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Abstract This essay originates in a seminar with research students and staff of the Lincoln School of Management in Lincoln, UK, as well as in countless discussions with students and professionals from many other fields, on the nature of research competence in the applied disciplines. The aim was to guide the participants towards reflection on their personal notion of competence as researchers and (future) professionals, especially as seen from a perspective of critical systems thinking. By expanding the original working paper and making it available publicly, the author hopes to reach a wider group of readers who seek orientation in formulating or advancing a research project, or who quite generally wish to clarify and develop their notions of good research practice and professional competence. The essay addresses readers in a direct and personal way.

Essay history and suggested citation The original article was circulated as Working Paper No. 22, Lincoln School of Management, University of Lincoln (formerly University of Lincolnshire & Humberside), Lincoln, UK, June 1998 (Ulrich, 1998a). A first, slightly amended online version (in PDF format only) was prepared for the author's *Academia.edu* site in April 2017 and was also made available in the *Downloads* section of the author's home page. A different, considerably expanded version of the original essay had previously appeared under the title "The quest for competence in systemic research and practice" in *Systems Research and Behavioral Science*, Vol. 18, No. 1, 2001, pp. 3-28 (Ulrich, 2001); but since this journal is not published in open-access mode, the article has to this date remained unavailable to a wider audience who has no access to the journal. Copyright conditions still do not allow me to offer that essay in my home page. The present, first-ever HTML version is a thoroughly revised, expanded and updated version of the April 2017 article, with some text passages that are entirely new, or different or missing, as compared to the earlier versions.

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Note: With this and the next *Bimonthly*, I continue my plan of occasionally offering HTML versions of older publications of mine that so far have been available in PDF and/or hard print format only. I have revised and updated a working paper that dates back to my time at Lincoln University in England (1997-2000) and in which I report about a seminar on the nature of competent research as seen from a "systemic" perspective. As the new version has grown a bit long for a web page, I decided to split it up into two parts that appear in this and the next *Bimonthly*. The PDF version, however, offers an integral version of the full essay.

Introduction To "understand" means to be able to formulate a question that is answered accurately by what one assumes that one knows, or which at least tells us accurately what we do not know.¹⁾ Hence if we want to understand what it means to be "competent" in systemic research practice, we need first of all to ask what sort of question we are trying to answer through such competence. As research students pursuing a Ph.D. or a Master of Science degree here in Lincoln, most of you are, among other things, interested in systems thinking. You believe (or perhaps merely hope) that systems thinking is a meaningful thing to study. You invest personal hopes, time and effort in order to qualify as a systems researcher. So, if systems thinking is (part of) the answer, what is the question?

Systemic thinking and research competence I think it is indeed important for you to ask yourself this question. The way you understand "systemic" thinking will shape your understanding of "competent" research, and vice versa. For instance, it seems a reasonable starting point to assume that systemic thinking is about considering "the whole relevant system" that matters for understanding a problem or improving a situation. You will thus need to make sure that your problem definitions are *large enough* to include everything relevant; but what does that mean? Since we live in a world of ever-growing complexity, it could basically mean you need to do justice to the *interdependencies* of any problem with other problems, or of whatever aspects of the problem you focus on with other aspects of the same problem. So systemic thinking becomes the "art of interconnected thinking" (Vester (2007; cf. Ulrich, 2015), and you need to study methods for supporting such thinking. But then, making your problems large enough could also mean first of all to include a larger time horizon than usual, so as to make sure that problem solutions are sustainable *over time*; you would thus want to put the main focus of systemic thinking on ideas of sustainable development, on ecological and perhaps also evolutionary thought, and would have to acquire conforming knowledge and methods of inquiry. With equal right you might want to say that making problems large enough demands first of all that one consider the divergent views and interests of different parties concerned; which would associate systems thinking with *multiple perspectives* thinking,

stakeholder analysis, participatory research approaches, and so on. As this short and very incomplete enumeration makes immediately clear, a "systemic" perspective lends itself to many different notions of what competent inquiry and practice can and should mean.

