."[26] But he worries that

> although the case is an excellent device for teaching students to apply theory and technique, it does not provide an ideal way of communicating concepts and analytic methods in the first instance.[27]

Exclusive concentration on cases leaves students little time to master analytic technique and conceptual material"—a limitation that has become more critical as "the corporate world grows more complex"—and it prevents faculty from engaging in "intensive work to develop better generalizations, theories and methods that can eventually be used to attack corporate problems in more effective ways."[28] What is especially interesting in this argument is its misreading of what many business case teachers would consider the heart of their teaching: carefully guided analysis of innumerable cases drawn from real-world business contexts in order to help students develop the generic problem-solving skills essential to effective management. Although some of the strongest advocates of case teaching admit that they cannot define these skills or relate them to general theory, they believe that the case method stands on its own unique merits.[29] President Bok has made a contrary assumption. He assumes that the business school faculty accepts both the mission to develop "better generalizations, theories and methods" and the normative idea of a curriculum which places general principles and methods before the skills of application. To faculty members who think they are engaged in a very different sort of educational enterprise, he argues from an unquestioned belief in a normative curriculum which derives from the model of Technical Rationality.

The Origins of Technical Rationality

It is striking that the dominant model of professional knowledge seems to its proponents to require very little justification. How comes it that in the second half of the twentieth century we find in our universities, embedded not only in men's minds but in the institutions themselves, a dominant view of professional knowledge as the application of scientific theory and technique to the instrumental problems of practice?

The answer to this question lies in the last three hundred years of the history of Western ideas and institutions. Technical Rationality is the heritage of Positivism, the powerful philosophical doctrine that grew up in the nineteenth century as an account of the rise of science and technology and as a social movement aimed at applying the achievements of science and technology to the well-being of mankind. Technical Rationality is the Positivist epistemology of practice. It became institutionalized in the modern university, founded in the late nineteenth century when Positivism was at its height, and in the professional schools which secured their place in the university in the early decades of the twentieth century.

Because excellent accounts of this story exist elsewhere,[30] I shall only touch on its main points here.

Since the Reformation, the history of the West has been shaped by the rise of science and technology and by the industrial movement which was both cause and consequence of the increasingly powerful scientific world-view. As the scientific world-view gained dominance, so did the idea that human progress would be achieved by harnessing science to create technology for the achievement of human ends. This Technological Program,[31] which was first vividly expressed in the writings of Bacon and Hobbes, became a major theme for the philosophers of the Enlightenment in the eighteenth century, and by the late nineteenth century had been firmly established as a pillar of conventional wisdom. By this time, too, the professions had come to be seen as vehicles for the application of the new sciences to the achievement of human progress. The engineers, closely tied to the development of industrial technology, became a model of technical practice for the other professions. Medicine, a learned profession with origins in the medieval universities, was refashioned in the new image of a science-based technique for the preservation of health. And statecraft came to be seen as a kind of social engineering. As the professions evolved and proliferated, they became, increasingly, the principal agents of the Technological Program.

As the scientific movement, industrialism, and the Technological Program became dominant in Western society, a philosophy emerged which sought both to give an account of the triumphs of science and technology and to purge mankind of the residues of religion, mysticism, and metaphysics which still prevented scientific thought and technological practice from wholly ruling over the affairs of men. It was in this spirit that, in the first half of the nineteenth century, Auguste Comte first expressed the three principal doctrines of Positivism. First, there was the conviction that empirical science was not just a form of knowledge but the only source of positive knowledge of the world. Second, there was the intention to cleanse men's minds of mysticism, superstition, and other forms of pseudo-knowledge. And finally, there was the program of extending scientific knowledge and technical control to human society, to make technology, as Comte said, "no longer exclusively geometrical, mechanical or chemical, but also and primarily political and moral."[32]

By late nineteenth century, Positivism had become a dominant philosophy. And in the early twentieth century, in the theories of the Vienna Circle, its epistemological program took on a beguiling clarity. Meaningful propositions were held to be of two kinds, either the analytic and essentially tautological propositions of logic and mathematics, or the empirical propositions which express knowledge of the world. The truth of the former was to be grounded in the fact that their negation implies a self-contradiction; the truth of the latter, in some relevant empirical observation. The only significant statements about the world were those based on empirical observation, and all disagreements about the world could be resolved, in principle, by reference to observable facts. Propositions which were neither analytically nor empirically testable were held to have no meaning at all. They were dismissed as emotive utterance, poetry, or mere nonsense.

As Positivists became increasingly sophisticated in their efforts to explain and justify the exclusivity of scientific knowledge, they recognized to what extent observational statements were theory-laden, and found it necessary to ground empirical knowledge in irreducible elements of sensory experience. They began to see laws of nature not as facts inherent in nature but as constructs created to explain observed phenomena, and science became for them a hypothetico-deductive system. In order to account for his observations, the scientist constructed hypotheses, abstract models of an unseen world which could be tested only indirectly through deductions susceptible to confirmation or disconfirmation by experiment. The heart of scientific inquiry consisted in the use of crucial experiments to choose among competing theories of explanation.

In the light of such Positivist doctrines as these, practice appeared as a puzzling anomaly. Practical knowledge exists, but it does not fit neatly into Positivist categories. We cannot readily treat it as a form of descriptive knowledge of the world, nor can we reduce it to the analytic schemas of logic and mathematics. Positivism solved the puzzle of practical knowledge in a way that had been foreshadowed by the Technological Program and by Comte's program for applying science to morality and politics. Practical knowledge was to be construed as knowledge of the relationship of means to ends. Given agreement about ends,[33] the question, "How ought I to act?" could be reduced to a merely instrumental question about the means best suited to achieve one's ends. Disagreement about means could be resolved by reference to facts concerning the possible means, their relevant consequences, and the methods for comparing them with respect to the chosen ends of action. Ultimately, the instrumental question could be resolved by recourse to experiment. And as men built up scientific understandings of cause and effect, causal relationships could be mapped onto instrumental ones. It would be possible to select the means appropriate to one's ends by applying the relevant scientific theory. The question, "How ought I to act?" could become a scientific one, and the best means could be selected by the use of science-based technique.

In the late nineteenth and early twentieth centuries, the professions of engineering and medicine achieved dramatic successes in reliably adjusting means to ends and became models of instrumental practice. The engineer's design and analysis of materials and artifacts, the physician's diagnosis and treatment of disease, became prototypes of the science-based, technical practice which was destined to supplant craft and artistry. For according to the Positivist epistemology of practice, craft and artistry had no lasting place in rigorous practical knowledge.

Universities came of age in the United States, assumed their now familiar structure and styles of operation, in the late nineteenth and early twentieth centuries when science and technology were on the rise and the intellectual hegemony of Positivism was beginning to be established. Although other traditions of thought were never wholly extinguished in American universities—indeed, in some places managed to preserve a kind of local dominance—nevertheless, in the United States more than in any other nation except Germany, the very heart of the university was given over to the scientific enterprise, to the ethos of the Technological Program, and to Positivism.

