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Reflections on Reflective Practice (2/7)

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Part 2: Applied science and expertise, or the art of testing and contesting practical claims In the first part of this series, we had a first brief look at the "reflective practice" mainstream and encountered a somewhat ambivalent situation. Clearly, the world we live in is becoming ever more pluralist and this raises questions about the professional's rightful claims to special expertise, rationality, and objectivity; about the role of applied science in justifying such claims; about the part that values play in professional practice and the need for better equipping professionals with corresponding critical skills; and about how we can reform both professional and civic education so as to teach and learn such skills systematically. *Reflective practice* might be the answer; but as we also noted, the mainstream literature on reflective practice is so preoccupied with "soft," psychological issues that it has hardly begun to address these questions systematically.

My conclusion in the first part was that the reflective practice mainstream is not giving sufficient attention to the philosophical and methodological issues that these questions raise. I do not believe, therefore, that it can give us adequate answers to the challenges that professional practice is facing today. I fear it fails to offer professionals a proper understanding of the nature and limits of their own expertise and competence, as well as of the normative core of "applied science" and of the difference that well understood "critical" reflection might make for their practice.

Accordingly, this second part of our discussion should help us better understand what it takes to recover the role of critical reflection in applied science and expertise. Equipped with such understanding we will then in the next part return to the diagnosed "soft spot" of the reflective practice mainstream literature and try to see what's beneath it, its core motives and errors. But before we are able to do that, we need to focus on the notion of *applied science* so as to appreciate both its merits and defects. Its merits, because they might provide a corrective to the mainstream's "soft spot"; its defects, because they should help us understand some of the fundamental concerns to which the reflective practice movement sought to respond with its soft turn.

Expertise, science education, and scientific attitude There is a widespread belief among professionals as well as in the general public, according to which professional competence in some domain of specialization is basically grounded in (if not essentially the same as) proficiency in the scientific disciplines concerned with that domain; so much so that "expertise" is frequently equated with such proficiency. Even where a profession is considered to be as much an art as an applied science, as in the case of architecture, management, the legal professions, social work or psychotherapy, among others, most of us (and I include myself) believe that professional competence will still benefit from adopting a *scientific attitude,* that is, a stance of deferred judgment in favor of careful and systematic inquiry. This may explain why *science education* is often taken to provide an essential, although not necessarily the only, qualification for entering a profession or becoming an "expert."

This is not a bad starting point, but it has its dangers. Science education in the sense just mentioned is more properly called *science training,* for it aims at training research skills. It is in this sense that popular opinion tends to expect experts to be "scientific"**:** we expect from them that their findings and conclusions meet scientific criteria of validity. We need not follow this popular view and identify expertise with scientific competence to appreciate what speaks in favor of it. It reminds us that professional competence has something to do with *disciplined* inquiry; with that *disciplined mind* which already John Dewey (1910, pp. 63 and 78), who certainly cannot be said to have been a narrow theorist of science removed from educational practice, considered the central aim of education. I believe with Dewey that true expertise requires a disciplined mind, and that one way to acquire such discipline of mind is through a sustained personal quest for competence in disciplined inquiry. The quest is never ending, of course. Becoming an expert in this sense may take years of training and experience in the proper use of methods; but just as importantly, it takes relentless questioning of one's assumptions and results, and corresponding self-limitation of one's claims. This is the ideal "discipline of mind" that I would associate with the quest for competence, along with many other practical skills and virtues (for extensive discussion see Ulrich, 2001).

However, ideals are not to everybody's taste, nor is science training. There is another meaningful understanding of science education, which aims more at a basic *science literacy* for all than at science training for a few. This understanding is just as important for our present purpose; for obviously not every professional needs to be a thoroughly trained researcher or "applied scientist" to be competent. What is indispensable, rather, is the discipline of respecting one's own limitations of competence; of recognizing when one needs to consult others; of being able to learn from them without renouncing one's own informed judgment and responsibility. This is what science education in the broader sense of "science literacy for all" is all about. The U.S. National Committee on Science Education has said it well, in a way that applies to general education as much as to professional education**:**