Accordingly important it is for you to as a research student to ask yourself what kind of competence you are striving for. The primary concern is competence, not systems thinking. How can you study successfully without a clear understanding of your goal? Of course your immediate goal is to get a degree; but I suppose getting a degree makes sense only if it is linked to personal learning and growth. By acquiring some of the specific skills that you expect from systems thinking, you may wish to deepen your competencies as a future professional or become a better researcher than you already are. Or you feel a need to strengthen your capabilities in general rather than just as a researcher. Perhaps you already feel confident about your professional training and experience but would like to become a more reflective professional or even a more mature person in general. You may then want to read this essay thinking of yourself as a "student of competence" rather than as a "student of systems thinking" and/or a "student of research"; for students of competence, I take it, we all remain throughout our lives.

Towards a personal notion of competence Whatever your individual motives and state of preparation may be, I cannot formulate "the" relevant question for you. All I can attempt is to help you find *your own* individual question, by offering a few possible topics for reflection. As far as the paper also offers some considerations as to how you might deal with these topics, please bear in mind that I do not mean to claim these considerations are the only relevant or valid ones (a claim that again would presume one has found the one, right question to be asked when it comes to competence). I offer them as examples only. Their choice looks relevant to me at this particular moment in my academic and personal biography; but you are different persons and will therefore have to pursue your quest for competence in your own unique way. Contrary to academic custom, the game for once is not to be right but only to be true to yourself.

The Burden of Becoming a "Researcher" As a research student you are supposed to do "research." Through your dissertation, you have to prove that you are prepared to treat an agreed-upon topic in a scholarly manner; in other words, that you are a competent researcher. Not surprisingly, then, you are eager to learn *how* to be a competent researcher. But I suspect that few of you are quite sure what precisely is expected from you. Hence the job of "becoming a competent researcher" is likely to sound like a tall order to you, one that makes you feel a bit uncomfortable, to say the least. What do you have to *do* to establish yourself as a "competent" researcher?

From what you have been told by your professors, you probably have gathered that being a competent researcher has something to do with being able to choose and apply *methods*. Methods, you have understood, should be appropriate to the problem you are dealing with and should help you to produce findings and conclusions that you can explain and justify in methodological terms. That is to say, you should be able to demonstrate how your findings and conclusions result from the application of chosen methods and why methods and results are all valid.

Questions concerning method Previous to this seminar, I have spoken to many of you individually and I have felt that most of you worry a lot about *which methods* you should apply and how to justify your choices. It really seems to be an issue of choice rather than theory. There are so many different methods! The choice appears to some extent arbitrary. What does it mean to be a competent researcher in view of this apparent arbitrariness? You may have turned to the epistemological literature in order to find help, but what you have found is likely to have confused you even more. The prescriptions given there certainly seem abstract and remote from practice, apart from the fact that the diverse prescriptions often enough appear to conflict with one other.

As a second difficulty, once you have chosen a methodology and start to apply it, you will at times feel a strong sense of uncertainty as to *how to apply* it correctly. Methods are supposed to give you guidance in advancing step by step. You expect them to give you some security as to whether you are approaching your research task in an adequate way, so as to find

interesting and valid answers to your research questions. But instead, what you experience is a lot of problems and doubts. There seem to be more questions than answers, and whenever you dare to formulate an answer, there again seems to be a surprising degree of choice and arbitrariness. What answers you formulate seems to be as much a matter of choice as what method you use and how exactly you use it.

Given this burden of personal choice and interpretation, you may wonder how you are supposed to know whether your observations and conjectures are the right ones. How can you develop confidence in their *quality*? How can you ever make a compelling argument concerning their *validity*? And if you hope that in time, as you gradually learn to master your chosen method, you will also learn how to judge the quality of your observations, as well as to justify the validity of your conclusions, yet a third intimidating issue may surface: how can you ever carry the burden of *responsibility* concerning the actual consequences that your research might have if it is taken seriously by other people, for example, in an organization whose problems you study and which then, based on your findings and conclusions, may implement changes that cost jobs or affects people in other ways?

As a fourth and final example of such worries, your major problem may well be to *define "the problem"* of your research, that is, the issue to which you are supposed to apply methods in a competent fashion. This is indeed a crucial issue, but here again the epistemological and the methodological literature is rarely of help. Its prescriptions seem so remote from your needs!