Indeed, it was from the Germanic tradition, carried to the United States after the Civil War by young American graduates of the German universities, that the new concept of the university as a multidisciplinary research institution took root in the United States, first in Johns Hopkins University, the founding of which was "perhaps the most decisive single event in the history of learning in the Western hemisphere."[34] And it was from the model of Johns Hopkins that other universities began to mold themselves around the German ideal and to manifest, as Edward Shils has written,

> a drift of opinion [toward] . . . the appreciation of knowledge, particularly knowledge of a scientific character. There was general agreement that knowledge could be accepted as knowledge only if it rested on empirical evidence, rigorously criticized and rationally analyzed . . . The knowledge which was appreciated was secular knowledge which continued the mission of sacred knowledge, complemented it, led to it, or replaced it; fundamental, systematically acquired knowledge was thought in some way to be a step toward redemption. This kind of knowledge held out the prospect of the transfiguration of life by improving man's control over the resources of nature and over the powers that weaken his body; it offered the prospect of better understanding of society which it was thought would lead to the improvement of society.[35]

With the coming of the new model of the university, the Positivist epistemology found expression in normative ideas about the proper division of labor between the university and the professions. As Thorsten Veblen argued in *The Higher Learning in America*, "The difference between the modern university and the lower and professional schools is broad and simple; not so much a difference of degree as of kind."[36] The universities have a higher mission to "fit men for a life of science and scholarship; and [they are] accordingly concerned with such discipline only as they will give efficiency in the pursuit of knowledge"; whereas the lower schools are occupied with "instilling such knowledge and habits as will make their pupils fit citizens of the world in whatever position in the fabric of workday life they may fall."[37] The proper relation between the higher and lower schools is one of separation and exchange. Quite simply, the professions are to give their practical problems to the university, and the university, the unique source of research, is to give back to the professions the new scientific knowledge which it will be their business to apply and test. Under no conditions are the technical men of the lower schools to be allowed into the university, for this would put them in a false position

> which unavoidably leads them to court a specious appearance of scholarship and so to invest their technological discipline with a degree of pedantry and sophistication; whereby it is hoped to give these schools and their work some scientific and scholarly prestige.[38]

Veblen's battle was, of course, quixotic. The evils against which he railed at the University of Chicago in 1916 were harbingers of a general trend. The survival-oriented interests of the professions reinforced the interest of university boards of governors in appropriating schools of useful knowledge. The professions did enter the new universities, in increasing numbers, until by 1963 Bernard Barber could write in *Daedalus* that "nearly all the well-established professions are located in the universities."[39]

But for this, the professionalizing occupations paid a price. They had to accept the Positivist epistemology of practice which was now built into the very tissue of the universities. And they had also to accept the fundamental division of labor on which Veblen had placed so great an emphasis. It was to be the business of university-based scientists and scholars to create the fundamental theory which professionals and technicians would apply to practice. The function of the professional school would be

> the transmission to its students of the generalized and systematic knowledge that is the basis of professional performance.[40]

But this division of labor reflected a hierarchy of kinds of knowledge which was also a ladder of status. Those who create new theory were thought to be higher in status than those who apply it, and the schools of "higher learning" were thought to be superior to the "lower."

Thus were planted the seeds of the Positivist curriculum, typical of professional schools in American universities, and the roots of the now-familiar split between research and practice.

Emerging Awareness of the Limits of Technical Rationality

Although it was in the early decades of the twentieth century that occupations professionalized and professional schools sought their places in the universities, it was World War II that gave a major new impetus both to the Technological Program and to the Positivist epistemology of practice.

In World War II, technologists drew upon scientific research as never before. Vannevar Bush created the first large-scale national research and development institute, the National Research and Development Corporation. The new discipline of operations research grew out of the American and British efforts to use applied mathematics for bomb tracking and submarine search. And the Manhattan project became the very symbol of the successful use of science-based technology for national ends. Its lesson seemed to be this: If a great social objective could be clearly defined, if a national commitment to it could be mustered, if unlimited resources could be poured into the necessary research and development, then any such objective could be achieved. The greatest beneficiary of this lesson was the institution of research and development itself. But as a side effect, there was also a reinforcement of the idea of scientific research as a basis for professional practice.

Following World War II, the United States government began an unparalleled increase in the rate of spending for research. As government spending for research increased, research institutions proliferated. Some were associated with the universities, others stood outside them. All were organized around the production of new scientific knowledge and were largely promoted on the basis of the proposition that the production of new scientific knowledge could be used to create wealth, achieve national goals, improve human life, and solve social problems. Nowhere was the rate of increase in research spending more dramatic, and nowhere were the results of that spending more visible, than in the field of medicine. The great centers of medical research and teaching were expanded, and new ones were created. The medical research center, with its medical school and its teaching hospital, became the institutional model to which other professions aspired. Here was a solid base of fundamental science, an equally solid body of applied clinical science, and a profession which had geared itself to implement the ever-changing products of research. Other professions, hoping to achieve some of medicine's effectiveness and prestige, sought to emulate its linkage of research and teaching institutions, its hierarchy of research and clinical roles, and its system for connecting basic and applied research to practice.

The prestige and apparent success of the medical and engineering models exerted a great attraction for the social sciences. In such fields as education, social work, planning, and

policy making, social scientists attempted to do research, to apply it, and to educate practitioners, all according to their perceptions of the models of medicine and engineering. Indeed, the very language of social scientists, rich in references to measurement, controlled experiment, applied science, laboratories, and clinics, was striking in its reverence for these models.

In the mid-1950s, the Soviet launching of Sputnik gave a further impetus to national investment in science and technology. Sputnik shocked America into increased support for science, especially basic science, and created a new sense of urgency about the building of a society based on science. Suddenly we became acutely aware of a national shortage of professionals—scientists and engineers, but also physicians and teachers—who were seen as necessary to the development and application of scientific knowledge. It was the cumulative impact of these national responses to World War II and Sputnik which set the stage for the triumph of professionalism, the triumph celebrated in the *Daedalus* issue of 1963.

Between 1963 and 1982, however, both the general public and the professionals have become increasingly aware of the flaws and limitations of the professions. As I have pointed out in chapter 1, the professions have suffered a crisis of legitimacy rooted both in their perceived failure to live up to their own norms and in their perceived incapacity to help society achieve its objectives and solve its problems. Increasingly we have become aware of the importance to actual practice of phenomena—complexity, uncertainty, instability, uniqueness, and value-conflict—which do not fit the model of Technical Rationality. Now, in the light of the Positivist origins of Technical Rationality, we can more readily see why these phenomena are so troublesome.