> In a world filled with the products of scientific inquiry, scientific literacy has become a necessity for everyone. Everyone needs to use scientific information to make choices that arise everyday. Everyone needs to be able to engage intelligently in public discourse and debate about important issues that involve science and technology. … Scientific literacy also is of increasing importance in the workplace. More and more jobs demand advanced skills, requiring that people be able to learn, reason, think creatively, make decisions, and solve problems. An understanding of science and the processes of science contributes in an essential way to these skills. (National Research Council, 1996, p. 1)

Science education in this sense matters because not only every professional but also every non-professional will benefit from a clear understanding of what to expect from science and what not. What is a valuable skill for ordinary citizens is surely valuable for professionals as well**:** being able to appreciate (as well as to question) the findings and claims of specialized researchers – to "engage intelligently" in debate with them – is important even for those professionals who are not, and need not be, "applied scientists." Investing in science education may thus indeed contribute to well-understood reflective practice; well understood in the sense of allowing both professionals and citizens to meet (other) experts at eye-level. Furthermore, the value that science education attaches to the idea of a scientific attitude may also help us (professionals and citizens alike) to avoid the trap of psychologizing our notion of professional competence too quickly, and in this way may furnish a partial corrective to the soft spot of the reflective practice mainstream.

On the negative side, emphasizing the value of science education risks being misunderstood. It might have even more people fall into the trap of equating competent professional practice with the use of "sound science" in solving practical problems. Many have fallen into the trap – trained researchers and professionals no less than ordinary citizens and decision makers – and thus have contributed, perhaps against their best intentions, to the narrowly technocratic understanding and image of professional expertise that is so prevalent today. It is responsible for a widespread loss of credibility and reputation that "the expert" has doubtlessly suffered (compare, e.g., Armstrong, 1981; White and Taket, 1994). Obviously, such a loss of critical distance is not what I mean by science literacy.

"Applied science" Despite their somewhat battered reputation, "experts" are in demand. They are in demand because decision makers are under pressure to draw on the special knowledge of researchers and "applied scientists." If they don't, they will unavoidably be accused of incompetence and irresponsibility, at latest when something goes wrong or if some of the parties concerned are not happy about the outcome. But if they do, they can refer to the higher authority of science and expertise, as it were; for who could blame them for having based their decisions on the best available knowledge?

But can reference to applied science and expertise really justify the consequences that our decisions and actions may impose on other people? Might it be that our notions of professional competence and expertise have not kept pace with the increasingly pluralistic nature of society, or with the growing reach and impact of professionally supported decision making both in the public and in the private sector?

One thing at least is clear**:** The way we train and practice professional intervention can hardly be better than is our understanding of the role of science in it. The point is neither to overrate nor underestimate that role but rather, to recover and maintain some critical distance; to always take into account both its merits and its limitations. We would not need to talk about professional competence and expertise at all if they were the same as proficiency in some discipline of science.

Let us take a step back, then. It may help some readers if we begin by briefly reminding ourselves of three basic distinctions that are relevant in the context of this discussion, and of the different kinds of competencies required**:**

- **Natural vs. social sciences:** Popular opinion tends to associate concepts such as "science education" and "scientific attitude" primarily with the natural and technical sciences. However, I see no reason why the social sciences and the arts should *a priori* be considered unable to contribute to science education and to foster a scientific attitude. Perhaps a better stance would be to say that science education, as well as a scientific attitude, may be grounded in and apply to all fields of study and expertise that rely on some well-defined forms of *systematic and disciplined inquiry.*

  The exact meaning of these terms will depend on the specific area of expertise concerned, but for me "systematic" inquiry basically means that *judgment is deferred* until all evidence has been considered, and "disciplined" inquiry means that findings and conclusions are *controlled* by standardized (repeatable, explainable) procedures of observation and analytical reasoning. In addition, we may expect "scientific" fields of study and expertise to offer some institutionalized programs of training and qualification. Basically, these virtues of a "scientific" approach should remain the same, regardless of what the field of study is.

- **Basic vs. applied research:** Distinguishing applied science from basic science makes good sense, not so much because the scientific methods in question would be different but rather because applying them to practical questions requires more than scientific training; equally important is a thorough understanding of the concrete situation to which such methods are applied – the *context of application*, to use the language of critical systems heuristics (e.g., Ulrich, 1987, p. 276) – and of the decision-making and legitimation processes by which results gain recognition as a basis for taking action.