A lot of questions to worry about, indeed. But didn't we just say that without questions there is no understanding? So take your questions and doubts as a good sign that you are on your way towards understanding. Let us explore together where this way might lead you. One thing seems certain: if you do not try to understand where you want to go, you are not likely to arrive there!

The Death of the Expert²⁾ Sometimes it is easier to say what our goal is *not*, rather than what it is. Are there aspects or implications of "competence" that you might wish to exclude from your understanding of competence in research? Certainly.

For instance, in what way do you aim to be an "expert" on systems methodologies (or any other set of methodologies), and in what way do you *not* want to become an "expert"? To be competent in some field of knowledge means to be an expert, doesn't it? The role that experts play in our society is so prominent and seemingly ever more important that a lot of associations immediately come to our mind. To mention just three: experts seem to be able to make common cause with almost any purpose; most of the time (except when they are talking about something we, too, happen to be experts in) experts put us in the situation of being "lay people" or non-experts (i.e., incompetent?); experts frequently cease to reflect on what they are doing and claiming. So, are there roles you would rather not want to play, causes you'd rather not serve, as a competent researcher? Are there circumstances or situations in which you would rather *not* claim to be an expert, that is, rely on, and refer to, your "expertise"? Where do you see particular dangers of ceasing to be self-critical?

Expertise or the pitfall of claiming too much Ceasing to be self-critical, with the consequent risk of claiming too much, is unfortunately very easy. There are so many aspects of expertise or competence that need to be handled self-critically! So much seems clear: as competent researchers we do not want to ignore or conceal the *limitations* of the methods on which our competence depends – "methods" in the widest possible sense of any systematically considered way to proceed. The limitations of a method are among its most important characteristics; for if we are not competent in respecting these limitations, we are not using the method in a competent manner at all. Hence, one of the first questions we should ask about every method concerns its limitations.

Technically speaking, the limitations of a method may be said to be contained in the *theoretical and methodological assumptions* that underpin any reliance on it. Some of these may be built into a method we use; others may arise due to the imperfect ways in which we use it, for example, if we don't master the method or use it in biased ways.

Perhaps an even more basic assumption is that experts, by virtue of their expertise, have a proper grasp of the situations to which they apply their

expertise, so that they can properly judge what method is appropriate and this choice will then ensure valid findings and conclusions. Experts often seem to take such assumptions for granted, or else tend to cover them behind a facade of busy routine.

Sources of deception To the extent that we are insensible to these assumptions, they threaten to become *sources of deception*. We ourselves may be deceived as researchers, but inadvertently we may also deceive those who invest their confidence in our competence. There need not be any deliberate intention to deceive others on the part of the researcher; it may simply be *routine* that stops researchers from revealing to themselves and to others concerned the specific assumptions that flow into every concrete application of their expertise. Even so, this is probably not what you would like to understand by "competence."

The earlier-mentioned questions and doubts that plague many a research student are then perhaps indeed a healthy symptom that your research competencies have not yet reached the stage of routine where such lack of reflection threatens. This danger is more of a threat to established researchers who have already become recognized as experts in their field of competence. Although some degree of routine is certainly desirable, it should not be confused with competence. *Routine implies economy, not competence.*

When experts forget this distinction, they risk suffering the silent *death of the expert*. It seems to me at times that in our contemporary society, the death of the expert has taken on epidemic dimensions! We are facing an illness that has remained largely unrecognized or incorrectly diagnosed, perhaps because it causes an almost invisible death, one that often enough is hidden by the vigorous and impressive behavior patterns of those who have developed the disease.

There is a second cause of the death of the expert that we must consider. Even if a researcher remains thoroughly aware of the methodological and theoretical underpinnings of his or her competence and makes an appropriate effort to make them explicit, does that mean that the research findings provide a valid ground for *practical conclusions*? This is often assumed to be the case, but repeated assumption does not make a proposition valid. A

sound theoretical and methodological grounding of research – at least in the usual understanding of "theory" and "methodology" – implies at best the empirical (i.e., descriptive) but not the normative (i.e., prescriptive) validity of the findings. Well-grounded research may tell us what we can and cannot do, but this is different from what we *should* do on normative grounds.