From the perspective of Technical Rationality, professional practice is a process of problem *solving*. Problems of choice or decision are solved through the selection, from available means, of the one best suited to established ends. But with this emphasis on problem solving, we ignore problem *setting,* the process by which we define the decision to be made, the ends to be achieved, the means which may be chosen. In real-world practice, problems do not present themselves to the practitioner as givens. They must be constructed from the materials of problematic situations which are puzzling, troubling, and uncertain. In order to convert a problematic situation to a problem, a practitioner must do a certain kind of work. He must make sense of an uncertain situation that initially makes no sense. When professionals consider what road to build, for example, they deal usually with a complex and ill-defined situation in which geographic, topological, financial, economic, and political issues are all mixed up together. Once they have somehow decided what road to build and go on to consider how best to build it, they may have a problem they can solve by the application of available techniques; but when the road they have built leads unexpectedly to the destruction of a neighborhood, they may find themselves again in a situation of uncertainty.

It is this sort of situation that professionals are coming increasingly to see as central to their practice. They are coming to recognize that although problem setting is a necessary condition for technical problem solving, it is not itself a technical problem. When we set the problem, we select what we will treat as the "things" of the situation, we set the boundaries of our attention to it, and we impose upon it a coherence which allows us to say what is wrong and in what directions the situation needs to be changed. Problem setting is a process in which, interactively, we *name* the things to which we will attend and *frame* the context in which we will attend to them.

Even when a problem has been constructed, it may escape the categories of applied science because it presents itself as unique or unstable. In order to solve a problem by the application of existing theory or technique, a practitioner must be able to map those categories onto features of the practice situation. When a nutritionist finds a diet deficient in lysine, for example, dietary supplements known to contain lysine can be recommended. A physician who recognizes a case of measles can map it onto a system of techniques for diagnosis, treatment, and prognosis. But a unique case falls outside the categories of applied theory; an unstable situation slips out from under them. A physician cannot apply standard techniques to a case that is not in the books. And a nutritionist attempting a planned nutritional intervention in a rural Central American community may discover that the intervention fails because the situation has become something other than the one planned for.

Technical Rationality depends on agreement about ends. When ends are fixed and clear, then the decision to act can present itself as an instrumental problem. But when ends are confused and conflicting, there is as yet no "problem" to solve. A conflict of ends cannot be resolved by the use of techniques derived from applied research. It is rather through the non-technical process of framing the problematic situation that we may organize and clarify both the ends to be achieved and the possible means of achieving them.

Similarly, when there are conflicting paradigms of professional practice, such as we find in the pluralism of psychiatry, social work, or town planning, there is no clearly established context for the use of technique. There is contention over multiple ways of framing the practice role, each of which entrains a distinctive approach to problem setting and solving. And when practitioners do resolve conflicting role frames, it is through a kind of inquiry which falls outside the model of

Technical Rationality. Again, it is the work of naming and framing that creates the conditions necessary to the exercise of technical expertise.

We can readily understand, therefore, not only why uncertainty, uniqueness, instability, and value conflict are so troublesome to the Positivist epistemology of practice, but also why practitioners bound by this epistemology find themselves caught in a dilemma. Their definition of rigorous professional knowledge excludes phenomena they have learned to see as central to their practice. And artistic ways of coping with these phenomena do not qualify, for them, as rigorous professional knowledge.

This dilemma of "rigor or relevance" arises more acutely in some areas of practice than in others. In the varied topography of professional practice, there is a high, hard ground where practitioners can make effective use of research-based theory and technique, and there is a swampy lowland where situations are confusing "messes" incapable of technical solution. The difficulty is that the problems of the high ground, however great their technical interest, are often relatively unimportant to clients or to the larger society, while in the swamp are the problems of greatest human concern. Shall the practitioner stay on the high, hard ground where he can practice rigorously, as he understands rigor, but where he is constrained to deal with problems of relatively little social importance? Or shall he descend to the swamp where he can engage the most important and challenging problems if he is willing to forsake technical rigor?

In such "major" professions as medicine, engineering, or agronomy there are zones where practitioners can function as technical experts. But there are also zones where the major professions resemble the minor ones. Medical technologies such as kidney dialysis generate demands in excess of the na-

tion's willingness to invest in medical care. Engineering that seems powerful and elegant when judged from a narrowly technical perspective may also carry unacceptable risks to environmental quality or human safety. Large-scale, industrialized agriculture destroys the peasant economies of the developing worlds. How should professionals take account of such issues as these?

There are those who choose the swampy lowlands. They deliberately involve themselves in messy but crucially important problems and, when asked to describe their methods of inquiry, they speak of experience, trial and error, intuition, and muddling through.

Other professionals opt for the high ground. Hungry for technical rigor, devoted to an image of solid professional competence, or fearful of entering a world in which they feel they do not know what they are doing, they choose to confine themselves to a narrowly technical practice.

The field of "formal modelling" offers an interesting context in which to observe the two responses.

During World War II, operations research grew out of the successful use of applied mathematics in submarine search and bomb tracking. After World War II, the development of the digital computer sparked widespread interest in formal, quantitative, computerized models which seemed to offer a new technique for converting "soft" problems into "hard" ones. A new breed of technical practitioner came into being. Systems analysts, management scientists, policy analysts, began to use formal modelling techniques on problems of inventory control, business policy, information retrieval, transportation planning, urban land use, the delivery of medical care, the criminal justice system, and the control of the economy. By the late 1960s, there was scarcely a described problem for which someone had not constructed a computerized model. But in recent years there has been a widening consensus, even among formal modellers, that the early hopes were greatly inflated. Formal models have been usefully employed to solve problems in such relatively undemanding areas as inventory control and logistics. They have generally failed to yield effective results in the more complex, less clearly defined problems of business management, housing policy, or criminal justice.

Formal modellers have responded to this unpleasant discovery in several different ways. Some have continued to ply their trade in the less demanding areas of the field. Some have abandoned their original training in order to address themselves to real-world problems. Others have decided to treat formal models as "probes" or "metaphors" useful only as sources of new perspectives on complex situations. But for the most part, the use of formal models has proceeded as though it had a life of its own. Driven by the evolving questions of theory and technique, formal modelling has become increasingly divergent from the real-world problems of practice. And practitioners who choose to remain on the high ground have continued to use formal models for complex problems, quite oblivious to the troubles incurred whenever a serious attempt is made to implement them.

Many practitioners have adopted this response to the dilemma of rigor or relevance, cutting the practice situation to fit professional knowledge. This they do in several ways. They may become selectively inattentive to data that fall outside their categories. Designers of management information systems may simply avoid noticing, for example, how their systems trigger games of control and evasion. They may use "junk categories" to explain away discrepant data, as technical analysts sometimes attribute the failure of their recommendations to "personality" or to "politics."[41] Or they may try to force the situation into a mold which lends itself to the use of available techniques. Thus an industrial engineer may simplify the actual arrangement of a manufacturing system in order to make it easier to analyze; or, more ominously, members of the helping professions may get rid of clients who resist professional help, relegating them to such categories as "problem tenant" or "rebellious child." All such strategies carry a danger of misreading situations, or manipulating them, to serve the practitioner's interest in maintaining his confidence in his standard models and techniques. When people are involved in the situation, the practitioner may preserve his sense of expertise at his clients' expense.