  It is rather obvious, of course, that proper application of science requires good knowledge of the situation to which it is to be applied. Scientific training and/ or a scientific attitude (or a disciplined mind, to use Dewey's term) cannot replace thorough familiarity with the situation. However, the essential point goes deeper**:** it is that "all knowledge, in the context of its application, has not only an empirical or theoretical but also a normative content" (Ulrich, 1983, p. 20n). As soon as we understand a research effort in terms of applied rather than basic science, we implicitly recognize that our *concept of rationality,* that is, the criteria of valid justification of claims to knowledge and expertise, must change and must do justice not only to the theoretical but also to the normative dimension.

- **Specialist vs. generalist skills:** While a basic scientist needs first of all specialized knowledge of a theoretical and methodological area of research, a good practitioner needs first of all a good portion of generalist skills. That is, in addition to bringing in some particular expertise, he or she should understand enough of other areas of expertise to know which specialists to consult and how to evaluate their contributions. Furthermore, some specific skills in structuring complex situations of problem solving and questioning solution proposals are vital.

> Among the most important generalist skills, I would count some expertise in the *logic of argumentation* (Toulmin, 1958); in *boundary critique* (Ulrich, e.g., 1996, 2000, 2001, and 2006b); in some special *problem-structuring methods* (Rosenhead and Mingers, 2001); perhaps in some *creativity methods;* and a good number of additional skills of a generic *methodological* nature. Such skills are to some extent automatically developed through the earlier-mentioned cultivation of a disciplined mind; but they also can (and sometimes need to) be acquired through specific training and experience (e.g., in discourse theory and facilitation, systemic thinking and systems methodologies, evaluation methods, action research, management of research projects, frameworks and tools for reflective practice, etc.). Developing generalist skills thus goes far beyond the popular notion that in distinction to a specialist, who knows everything about almost nothing, the generalist knows little about almost everything. The competent generalist is a specialist for *the methodologically generic,* as it were, rather than someone who knows nothing particular. This definition also suggests that some *basic* understanding of philosophical questioning will not do harm, particularly the sort of questioning that is essential to epistemology (theory of knowledge), science theory, practical philosophy (theory of rational action), discourse theory (theory of rational discourse), hermeneutics (theory of interpretation), and ethics (moral theory), although it is of course clear that reflective practice isn't taking place in the philosophy seminar.

One may certainly have different views as to which kinds of skills are essential for these different forms of science. It should be clear, however, that any attempt to understand the requirements of applied science in the popular terms of basic natural science *only,* risks leading us astray. At the end of that road looms *scientism,* an impoverished variant of scientific attitude that identifies the reach of rationality with that of the methods of the natural sciences. It is equally clear that we do not want to fall into the opposite trap of adopting a merely "soft," psychological notion of reflective competence. Grounding our quest for competence in disciplined inquiry – in other words, associating it with a *research orientation* – is also meant to serve as an antidote to today's reflective practice mainstream, lest its soft spot become our blind spot.

Working to overcome the split of the "two cultures" I do not want to be misunderstood. I certainly do not mean to claim that science education provides (or should provide) *the* basic and generic model of professional education. Even in the basic and limited sense in which I understand the concept here, of fostering a scientific attitude or research orientation among practitioners, I introduce it as an orientation that should complement rather than replace the various other elements that I consider essential for educating the reflective practitioner. I have already suggested that beyond science education, and also as a basis for it, *some* philosophical grounding will be equally helpful (cf. also the conclusion of Part 1). Likewise, I have hinted at the need for giving ordinary people a relevant role to play, that is, for embedding our notion of professional competence in a framework of civil society and deliberative democracy (cf. also Ulrich, 2000, 2003).

The point, obviously, cannot be to play science education off against the soft skills of reflective practice, or vice versa. It can only be to overcome the breakdown of communication between the "applied science" mainstream and the "reflective practice" mainstream. The situation is conspicuously reminiscent of the unproductive split between the sciences and the arts (or humanities) that C.P. Snow (1959) diagnosed half a century ago in his book *The Two Cultures.* His account of science may be somewhat dated, but the split appears to be deeply entrenched in our minds, as well as to persist in our educational programs: Either you believe in the power of "sound science" but don't know beans about the obscure arts of the humanities, or you believe in the value of the humanities but lack a proper understanding of what a scientific attitude means. I suspect that the current divide between the mainstream of "applied science," which tends to equate sound practice with "sound science," and the mainstream of "reflective practice," which tends to equate sound practice with personal artistry and emotional navel-gazing, is something like a late expression of the divide between *the two cultures.*

This observation may help us understand why both sides, the applied science mainstream as well as the reflective practice mainstream, have developed in the way they have, and in what ways they both have got it wrong. We will return to the reflective practice mainstream in Part 3 and will then also try to understand its one-sided development against the background of the development of the applied science mainstream, and of the division that developed between these two research cultures; meanwhile we need to analyze in some more detail why it so insufficient to understand applied science in terms of "sound science." At issue, of course, is not the idea of applied science as such but only the currently *prevalent model* of applied science.