The virtue of self-limitation When it comes to that sort of issue, the researcher has no advantage over other people. Competence in research then gains another meaning, namely, that of the *researcher's self-restraint*. No method, no skill, no kind of expertise answers all the questions that its application raises. One of the most important aspects of one's research competence is therefore to understand the questions that it does *not* answer.

The number of questions that may be asked is, of course, infinite – as is, consequently, the number of questions that competence cannot be expected to answer. You have thus good reason to worry about the meaning of competence in research. If you want to become a competent researcher, you should indeed never stop worrying about the limitations of your competence! As soon as you stop worrying, the deadly disease may strike. The goal of your quest for competence is not to be free of worries but rather to learn to make them a source of continuous learning and self-correction. That is the spirit of competent research. Competence in research does not mean that research becomes a royal road to certainty. What we learn today may (and should) always make us understand that what we believed yesterday was an error. The more competent we become as researchers, the more we begin to understand that competence depends more on the questions we ask than on the answers we find. It is better to ask the right questions without having the answers than to have the answers without asking the right questions. If we do not question our answers properly, we do not understand them properly, that is, they do not mean a lot.

This holds true as much in the world of practice as in research, of course. The difference may be that under the pressures of decision making and action in the real world, the process of questioning is often severely constrained. It usually stops as soon as answers are found that serve the given purpose. As a competent researcher, your focus will be more on the

limitations of the answers and less on limiting the questions. This is what a researcher's well-understood self-limitation is all about.

A preliminary definition of competence in research Your tentative first definition of competency in research, then, might be something like this (modify as necessary):

Competence in research means to me pursuing a self-reflective, self-correcting, and self-limiting approach to inquiry. That is, I will seek to question my inquiry in respect of all conceivable sources of deception, for example, its (my) presuppositions, its (my) methods and procedures, and its (my) findings and the way I translate them into practical recommendations.

In this tentative definition, the pronoun "its" refers to the inherent limitations of whatever approach to inquiry I may choose in a specific situation, *limitations that are inevitable* even if I understand and apply that approach in the most competent way. The pronoun "my," in contrast, refers to my *personal limitations* in understanding and applying the chosen approach. Accordingly, the essential underlying question is how as a researcher you are to deal adequately with these limiting factors in the quest for relevant, valid, and responsible research. The three underlined phrases stand for key notions in my personal attempt to respond to this question. Given their personal nature, I encourage you to interpret, question and modify them according to your own experiences, needs, and hopes. Do not allow your thinking to be limited by them! The only reason I introduce them here is that they inform my personal concept of research and thus may help you in better understanding (and thus questioning) the reflections on the nature of competent research offered in this essay.

A major implication of this preliminary definition is the following. Competence in research means more – much more – than mastering some research tools in the sense of knowing *what* methodology to choose for a certain research purpose and *how* to apply it in the specific situation of interest. Technical mastery, although necessary, is not equal to competence. It becomes competence only if it goes hand in hand with at least two additional requirements:

(a) that we learn to cultivate a continuous (self-) critical observation of the built-in limitations of a chosen research approach – "observing" its limitations, that is, in the double sense of "understanding" *and* "respecting" them; and, perhaps even more importantly and certainly more radically,

(b) that we quite generally renounce the notion that we can ever justify the validity of our eventual findings by referring to the proper choice and application of methods.

The obvious reason for (b) is that justifying findings by virtue of methods does little to justify the *selectivity* of those findings regarding both their empirical and their normative content, that is, the circumstances taken to be relevant for understanding a situation and the criteria considered adequate for evaluating or improving it. Selectivity results from inherent limitations of methods as well as from the limitations of our resources and understanding in applying them (which is not to say that there are no other sources of selectivity, such as personal world views and interests or institutional, political and economic mechanisms and pressures).