Some students of the professions have tried to take account of the limitations of technical expertise and have proposed new approaches to the predicament of professional knowledge. Among these are Edgar Schein and Nathan Glazer, whom I have already mentioned, and Herbert Simon, whose *The Sciences of the Artificial* has aroused a great deal of interest in professional circles. Each of these writers has identified a gap between professional knowledge and the demands of real-world practice. Their formulations of the gap are intriguingly different, yet they reveal an important underlying similarity.

To Schein, the gap lies in the fact that basic and applied sciences are "convergent," whereas practice is "divergent." He believes that some professions have already achieved, and that others will eventually achieve, "a high degree of consensus on the paradigms to be used in the analysis of phenomena and . . . what constitutes the relevant knowledge base for practice."[42] Nevertheless, Schein also believes that the problems of professional practice continue to have unique and unpredictable elements. One of the hallmarks of the professional, therefore, is his ability to "take a convergent knowledge base and convert it into professional services that are tailored to the *unique* requirements of the client system," a process which demands "divergent thinking skills."[43] About these, however, Schein has very little to say, and for good reason. If divergent skills could be described in terms of theory or technique, they would belong to one or another of the components of the hierarchy of professional knowledge. But if they are neither theory nor technique, and are still a kind of knowledge, how are they to be described? They must remain a mysterious, residual category.

For Glazer, the critical distinction is between kinds of professions. To professions like medicine and law Glazer attributes fixed and unambiguous ends, stable institutional contexts, and fixed contents of professional knowledge sufficient for rigorous practice. To professions such as divinity and social work he attributes ambiguous ends, shifting contexts of practice, and no fixed content of professional knowledge. Of these professions, he despairs. Thus the gap which Schein locates between "convergent" science and "divergent" practice, Glazer locates between major and minor professions.

It is Simon, however, who most clearly links the predicament of professional knowledge to the historical origins of the Positivist epistemology of practice. Simon believes that all professional practice is centrally concerned with what he calls "design," that is, with the process of "changing existing situations into preferred ones."[44] But design in this sense is precisely what the professional schools do not teach. The older schools have a knowledge of design that is "intellectually soft, intuitive, informal and cookbooky,"[45] and the newer ones, more absorbed into the general culture of the modern university, have become schools of natural science. Thus,

> engineering schools have become schools of physics and mathematics; medical schools have become schools of biological science; business schools have become schools of finite mathematics.[46]

Both older and newer schools have "nearly abdicated responsibility for training in the core professional skill,"[47] in large part because such training would have to be grounded in a science of design which does not yet exist. Simon proposes to build a science of design by emulating and extending the optimization methods which have been developed in statistical decision theory and management science. An optimization problem is a well-formed problem of the following kind:

> A list of foods is provided, the command variables being quantities of the various foods that are to be included in the diet. The environmental parameters are the prices and nutritional contents (calories, vitamins, minerals, and so on) of each of the foods. The utility function is the cost (with a minus sign attached) of the diet, subject to the constraints, say, that it not contain more than 2000 calories per day, that it meet specified minimum needs for vitamins and minerals, and that rutabaga not be eaten more than once a week . . . The problem is to select the quantities of foods that will meet the nutritional requirements and side conditions at the given prices for the lowest cost.[48]

Here, ends have been converted to "constraints" and "utility functions"; means, to "command variables"; and laws, to "environmental parameters." Once problems are well formed in this way, they can be solved by a calculus of decision. As we have seen, however, well-formed instrumental problems are not given but must be constructed from messy problematic situations. Although Simon proposes to fill the gap between natural science and design practice with a science of design, his science can be applied only to well-formed problems already extracted from situations of practice.

Schein, Glazer, and Simon propose three different approaches to the limitations of Technical Rationality and the related dilemma of rigor or relevance. All three employ a common strategy, however. They try to fill the gap between the scientific basis of professional knowledge and the demands of real-world practice in such a way as to preserve the model of Technical Rationality. Schein does it by segregating convergent science from divergent practice, relegating divergence to a residual category called "divergent skill." Glazer does it by attributing convergence to the major professions, which he applauds, and divergence to the minor professions, which he dismisses. Simon does it by proposing a science of design which depends on having well-formed instrumental problems to begin with.

Yet the Positivist epistemology of practice, the model of professional knowledge to which these writers cling, has fallen into disrepute in its original home, the philosophy of science. As Richard Bernstein has written,

> There is not a single major thesis advanced by either nineteenth-century Positivists or the Vienna Circle that has not been devastatingly criticized when measured by the Positivists' own standards for philosophical argument. The original formulations of the analytic-synthetic dichotomy and the verifiability criterion of meaning have been abandoned. It has been effectively shown that the Positivists' understanding of the natural sciences and the formal disciplines is grossly oversimplified. Whatever one's final judgment about the current disputes in the post-empiricist philosophy and history of science . . . there is rational agreement about the inadequacy of the original Positivist understanding of science, knowledge and meaning.[49]

Among philosophers of science no one wants any longer to be called a Positivist, and there is a rebirth of interest in the ancient topics of craft, artistry, and myth—topics whose fate Positivism once claimed to have sealed. It seems clear, however, that the dilemma which afflicts the professions hinges not on science per se but on the Positivist view of science. From this perspective, we tend to see science, after the fact, as a body

of established propositions derived from research. When we recognize their limited utility in practice, we experience the dilemma of rigor or relevance. But we may also consider science before the fact as a process in which scientists grapple with uncertainties and display arts of inquiry akin to the uncertainties and arts of practice.

Let us then reconsider the question of professional knowledge; let us stand the question on its head. If the model of Technical Rationality is incomplete, in that it fails to account for practical competence in "divergent" situations, so much the worse for the model. Let us search, instead, for an epistemology of practice implicit in the artistic, intuitive processes which some practitioners do bring to situations of uncertainty, instability, uniqueness, and value conflict.

Reflection-in-Action

When we go about the spontaneous, intuitive performance of the actions of everyday life, we show ourselves to be knowledgeable in a special way. Often we cannot say what it is that we know. When we try to describe it we find ourselves at a loss, or we produce descriptions that are obviously inappropriate. Our knowing is ordinarily tacit, implicit in our patterns of action and in our feel for the stuff with which we are dealing. It seems right to say that our knowing is *in* our action.

Similarly, the workaday life of the professional depends on tacit knowing-in-action. Every competent practitioner can recognize phenomena—families of symptoms associated with a particular disease, peculiarities of a certain kind of building site, irregularities of materials or structures—for which he cannot give a reasonably accurate or complete description. In his day-to-day practice he makes innumerable judgments of quality for which he cannot state adequate criteria, and he displays skills for which he cannot state the rules and procedures. Even when he makes conscious use of research-based theories and techniques, he is dependent on tacit recognitions, judgments, and skillful performances.