Popper's model of applied science Perhaps the most influential spokesman of the applied science mainstream today is Karl Popper (e.g., 1959/ 2002a, 1963/ 2002b, 1966, 1972, 1976, and 1999; for a useful summary, see O'Hear, 1995a; for a sympathetic account on the part of a practicing operations researcher, see Ormerod, 2009 [forthcoming]; for my rather critical views, cf. Ulrich, 1983, pp.26-30 and 41-105, and more recently 2001, p. 10, and particularly 2006c). I propose we focus on Popper not because he would be my preferred theorist of science or because I would find his writings particularly insightful, but simply because of the considerable influence that he has had, and continues to have, on many practicing researchers. As the *Stanford Encyclopedia of Philosophy* notes, "one of the many remarkable features of Popper's thought is the scope of his intellectual influence.… It is virtually unprecedented to find [scientists] queuing up, as they have done in Popper's case, to testify to the enormously practical beneficial impact which that philosophical work has had upon their own." (Thornton, 1997)

One reason for Popper's influence may be that his starting point is close to that of practicing researchers and professionals. Research for him begins not with theories or observations but with problems: "All life is problem solving" (1999, p. 99f). To Popper, science is indeed the quintessence of a *problem-solving approach.* What distinguishes scientific from every-day problem solving is only that it is a particularly qualified form of problem solving. Basically, all forms of problem solving rely on some kind of trial and error: they all devise, try out, and eliminate various solutions attempts, until one of these is found to work satisfactorily.

Scientific problem solving draws its solution attempts from theoretical reasoning rather than just from common-sense, and then subjects them to systematic empirical testing rather than just accepting them *ad hoc* (i.e., based on personal opinions and preferences). *Theoretical reasoning* works by systematically formulating universal (also called nomological, i.e. law-like) *hypotheses,* that is, causal or statistical explanation attempts of empirical phenomena. From these we can deduce the consequences that we expect our actions to have. How reliable these *anticipations* are depends on the degree to which the underpinning hypotheses have withstood serious testing. *Empirical testing* aims at eliminating erroneous theories (universal hypotheses) by systematically looking for observations that contradict our anticipations. This then either "falsifies" the theories or else "corroborates" them for the time being, although it can never verify them definitively (Popper, 1959/2002a, p. 248f). Popper calls this combined reliance on theoretical reasoning and empirical testing, along with deductive logic as the "organon of rational criticism" (1963/2002b, p. 85; 1972, p. 31; 1976, p. 98), the *hypothetico-deductive method,* or simply the *deductive method of testing* (see, e.g., Popper, 1959/2002a, pp. 7-10, and 1966b, p. 383; cf. O'Hear, 1995b). Its epitome is of course the controlled experiment of the experimental sciences. It constitutes to him the *critical method of science,* which in turn ensures the objectivity of its findings and the rationality of using these as a basis for rational action – which is what applied science is all about.

Many professionals feel at ease with this view of applied science, particularly those who have had their basic training in some field of "exact" quantitative science or in a similarly oriented applied discipline such as engineering, economics, statistics or operations research, to name just a few examples. Popper's model offers them a welcome personal sense of familiarity and orientation in confronting the messy world of practice.

So far, so good. However, familiarity is not the same as critical relevance. A good model of applied science should not only offer us a familiar framework for practicing what we have learned, it should also provide critical impetus and guidance for moving beyond and improving our expertise. In particular, it should help us understand the many and various validity claims that each concrete application of our expertise involves. To what extent may we not be able to justify them? How do we deal rationally with such justification gaps? Given that every concrete context of application is to some extent unique, what do we need to assume about it to apply our theoretical knowledge? How else could we define the relevant context? What kind of value implications would that have? Who is concerned? And so on. In a word, a good model should help us question the rationality of our practice. We need to ask, then, what exactly is the model's underpinning notion of rational practice (a), and how does it translate this notion into a critical method or framework for reflective practice (b)?