The *limited justificatory power of methods* is bad news, I fear, for some of you who probably have been taught to base your search for competence on the idea of a theoretically based choice among methodologies. To be sure, there is nothing wrong with this idea – so long as you do not expect it to ensure critical inquiry. The notion of securing critical inquiry and practice through theoretically based methodology choice is currently prominent in systems research and particularly in the methodological discussions around the notion of critical systems thinking (CST); but I invite you to adopt it with caution. It does not carry far enough.³⁾

Further sources of orientation and questioning We must ask, then, what else can give us the necessary sense of orientation and competence in designing and critically assessing our research, if not (or not alone) the power of well-chosen methods? I suggest that you consider first of all the following three additional sources of orientation that I have found valuable (among others), namely:

- understanding your personal quest for "improvement" in each specific inquiry;
- observing what following Kant I call "the primacy of practice in research";
- recognizing and using the significance of C.S. Peirce's "pragmatic maxim."

Further considerations will then concern the concepts of

- "systematic boundary critique";
- "high-quality observations";
- cogent reasoning or compelling argumentation;
- mediating between theory and practice (or science and politics); and finally,
- the "critical turn" that informs my work on critical systems heuristics.

The Quest for Improvement One of the sources of orientation that I find most fundamental for myself is continuously to question my research with regard to its underlying concept of improvement. How can I develop a clear notion of what, in a certain situation, constitutes "competent" research, without a clear idea of *the difference it should make?*

The "difference it should make" is a pragmatic rather than merely a semantic category, that is, it refers to the implications of my research for some domain of practice. If I am pursuing a purely theoretical or methodological research purpose, or even meta-level research in the sense of "research on research," the practice of research itself may be the domain of practice in which I am interested primarily; but when we do "applied" research in the sense of inquiry into some real-world issue, it will have implications for the world of social practice, that is, the life-worlds of individuals and their interactions in the pursuit of individual or collective (organizational, political, altruistic, etc.) goals.

In either case I will need to gain a clear idea of the specific domain of practice that is to be improved, as well as of the kind of improvement that is required. One way to clarify this issue is by asking what group of people or organizations belong to the intended "client" (beneficiary) of a research project, and what other people or organizations might be affected, whether in a desired or undesired way. (Note that from a critical point of view, we must not lightly rule out the possibility of undesired side-effects; hence, when we seek to identify the people or organizations that might be affected, we should err on the side of caution and include all those whom we cannot safely assume *not* to be affected.) Together these groups of people or organizations constitute the domain of practice that I will consider as relevant for understanding the meaning of "improvement."

What makes research valuable? Once the client and the respective domain of practice are clear, the next question concerns the sort of practice that my research is supposed (or, critically speaking, likely) to promote. The competence of a research expresses itself not by its sheer beauty but by its value to the practice it is to support. In order to have such value, it must be *relevant* – answer the right questions; and *valid* – give the right answers. But how can we, as researchers, claim to know (i.e., stipulate) the kind of practice to which we should contribute? Have we not been taught long enough that competent ("scientific") inquiry should refrain from being purpose and value driven?

The German sociologist and philosopher of social science Max Weber (1991, p. 145) has given this concern its most famous formulation: "Politics is out of place in the lecture room." I can appreciate Weber's critical intent, namely, that academic teaching should be oriented towards theory rather than towards ideology. But can that mean, as Weber is frequently understood, that research is to be "value-free"?⁴⁾ A better conclusion, in my opinion, would be that as researchers we must make it clear to ourselves, and to all those concerned, what values our research is to promote and *whose* values they are; for whether we want it or not, we will hardly ever be able to claim that our research serves all interests equally. We cannot gain clarity about the "value" (relevance and validity) of our research unless we develop a clear notion of what kind of difference it is going to make and to whom. A clear sense of purpose is vital in competent research.

If you have experienced blockages in advancing your project, for example in defining research strategies and so on, ask yourself whether this might have to do with the lack of a sense of purpose. *When you do not know what you want to achieve, it is very difficult indeed to develop ideas.* Conversely, when your motivation and your vision of what you want to achieve are clear, ideas will not remain absent for long. Your personal vision of the difference that your research should make can drive the process of thinking about your research more effectively than any other kind of reflection.