On the other hand, both ordinary people and professional practitioners often think about what they are doing, sometimes even while doing it. Stimulated by surprise, they turn thought back on action and on the knowing which is implicit in action. They may ask themselves, for example, "What features do I notice when I recognize this thing? What are the criteria by which I make this judgment? What procedures am I enacting when I perform this skill? How am I framing the problem that I am trying to solve?" Usually reflection on knowing-in-action goes together with reflection on the stuff at hand. There is some puzzling, or troubling, or interesting phenomenon with which the individual is trying to deal. As he tries to make sense of it, he also reflects on the understandings which have been implicit in his action, understandings which he surfaces, criticizes, restructures, and embodies in further action.

It is this entire process of reflection-in-action which is central to the "art" by which practitioners sometimes deal well with situations of uncertainty, instability, uniqueness, and value conflict.

Knowing-in-action. Once we put aside the model of Technical Rationality, which leads us to think of intelligent practice as an *application* of knowledge to instrumental decisions, there is nothing strange about the idea that a kind of knowing is inherent in intelligent action. Common sense admits the category of know-how, and it does not stretch common sense very much to say that the know-how is *in* the action—that a tightrope walker's know-how, for example, lies in, and is revealed

by, the way he takes his trip across the wire, or that a big-league pitcher's know-how is in his way of pitching to a batter's weakness, changing his pace, or distributing his energies over the course of a game. There is nothing in common sense to make us say that know-how consists in rules or plans which we entertain in the mind prior to action. Although we sometimes think before acting, it is also true that in much of the spontaneous behavior of skillful practice we reveal a kind of knowing which does not stem from a prior intellectual operation.

As Gilbert Ryle has put it,

> What distinguishes sensible from silly operations is not their parentage but their procedure, and this holds no less for intellectual than for practical performances. "Intelligent" cannot be defined in terms of "intellectual" or "knowing *how*" in terms of "knowing *that*"; "thinking what I am doing" does not connote "both thinking what to do and doing it." When I do something intelligently . . . I am doing one thing and not two. My performance has a special procedure or manner, not special antecedents.[50]

And Andrew Harrison has recently put the same thought in this pithy phrase: when someone acts intelligently, he "acts his mind."[51]

Over the years, several writers on the epistemology of practice have been struck by the fact that skillful action often reveals a "knowing more than we can say." They have invented various names for this sort of knowing, and have drawn their examples from different domains of practice.

As early as 1938, in an essay called "Mind in Everyday Affairs," Chester Barnard distinguished "thinking processes" from "non-logical processes" which are not capable of being expressed in words or as reasoning, and which are only made known by a judgment, decision, or action.[52] Barnard's examples include judgments of distance in golf or ball-throwing, a high-school boy solving quadratic equations, and a practiced accountant who can take "a balance sheet of considerable complexity and within minutes or even seconds get a significant set of facts from it."[53] Such processes may be unconscious or they may occur so rapidly that "they could not be analyzed by the persons in whose brain they take place."[54] Of the high-school mathematician, Barnard says, memorably, "He could not write the text books which are registered in his mind."[55] Barnard believes that our bias toward thinking blinds us to the non-logical processes which are omnipresent in effective practice.

Michael Polanyi, who invented the phrase "tacit knowing," draws examples from the recognition of faces and the use of tools. If we know a person's face, we can recognize it among a thousand, indeed, among a million, though we usually cannot tell how we recognize a face we know. Similarly, we can recognize the moods of the human face without being able to tell, "except quite vaguely,"[56] by what signs we know them. When we learn to use a tool, or a probe or stick for feeling our way, our initial awareness of its impact on our hand is transformed "into a sense of its point touching the objects we are exploring."[57] In Polanyi's phrase, we attend "from" its impact on our hand "to" its effect on the things to which we are applying it. In this process, which is essential to the acquisition of a skill, the feelings of which we are initially aware become internalized in our tacit knowing.

Chris Alexander, in his *Notes Toward a Synthesis of Form*,[58] considers the knowing involved in design. He believes that we can often recognize and correct the "bad fit" of a form to its context, but that we usually cannot describe the rules by which we find a fit bad or recognize the corrected form to be good. Traditional artifacts evolve culturally through successive detections and corrections of bad fit until

the resulting forms are good. Thus for generations the Slovakian peasants made beautiful shawls woven of yarns which had been dipped in homemade dyes. When aniline dyes were made available to them, "the glory of the shawls was spoiled."[59] The shawlmakers had no innate ability to make good shawls but "were simply able, as many of us are, to recognize bad shawls and their own mistakes. Over the generations . whenever a bad one was made, it was recognized as such, and therefore not repeated."[60] The introduction of aniline dyes disrupted the cultural process of design, for the shawlmakers could not produce wholly new designs of high quality; they could only recognize "bad fit" within a familiar pattern.

Ruminating on Alexander's example, Geoffrey Vickers points out that it is not only artistic judgments which are based on a sense of form which cannot be fully articulated:

> artists, so far from being alone in this, exhibit most clearly an oddity which is present in all such judgments. We can recognize and describe deviations from a norm very much more clearly than we can describe the norm itself.[61]

For Vickers, it is through such tacit norms that all of us make the judgments, the qualitative appreciations of situations, on which our practical competence depends.

Psycholinguists have noted that we speak in conformity with rules of phonology and syntax which most of us cannot describe.[62] Alfred Schultz and his intellectual descendants have analyzed the tacit, everyday know-how that we bring to social interactions such as the rituals of greeting, ending a meeting, or standing in a crowded elevator.[63] Birdwhistell has made comparable contributions to a description of the tacit knowledge embodied in our use and recognition of movement and gesture.[64] In these domains, too, we behave according to rules and procedures that we cannot usually describe and of which we are often unaware.

In examples like these, knowing has the following properties:

- There are actions, recognitions, and judgments which we know how to carry out spontaneously; we do not have to think about them prior to or during their performance.
- We are often unaware of having learned to do these things; we simply find ourselves doing them.
- In some cases, we were once aware of the understandings which were subsequently internalized in our feeling for the stuff of action. In other cases, we may never have been aware of them. In both cases, however, we are usually unable to describe the knowing which our action reveals.

It is in this sense that I speak of knowing-*in*-action, the characteristic mode of ordinary practical knowledge.

Reflecting-in-action. If common sense recognizes knowing-in-action, it also recognizes that we sometimes think about what we are doing. Phrases like "thinking on your feet," "keeping your wits about you," and "learning by doing" suggest not only that we can think about doing but that we can think about doing something while doing it. Some of the most interesting examples of this process occur in the midst of a performance.