(a) Popper's notion of rational practice is conditioned by his view of all research and practice as a process of problem solving, and of all problem solving as systematic trial and error. Since applied science is a qualified way of thinking up "trials" and eliminating "errors" by means of theoretical reasoning and empirical testing, it follows that practice is rational to the extent it relies on the so far *best-tested* available hypotheses that explain how to reach a desired outcome (Popper, 1972, p. 21f). Furthermore, rational practice will carefully control how well actual and expected outcomes correspond, with a view to eliminating or refining false or inaccurate hypotheses. Theory is thus doubly primary: first, in that the rationality of practical action is grounded in it, and secondly, in that its application in turn is to be understood and designed as a test of *theoretical* conjectures and expectations.

This double primacy of theory has major implications for science-based practice. How rational it is depends on how conclusive the application test is, not on how welcome are its consequences for those who may have to live with them. The concept of "application" is thus purified of any ethical content. Its value consists in demonstrating the *fitness of theories,* not the ethics of practice. "A theory is a tool which we test by applying it, and which we judge as to it fitness by the results of its applications." (Popper, 1959/2002a, p. 91). The concept of justification or critique adopted is interested exclusively in the *theoretical* implications of "application," it has no grasp of normative implications at all. That may be adequate for the laboratory of the experimental scientist, but applied science does not take place in the laboratory, as little as in the philosophy seminar.

All these implications betray a fundamental error of categories: we are dealing with a model of applied science that remains entirely trapped in a framework of theoretical, basic science. The model begs the question of what constitutes the specific nature and rationale of applied science, it merely extends basic science to the world of practice. Rather than asking what differences there are between the experimental lab and the world of practice, and trying to adapt its concepts of rationality and criticism accordingly, it merely tells us: "*Njet!* There is no difference."

In one sense, though, one might agree with Popper. Isn't it true, after all, that we should not burden science with tasks for which it has not been designed? My response is, I have no problem with such a self-limiting view of science, only with the *confusion of the scientific with the rational* that goes hand in hand with it. The issue is not science as such, but the way we use it to promote rational practice. While it is correct to say with Popper that rational practice cannot do without clear anticipation of its consequences and in this respect depends on theoretical hypotheses in need of scientific testing, it is not correct to conclude that its rationality depends on theoretical reasoning *only*. The very fact that we are talking about the *consequences* of our actions implies that rational action is not only theory-laden but equally value-laden. Consequences "have value" for people. Whether and to what extent they are "rational" (i.e., justifiable) depends on what they *mean* to those whom they may affect, that is, on people's needs, interests, and worldviews. When it comes to this inevitable value content of our practice, the extent to which it is grounded in well-corroborated theory is actually quite irrelevant for its justification! How rational, then, is a concept of rational practice that takes refuge in the world of theory, rather than exposing itself to the specific challenges of practice?

(b) Popper's critical method fares hardly better. As we have seen, it is grounded in the experimental method of the natural sciences. The difficulty lies in what Popper's view of the critical method of science excludes, rather than in what it stipulates. Its methodological core principle, the hypothetico-deductive method, leaves no adequate room for *discursive* methods of critical examination as they are important in the humanities (including the social sciences) and the arts (including the applied disciplines) as well as in everyday practice. Although Popper does give a role to critical discussion, its criteria of criticism are drawn exclusively from the supposedly objective tools of controlled observation and deductive reasoning. Such criteria may allow us to judge the consistency or inconsistency of empirical findings with theoretical hypotheses, but they offer no way of judging the adequacy of the value assumptions or implications of our practice. Obviously (except for Popper, that is), such a framework of "objective" criticism does not easily apply to the needs of the applied disciplines and of professional practice.

Even where it does apply, it is a dangerous idea that applied scientists and practicing professionals should understand and question their methods, findings and conclusions in such terms of objective (because empirically grounded) criticism *only*. The unspoken assumption is that sound problem solving and rational practice are ensured by adhering to established standards of scientific procedure and rigor. By implication, the expert's findings and conclusions may then also claim a status of objectivity and rationality that other people's views do not usually enjoy; which is only another way of saying that not everyone is considered competent to challenge the professional's results. But how critical is a concept of critique that in effect immunizes its own assumptions and consequences against the critical efforts of a majority of (ordinary, but by no means unreasonable) people?