Big-league baseball pitchers speak, for example, of the experience of "finding the groove":

> Only a few pitchers can control the whole game with pure physical ability. The rest have to learn to adjust once they're out there. If they can't, they're dead ducks.
>
> [You get] a special feel for the ball, a kind of command that lets you repeat the exact same thing you did before that proved successful.
>
> Finding your groove has to do with studying those winning habits and trying to repeat them every time you perform.[65]

I do not wholly understand what it means to "find the groove." It is clear, however, that the pitchers are talking about a particular kind of reflection. What is "learning to adjust once you're out there"? Presumably it involves noticing how you have been pitching to the batters and how well it has been working, and on the basis of these thoughts and observations, changing the way you have been doing it. When you get a "feel for the ball" that lets you "repeat the exact same thing you did before that proved successful," you are noticing, at the very least, that you have been doing something right, and your "feeling" allows you to do that something again. When you "study those winning habits," you are thinking about the know-how that has enabled you to win. The pitchers seem to be talking about a kind of reflection on their patterns of action, on the situations in which they are performing, and on the know-how implicit in their performance. They are reflecting *on* action and, in some cases, reflecting *in* action.

When good jazz musicians improvise together, they also manifest a "feel for" their material and they make on-the-spot adjustments to the sounds they hear. Listening to one another and to themselves, they feel where the music is going and adjust their playing accordingly. They can do this, first of all, because their collective effort at musical invention makes use of a schema—a metric, melodic, and harmonic schema familiar to all the participants—which gives a predictable order to the piece. In addition, each of the musicians has at the ready a repertoire of musical figures which he can deliver at appropriate moments. Improvisation consists in varying, combining, and recombining a set of figures within the schema which bounds and gives coherence to the performance. As the musicians feel the direction of the music that is developing out of their interwoven contributions, they make new sense of it and adjust their performance to the new sense they have made.

They are reflecting-in-action on the music they are collectively making and on their individual contributions to it, thinking what they are doing and, in the process, evolving their way of doing it. Of course, we need not suppose that they reflect-in-action in the medium of words. More likely, they reflect through a "feel for the music" which is not unlike the pitcher's "feel for the ball."

Much reflection-in-action hinges on the experience of surprise. When intuitive, spontaneous performance yields nothing more than the results expected for it, then we tend not to think about it. But when intuitive performance leads to surprises, pleasing and promising or unwanted, we may respond by reflecting-in-action. Like the baseball pitcher, we may reflect on our "winning habits"; or like the jazz musician, on our sense of the music we have been making; or like the designer, on the misfit we have unintentionally created. In such processes, reflection tends to focus interactively on the outcomes of action, the action itself, and the intuitive knowing implicit in the action.

Let us consider an example which reveals these processes in some detail.

In an article entitled "If you want to get ahead, get a theory," Inhelder and Karmiloff-Smith[66] describe a rather unusual experiment concerning "children's processes of discovery in action."[67] They asked their subjects to balance wooden blocks on a metal bar. Some of the blocks were plain wooden blocks, but others were conspicuously or inconspicuously weighted at one end. The authors attended to the spontaneous processes by which the children tried to learn about the properties of the blocks, balance them on the bar, and regulate their actions after success or failure.

They found that virtually all children aged six to seven began the task in the same way:

> *all* blocks were systematically first tried at their geometric center.[68]

And they found that slightly older children would not only place all blocks at their geometric center but that

> when asked to add small blocks of varying shapes and sizes to blocks already in balance, they added up to ten blocks precariously one on top of the other at the geometric center rather than distributing them at the extremities.[69]

They explain this persistent and virtually universal behavior by attributing to the children what they call a "theory-in-action": a "geometric center theory" of balancing, or, as one child put it, a theory that "things always balance in the middle."

Of course, when the children tried to balance the counterweighted blocks at their geometric centers, they failed. How did they respond to failure? Some children made what the authors called an "action-response."

> They now placed the very same blocks more and more systematically at the geometric center, with only very slight corrections around this point. They showed considerable surprise at not being able to balance the blocks a second time ("Heh, what's gone wrong with this one, it worked before") . . . Action sequences then became reduced to: Place carefully at geometric center, correct very slightly around this center, abandon all attempts, declaring the object "impossible" to balance.[70]

Other children, generally between the ages of seven and eight, responded in a very different way. When the counterweighted blocks failed to balance at their geometric centers, these children began to de-center them. They did this first with conspicuously counterweighted blocks. Then

> gradually, and often almost reluctantly, the 7 to 8 year olds began to make corrections also on the inconspicuous weight blocks

> . . . At this point, we observed many pauses during action sequences on the inconspicuous weight items.[71]

Later still,

> As the children were now really beginning to question the generality of their geometric center theory, a negative response at the geometric center sufficed to have the child rapidly make corrections toward the point of balance.[72]

And finally,

> children paused *before* each item, roughly assessed the weight distribution of the block by lifting it ("you have to be careful, sometimes it's just as heavy on each side, sometimes it's heavier on one side"), inferred the probable point of balance and then placed the object immediately very close to it, without making any attempts at first balancing at the geometric center.[73]

The children now behaved as though they had come to hold a theory-in-action that blocks balance, not at their geometric centers, but at their centers of gravity.

This second pattern of response to error, the authors call "theory-response." Children work their way toward it through a series of stages. When they are first confronted with a number of events which refute their geometric center theories-in-action, they stop and think. Then, starting with the conspicuous-weight blocks, they begin to make corrections away from the geometric center. Finally, when they have really abandoned their earlier theories-in-action, they weigh all the blocks in their hands so as to infer the probable point of balance. As they shift their theories of balancing from geometric center to center of gravity, they also shift from a "success orientation" to a "theory orientation." Positive and negative results come

to be taken not as signs of success or failure in action but as information relevant to a theory of balancing.

It is interesting to note that as the authors observe and describe this process, they are compelled to invent a language. They describe theories-in-action which the children themselves cannot describe.

> Indeed, although the (younger) child's action sequences bear eloquent witness to a theory-in-action implicit in his behavior, this should not be taken as a capacity to conceptualize explicitly on what he is doing and why.[74]

Knowing-in-action which the child may represent to himself in terms of a "feel for the blocks," the observers redescribe in terms of "theories." I shall say that they convert the child's know*ing*-in-action to know*ledge*-in-action.

A conversion of this kind seems to be inevitable in any attempt to talk about reflection-in-action. One must use words to describe a kind of knowing, and a change of knowing, which are probably not originally represented in words at all. Thus, from their observations of the children's behavior, the authors make verbal descriptions of the children's intuitive understandings. These are the authors' theories about the children's knowing-in-action. Like all such theories, they are deliberate, idiosyncratic constructions, and they can be put to experimental test:

> Just as the child was constructing a theory-in-action in his endeavor to balance the blocks, so we, too, were making on-the-spot hypotheses about the child's theories and providing opportunities for negative and positive responses in order to verify our own theories![75]

Reflecting-in-practice The block-balancing experiment is a beautiful example of reflection-in-action, but it is very far removed from our usual images of professional practice. If we are to relate the idea of reflection-in-action to professional practice, we must consider what a practice is and how it is like and unlike the kinds of action we have been discussing.

The word "practice" is ambiguous. When we speak of a lawyer's practice, we mean the kinds of things he does, the kinds of clients he has, the range of cases he is called upon to handle. When we speak of someone practicing the piano, however, we mean the repetitive or experimental activity by which he tries to increase his proficiency on the instrument. In the first sense, "practice" refers to performance in a range of professional situations. In the second, it refers to preparation for performance. But professional practice also includes an element of repetition. A professional practitioner is a specialist who encounters certain types of situations again and again. This is suggested by the way in which professionals use the word "case"—or project, account, commission, or deal, depending on the profession. All such terms denote the units which make up a practice, and they denote types of family-resembling examples. Thus a physician may encounter many different "cases of measles"; a lawyer, many different "cases of libel." As a practitioner experiences many variations of a small number of types of cases, he is able to "practice" his practice. He develops a repertoire of expectations, images, and techniques. He learns what to look for and how to respond to what he finds. As long as his practice is stable, in the sense that it brings him the same types of cases, he becomes less and less subject to surprise. His knowing-in-practice tends to become increasingly tacit, spontaneous, and automatic, thereby conferring upon him and his clients the benefits of specialization.

On the other hand, professional specialization can have negative effects. In the individual, a high degree of specialization can lead to a parochial narrowness of vision. When a profession

divides into subspecialties, it can break apart an earlier wholeness of experience and understanding. Thus people sometimes yearn for the general practitioner of earlier days, who is thought to have concerned himself with the "whole patient," and they sometimes accuse contemporary specialists of treating particular illnesses in isolation from the rest of the patient's life experience. Further, as a practice becomes more repetitive and routine, and as knowing-in-practice becomes increasingly tacit and spontaneous, the practitioner may miss important opportunities to think about what he is doing. He may find that, like the younger children in the block-balancing experiment, he is drawn into patterns of error which he cannot correct. And if he learns, as often happens, to be selectively inattentive to phenomena that do not fit the categories of his knowing-in-action, then he may suffer from boredom or "burn-out" and afflict his clients with the consequences of his narrowness and rigidity. When this happens, the practitioner has "over-learned" what he knows.

A practitioner's reflection can serve as a corrective to over-learning. Through reflection, he can surface and criticize the tacit understandings that have grown up around the repetitive experiences of a specialized practice, and can make new sense of the situations of uncertainty or uniqueness which he may allow himself to experience.

Practitioners do reflect *on* their knowing-in-practice. Sometimes, in the relative tranquility of a postmortem, they think back on a project they have undertaken, a situation they have lived through, and they explore the understandings they have brought to their handling of the case. They may do this in a mood of idle speculation, or in a deliberate effort to prepare themselves for future cases.

But they may also reflect on practice while they are in the midst of it. Here they reflect-in-action, but the meaning of this term needs now to be considered in terms of the complexity of knowing-in-practice.

A practitioner's reflection-in-action may not be very rapid. It is bounded by the "action-present," the zone of time in which action can still make a difference to the situation. The action-present may stretch over minutes, hours, days, or even weeks or months, depending on the pace of activity and the situational boundaries that are characteristic of the practice. Within the give-and-take of courtroom behavior, for example, a lawyer's reflection-in-action may take place in seconds; but when the context is that of an antitrust case that drags on over years, reflection-in-action may proceed in leisurely fashion over the course of several months. An orchestra conductor may think of a single performance as a unit of practice, but in another sense a whole season is his unit. The pace and duration of episodes of reflection-in-action vary with the pace and duration of the situations of practice.

When a practitioner reflects in and on his practice, the possible objects of his reflection are as varied as the kinds of phenomena before him and the systems of knowing-in-practice which he brings to them. He may reflect on the tacit norms and appreciations which underlie a judgment, or on the strategies and theories implicit in a pattern of behavior. He may reflect on the feeling for a situation which has led him to adopt a particular course of action, on the way in which he has framed the problem he is trying to solve, or on the role he has constructed for himself within a larger institutional context.

Reflection-in-action, in these several modes, is central to the art through which practitioners sometimes cope with the troublesome "divergent" situations of practice.

When the phenomenon at hand eludes the ordinary categories of knowledge-in-practice, presenting itself as unique or unstable, the practitioner may surface and criticize his initial understanding of the phenomenon, construct a new description of it, and test the new description by an on-the-spot experiment. Sometimes he arrives at a new theory of the phenomenon by articulating a feeling he has about it.

When he finds himself stuck in a problematic situation which he cannot readily convert to a manageable problem, he may construct a new way of setting the problem—a new frame which, in what I shall call a "frame experiment," he tries to impose on the situation.

When he is confronted with demands that seem incompatible or inconsistent, he may respond by reflecting on the appreciations which he and others have brought to the situation. Conscious of a dilemma, he may attribute it to the way in which he has set his problem, or even to the way in which he has framed his role. He may then find a way of integrating, or choosing among, the values at stake in the situation.

The following are brief examples of the kinds of reflection-in-action which I shall illustrate and discuss at greater length later on.

An investment banker, speaking of the process by which he makes his judgments of investment risk, observes that he really cannot describe everything that goes into his judgments. The ordinary rules of thumb allow him to calculate "only 20 to 30 percent of the risk in investment." In terms of the rules of thumb, a company's operating numbers may be excellent. Still, if the management's explanation of the situation does not fit the numbers, or if there is something odd in the behavior of the people, that is a subject for worry which must be considered afresh in each new situation. He recalls a situation in which he spent a day with one of the largest banks in Latin America. Several new business proposals were made to him, and the bank's operating numbers seemed satisfactory. Still, he had a gnawing feeling that something was wrong. When he thought about it, it seemed that he was responding to the fact that he had been treated with a degree of deference out of all proportion to his actual position in the international world of banking. What could have led these bankers to treat him so inappropriately? When he left the bank at the end of the day, he said to his colleague, "No new business with that outfit! Let the existing obligations come in, but nothing new!" Some months later, the bank went through the biggest bankruptcy ever in Latin America—and all the time there had been nothing wrong with the numbers.

An ophthalmologist says that a great many of his patients bring problems that are not in the book. In 80 or 85 percent of the cases, the patient's complaints and symptoms do not fall into familiar categories of diagnosis and treatment. A good physician searches for new ways of making sense of such cases, and invents experiments by which to test his new hypotheses. In a particularly important family of situations, the patient suffers simultaneously from two or more diseases. While each of these, individually, lends itself to familiar patterns of thought and action, their combination may constitute a unique case that resists ordinary approaches to treatment.

The ophthalmologist recalls one patient who had inflammation of the eye (uveitis) combined with glaucoma. The treatment for glaucoma aggravated the inflammation, and the treatment for uveitis aggravated the glaucoma. When the patient came in, he was already under treatment at a level insufficient for cure but sufficient to irritate the complementary disease.

The ophthalmologist decided to remove all treatment and wait to see what would emerge. The result was that the patient's uveitis, a parasitic infection, remained in much reduced form. On the other hand, the glaucoma disappeared altogether, thus proving to have been an artifact of the treatment. The

opthalmologist then began to "titrate" the patient. Working with very small quantities of drugs, he aimed not at total cure but at a reduction of symptoms which would allow the patient to go back to work. (Seven lives depended on his 5000 ocular cells!) The prognosis was not good, for uveitis moves in cycles and leaves scars behind which impede vision. But for the time being, the patient was able to work.