Some critical thoughts I do not want to delve further into Popper's specific model here, as I have given extensive critical accounts elsewhere (Ulrich, 1983 and 2006). Rather, I would like to conclude this discussion of the merits and potential pitfalls of an "applied science" perspective of professional practice with a few general critical thoughts on the current mainstream conception of applied science, of which Popper's work is only a particularly well-articulated and influential example.

(1) On the limited reach of science: Science cannot justify its own consequences. It can suggest and test, corroborate or falsify, theoretically *anticipated* consequences of actions, but it would be wrong to conclude that on this basis, we can justify claims to rational *action.* Demonstrating correct anticipation of the consequences of action is not the same as justifying the consequences themselves. What is justified in this way is our theoretical reasoning, not our practical actions. We must be careful though that we do not draw the wrong conclusion, by going to the other extreme and relegating practice to an entirely non-rational (if not irrational) domain, a domain of merely subjective acts of belief and "decisions." We have no reason to assume that as a matter of principle, we cannot at all talk and decide about our actions *with reason;* for rational thought and argumentation reach beyond science. Science is limited to the realm of empirical phenomena; reason is not. To be sure, careful empirical testing is part of reason's call; but over and above that, reason's call is that in deciding about the means and ends of action, we do not rely on non-argumentative means such as status, authority, power, deception, manipulation, or others. Reason calls upon us to adopt the *argumentative principle* as a rational – and peaceful – alternative, in deciding about practical as much as theoretical claims. This is what in the philosophical tradition since Aristotle and Kant we mean by the quest for *practical reason,* but the concept of practical rationality that is underpinning the contemporary model of applied science completely misses this intent.

Because the mainstream model of applied science allows for no other form of rationality than what is amenable to science, it means that what cannot be grasped in scientific categories must indeed be relegated to a domain of merely subjective acts of belief about which we cannot argue rationally at all. There simply is no place for practical reason – the use of critical reason to decide about practical claims – in its model of rational criticism. But this consequence is an artefact of an impoverished concept of rationality, not an inherent limitation of the argumentative principle itself.

(2) On the critical use of practical reason: An adequate concept of competent practice, and of the role of applied science in it, cannot do without the idea of practical reason. This conclusion does not put into question the idea of applied science as such, only too narrow a notion of what constitutes its rationality. Practical reason is the messenger who tells us the *bad news,* namely, that in contexts of practical application of science, there is no such thing as a purely theoretical and instrumental rationality. The messenger admonishes us: Always beware that in your quest for competence, you do not substitute instrumental for practical rationality! Hence, resist the temptation of quickly relegating all those questions which do not lend themselves to your scientific tools, to a merely subjective status; instead, make them the subject of systematic reflection and talk openly and rationally about them!

To be sure, this causes us some methodological difficulties; we need to search for a middle ground between the neat world of theory and the messy world of practice. But as always with bad news, it's no use blaming the messenger. The only reasonable way out is to try and recover some lost argumentative ground *between* the quest for complete theoretical reason on the one hand, and a complete renunciation of the critical power of practical reason on the other hand.

Methodologically speaking, I locate the crucial point of attack in the inextricable *two- dimensionality of rational practice.* Practice ultimately always needs to be rational in both dimensions, in its ways of handling questions of fact as well as questions of value; or else it is not rational at all. This is so because rationality in the selection of means is no substitute for rationality in the selection of ends. Rational action in the service of unreasonable ends may be efficient, but is hardly justifiable on rational grounds.

(3) On applied science and means-end dualism: What's wrong with instrumental rationality, so long as we don't take it for all there is to practical reason? To be sure, nobody will want to argue that we should renounce the help that science can give us in dealing with questions of fact, including anticipating consequences of action. But mind you, the tacit assumption does not hold, according to which the selection of means (unlike that of ends) can be reduced to questions of fact and for this reason falls into the sole competence of (applied) science and expertise. The old trick of avowing that ends have to be assumed to be "given," as they (allegedly) cannot be justified rationally, so that one can then with no further ado focus on issues of purely instrumental rationality, does not buy as much immunity from value judgments as is generally assumed. Whatever science may contribute to the selection of means for achieving "given" purposes, the means still have a normative content of their own; for alternative means to reach an end may affect different parties differently. The choice of means, like that of ends, has consequences. Science, as we have noted above, can justify the *anticipation* of consequences, but not the consequences themselves.