In his mid-thirties, sometime between the composition of his early work *The Cossacks* and his later *War and Peace*, Lev Nikolayevitch Tolstoy became interested in education. He started a school for peasant children on his estate at Yasnaya Polanya, he visited Europe to learn the latest educational methods, and he published an educational journal, also called *Yasnaya Polanya*. Before he was done (his new novel eventually replaced his interest in education), he had built some seventy schools, had created an informal teacher-training program, and had written an exemplary piece of educational evaluation.

For the most part, the methods of the European schools filled him with disgust, yet he was entranced by Rousseau's writings on education. His own school anticipated John Dewey's later approach to learning by doing, and bore the stamp of his conviction that good teaching required "not a method but an art." In an essay, "On Teaching the Rudiments," he describes his notion of art in the teaching of reading:

> Every individual must, in order to acquire the art of reading in the shortest possible time, be taught quite apart from any other, and therefore there must be a separate method for each. That which forms an insuperable difficulty to one does not in the least keep back another, and vice versa. One pupil has a good memory, and it is easier for him to memorize the syllables than to comprehend the vowellessness of the consonants; another reflects calmly and will comprehend a most rational sound method; another has

a fine instinct, and he grasps the law of word combinations by reading whole words at a time.

> The best teacher will be he who has at his tongue's end the explanation of what it is that is bothering the pupil. These explanations give the teacher the knowledge of the greatest possible number of methods, the ability of inventing new methods and, above all, not a blind adherence to one method but the conviction that all methods are one-sided, and that the best method would be the one which would answer best to all the possible difficulties incurred by a pupil, that is, not a method but an art and talent.
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> . . . Every teacher must . . . by regarding every imperfection in the pupil's comprehension, not as a defect of the pupil, but as a defect of his own instruction, endeavor to develop in himself the ability of discovering new methods . . . [76]

An artful teacher sees a child's difficulty in learning to read not as a defect in the child but as a defect "of his own instruction." So he must find a way of explaining what is bothering the pupil. He must do a piece of experimental research, then and there, in the classroom. And because the child's difficulties may be unique, the teacher cannot assume that his repertoire of explanations will suffice, even though they are "at the tongue's end." He must be ready to invent new methods and must "endeavor to develop in himself the ability of discovering them."

Over the last two years, researchers at the Massachusetts Institute of Technology have undertaken a program of in-service education for teachers, a program organized around the idea of on-the-spot reflection and experiment, very much as in Tolstoy's art of teaching. In this Teacher Project,[77] the researchers have encouraged a small group of teachers to explore their own intuitive thinking about apparently simple tasks in such domains as mathematics, physics, music, and the perceived behavior of the moon. The teachers have made some important

discoveries. They have allowed themselves to become confused about subjects they are supposed to "know"; and as they have tried to work their way out of their confusions, they have also begun to think differently about learning and teaching.

Early in the project, a critical event occured. The teachers were asked to observe and react to a videotape of two boys engaged in playing a simple game. The boys sat at a table, separated from one another by an opaque screen. In front of one boy, blocks of various colors, shapes, and sizes were arranged in a pattern. In front of the other, similar blocks were lying on the table in no particular order. The first boy was to tell the second one how to reproduce the pattern. After the first few instructions, however, it became clear that the second boy had gone astray. In fact, the two boys had lost touch with one another, though neither of them knew it.

In their initial reactions to the videotape, the teachers spoke of a "communications problem." They said that the instruction giver had "well-developed verbal skills" and that the receiver was "unable to follow directions." Then one of the researchers pointed out that, although the blocks contained no green squares—all squares were orange and only triangles were green—she had heard the first boy tell the second to "take a green square." When the teachers watched the videotape again, they were astonished. That small mistake had set off a chain of false moves. The second boy had put a green thing, a triangle, where the first boy's pattern had an orange square, and from then on all the instructions became problematic. Under the circumstances, the second boy seemed to have displayed considerable ingenuity in his attempts to reconcile the instructions with the pattern before him.

At this point, the teachers reversed their picture of the situation. They could see why the second boy behaved as he did. He no longer seemed stupid; he had, indeed, "followed instructions." As one teacher put it, they were now "giving him reason." They saw reasons for his behavior; and his errors, which they had previously seen as an inability to follow directions, they now found reasonable.

Later on in the project, as the teachers increasingly challenged themselves to discover the meanings of a child's puzzling behavior, they often spoke of "giving him reason."

In examples such as these, something falls outside the range of ordinary expectations. The banker has a feeling that something is wrong, though he cannot at first say what it is. The physician sees an odd combination of diseases never before described in a medical text. Tolstoy thinks of each of his pupils as an individual with ways of learning and imperfections peculiar to himself. The teachers are astonished by the sense behind a student's mistake. In each instance, the practitioner allows himself to experience surprise, puzzlement, or confusion in a situation which he finds uncertain or unique. He reflects on the phenomena before him, and on the prior understandings which have been implicit in his behavior. He carries out an experiment which serves to generate both a new understanding of the phenomena and a change in the situation.

When someone reflects-in-action, he becomes a researcher in the practice context. He is not dependent on the categories of established theory and technique, but constructs a new theory of the unique case. His inquiry is not limited to a deliberation about means which depends on a prior agreement about ends. He does not keep means and ends separate, but defines them interactively as he frames a problematic situation. He does not separate thinking from doing, ratiocinating his way to a decision which he must later convert to action. Because his experimenting is a kind of action, implementation is built into his inquiry. Thus reflection-in-action can proceed, even

in situations of uncertainty or uniqueness, because it is not bound by the dichotomies of Technical Rationality.

Although reflection-in-action is an extraordinary process, it is not a rare event. Indeed, for some reflective practitioners it is the core of practice. Nevertheless, because professionalism is still mainly identified with technical expertise, reflection-in-action is not generally accepted—even by those who do it—as a legitimate form of professional knowing.

Many practitioners, locked into a view of themselves as technical experts, find nothing in the world of practice to occasion reflection. They have become too skillful at techniques of selective inattention, junk categories, and situational control, techniques which they use to preserve the constancy of their knowledge-in-practice. For them, uncertainty is a threat; its admission is a sign of weakness. Others, more inclined toward and adept at reflection-in-action, nevertheless feel profoundly uneasy because they cannot say what they know how to do, cannot justify its quality or rigor.

For these reasons, the study of reflection-in-action is critically important. The dilemma of rigor or relevance may be dissolved if we can develop an epistemology of practice which places technical problem solving within a broader context of reflective inquiry, shows how reflection-in-action may be rigorous in its own right, and links the art of practice in uncertainty and uniqueness to the scientist's art of research. We may thereby increase the legitimacy of reflection-in-action and encourage its broader, deeper, and more rigorous use.