This makes it understandable why in practice, the choice of means is often just as controversial as that of ends. Think of the example of energy policy: the end of ensuring sufficient and reliable provision of electrical energy to all households is often far less controversial than the question of how this is to be accomplished, say, via the nuclear option, the renewable energy option, or the energy efficiency (i.e., saving) option. The underlying means-end dualism, according to which all value content can be assigned to the choice of ends, is faulty! (Compare Ulrich, 1983, p. 71-73, for a critique of the prevalent means-end dualism.)

As soon as we recognize this circumstance, it becomes clear that the attempt to ground applied science and rational practice in theoretical reason alone is bound to fail. It only works so long as we tacitly (and uncritically) reduce questions of practical reason ("What *should* we do, reasonably?") to questions of instrumental reason ("What *can* we do, and how can we do it most effectively?"). As soon as we take those instrumental answers back into practical contexts of action, they recover their normative content and there is no way we can keep them free of value implications. Applied science differs from basic science in that there is no such thing as a purely instrumental question. We cannot, then, model applied science along the lines of Popper's methodological prescription for the experimental sciences, the hypothetico-deductive method of testing.

(4) The art of "testing" and "contesting": The principle of falsification is widely accepted today as a core concept of rational research and practice. There can be little doubt that it embodies a progress over previous inductivist and positivist conceptions of how research works. Even so, our considerations suggest that it has not yet been translated into satisfactory criteria of critical practice outside basic empirical science. I do not think this is so because the critical thrust of the falsification principle is faulty, but rather because it has up to now been associated with a rather impoverished concept of what constitutes rational criticism. Our discussion thus far has provided more than enough food for critical thought, so I'll try to formulate my criticism as positively and as simply as I can: When it comes to applied science and expertise, our criteria of *"testing"* need to give some room to *"contesting"* on the part of those who may have to live with the consequences. It is then not helpful to reduce "testing" to the methods of empirical science and thereby, implicitly, to declare the people concerned but not involved *a priori* incompetent to question the expert's "facts" and "solutions."

If this suggestion is not entirely mistaken, one must wonder whether the popularity that Popper's model of applied science enjoys among practicing researchers is not perhaps due in part to the wrong reasons. It is so convenient to reserve objectivity and rational criticism to oneself, while relegating the doubts and concerns of ordinary people to an extra-scientific domain that is of little relevance to testing! "Testing" and "contesting" can thus *apparently* be treated as two entirely different pairs of shoes; the one moves within the sphere of objective theoretical reasoning and the other in a sphere of personal acts of belief, and both work apparently best if left alone.

Paradoxically, not only ordinary citizens but also the decision makers who mandate and pay the expert's work, thus find themselves in a situation in which they ultimately have to *believe* in the expert's results. As they do not usually have all the knowledge and skills required to see through and question the many assumptions and considerations on which theses results depend, they have to "buy" them. In the name of competence, professional practice thus tends to put those it is supposed to serve in a position of incompetence and dependency (Ulrich, 1996, pp. 5f, 13f, and 41; 2000, pp. 247f, 249f, and 253f).

An adequate model of applied science will try to avoid this kind of elitist implications, as much as it will try to recover the lost "other," non-instrumental reason of practical reason. With a view to both ends, it will seek to extend our concept of rational criticism so that not only scientific or expert "testing" but also "contesting" citizens have a competent role to play in the quest for reflective practice. The key lies in adopting a wider concept of rational criticism, one that not only makes room for both testing *and* contesting but also accepts their fundamental interdependence. Neither can secure practical reason alone; but *together* they can inform an adequate notion of sound professional practice.

Applied science, then, becomes *the art of testing* <u>*and*</u> *contesting practical claims – problem definitions and solutions – with a view to securing truly reflective practice.*

(5) Towards a new concept of "applied science and expertise": Our reflections leave us with a somewhat weakened concept of applied science. I do not mean to suggest, however, that we ought to abandon the quest for a "scientific attitude" (or "research orientation," as I have also called it) as an important pillar of strength in professional practice. Let us not confuse the idea of applied science with what our epoch has made of it. Who says we *need* to associate applied science with a merely instrumental concept of rationality, by purifying it of all ethical content and thereby losing sight of the other, normative dimension of practical reason? Who says we need to adopt a massively impoverished concept of criticism, by identifying it with the hypothetico-deductive method of testing and thereby leaving no room for other, truly discursive and participatory forms of critical argumentation? Who says there is no alternative to a means-end dualism that confuses purpose-rationality with rationality of purposes and thereby immunizes the consequences of "rational" action against the critical efforts of practical reason? And who, finally, is there to tell us we need to subscribe to a glib professional elitism, putting those whom applied science is supposed to serve (decision makers and citizens alike) forever in a situation of incompetence and dependency? Why, in one sentence, should we not be able to put applied science in the service of ethical, critical, and emancipatory ends? To ask the question is to answer it**:** I can't see any compelling reason, except if we fail to shed all those self-imposed limitations of "applied science" against which I have argued.

To remind us of this critical intent which I propose to associate with the concept of applied science, I have adopted in my writings a slightly different terminology. I speak of ***«applied science and expertise»*** rather than just "applied science"; for true expertise reaches beyond (and thus undermines) the mainstream notion of applied science. This alternative term, then, invites us to associate with expertise a scientific attitude or research orientation that is less impoverished than the prevailing model of applied science. The term also has the advantage that it is fresh and unused, as it has not yet been taken into possession by the applied science mainstream (try a Google search for "applied science and expertise" and you will see what I mean). Cultivated understanding sometimes calls for new language; a cultivated understanding of applied science means to go beyond it.

This *Bimonthly's* picture: the Nemesis of professional education One should never generalize and I don't mean to, but I fear in most domains of professional education today the kind of cultivated understanding of expertise that I advocate is not exactly a matter of course. The signs of the times are it isn't. Why should we expect that the loss of the "other," non-instrumental dimension of true expertise should go unpunished, that is, without a resulting loss of quality? In fact, signs of Nemesis abound. We encounter them every day, not only in education but also at work, at home, in the daily news. Many examples come readily to mind, I'll only mention two**:** contemporary management education, and contemporary architecture. Management has come a long way towards professionalization, but I am not convinced the result is good. Somewhere on the way, that nameless quality which distinguishes good management – call it entrepreneurship, leadership, responsibility, or whatever – appears to have been lost to an alarming degree. Similar picture in architecture**:** it, too, has come a long way toward professionalism; it, too, appears to have lost much of that "quality without a name" (Alexander, 1979, reviewed in Ulrich, 2006a) that makes all the difference between a building or a neighborhood which is alive and one that is dead.

My picture shows one of those typical neighborhoods (I am not sure the term is right) that are currently being built all around our home near Bern. It's not one of the worst examples; I wouldn't deny that it has some quality. But it is a quality that is made with the ruler, as it were. For a counter-example, move your mouse over the picture. See the difference? This second neighborhood has not been made with the ruler. It hasn't been made at all, it has grown, since medieval times! No ruler was apparently needed to let it grow and be alive. The first, contemporary neighborhood is not entirely without some obvious commitment to design and clear aesthetics; but it appears to lack that nameless quality which would allow it to come alive. It's not alive, it doesn't make us feel alive. Instead, it is Nemesis, the ancient Greek goddess in charge of the wrath of the gods and their just punishment, which is alive and all around us.

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Picture data Digital photograph taken on 27 May 2006 around 9 p.m. in the community of Köniz, Bern. ISO 50, aperture f/2.8, shutter speed 1/125, focal length 7.8 mm (equivalent to 38 mm with a conventional 35 mm camera). Original resolution 2272 x 1704 pixels, reduced to 700 x 525 pixels and compressed to 120 KB. The second picture, which appears when you roll your mouse over the basic picture, was taken on 11 February 2008 at 15:40 p.m. at Ligerz, Canton Bern, with ISO 100, aperture f/3.5, shutter speed 1/250, focal length 15 mm (equivalent to 30 mm with a 35 mm camera), original resolution 3648 x 2736 pixels, reduced to 700 x 525 pixels and compressed to 100 KB.

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May-June, 2008

Nemesis of contemporary professional education? *(mouse over for a counterpoint)*

„This is the timeless way of building:
learning the discipline – and shedding it."

(Christopher Alexander, *The Timeless Way of Building*, 1979, p. 16)

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