The Primacy of Practice As research students studying for a Ph.D. or M.Sc. degree, your preoccupation with the question of "how" to do proper

research is sound. But as we have just seen, the danger is that as long as you put this concern above all others, it will remain difficult to be clear about what it is that you want to achieve. For it means that you rely unquestioningly on a very questionable assumption, namely, that good practice (P) – "practice" in the philosophical sense of *praxis* rather than in the everyday sense of "exercise" – is a function (f) of proper research (R), whereby "proper" essentially refers to adequate research methodology:

$$P = f(R)$$

Good research should of course serve the purpose of assuring good practice; but does it follow that the choice of research approaches and methods should determine what is good practice? I do not think so. Quite the contrary, it seems to me that good research should be a function of the practice to be achieved:

$$R = f(P)$$

Your primary concern, then, should not be *how* to do proper research but *what for*. This conjecture requires an immediate qualification, though, concerning the source of legitimation for the "what for." Note that in our inverted formula, practice (P) is no longer the dependent variable but is now the independent variable. This is precisely as it should be: *It is not up to the researcher to determine what is the right (legitimate) "what for."* Rather, it is the researcher's obligation to make it clear to herself or himself, and to all those concerned, what might be the practical implications of the research, that is, what kind of practice it is likely to promote – the factual "what for."

After that, *practice must itself be responsible for its purposes and measures of improvement*. Researchers may be able to point out ways to "improve" practice according to certain criteria, but they cannot delegate to themselves the political act of legitimizing these criteria (cf. Ulrich, 1983, p. 308). It is an error to believe that good practice can be justified by reference to the methods employed. Methods need to be justified by reference to their implications for practice, not the other way round!

In competent research, the choice of research methods and standards is secondary, that is, a function of the practice to be achieved. Good practice

cannot be justified by referring to research competence. Hence, let your concern for good research follow your concern for understanding the meaning of good practice, rather than vice versa.

The suggested primacy of the concern for the outcome of a research project over the usually prevailing concern for research methodology (the "input," as it were) is somewhat analogous to the primacy that Kant assigns to the practical over the theoretical (or speculative) employment of reason, or to what he refers to as the "primacy of practical reason in its union with speculative reason" (Kant, 1788, A 215, 218; cf. 1787, B 824f, 835f). Theoretical reason can generate valid knowledge only within the bounds of experience; but practical reason can conceive of ideas such as the moral idea that help us ensure good practice and thereby to create a new, different reality. Theoretical reason can tell us what we can and can't do and how to achieve it, but not what for (to what ends and according to what standards) we should try to achieve it. For Kant it is therefore practical-moral rather than theoretical-instrumental reasoning that has to play a leading role in the way we use reason, for it alone can lead us beyond the latter's limitations. I would therefore like to think of our conclusion in terms of a *primacy of practice in research*. But again, the point is not that it is upon the researcher to determine the right "what for"; the point is, rather, that well-understood reasoning involves a normative dimension for which theoretical and methodological expertise does not provide a privileged qualification.

Towards a two-dimensional concept of research competence Accordingly, the concept of competent research that I suggest here is based on Kant's *two-dimensional concept of reason*. This distinguishes it from the more usual concept of competence in research and professional practice that is implicit in most contemporary conceptions of knowledge and of science and which has lost sight of the normative dimension of rationality. I am thinking particularly of the model of empirical-analytic science (so-called science-theory) that has come to dominate the actual practice of science in many domains, a model that is rooted in the logical empiricism of the so-called Vienna Circle of the 1930s (Schlick, Carnap, Reichenbach and others) but which has since been developed and has found its most widely adopted expression today in the work of Popper (1959, 1963, 1972) on "critical

rationalism." Symptomatically, Popper replaced Kant's primacy of practical over theoretical reason with a one-sided primacy of theory, a model that in effect reduces practical to instrumental reason while relegating practical reasoning properly speaking, including moral reasoning, to a merely subjective and indeed non-rational status. For those interested, I have elsewhere explained and discussed this prevalent but impoverished model of rationality, for which the reach of reason is equal to that of science, in detail (see, e.g., Ulrich, 1983 and 2006c).

To conclude this brief discussion of the suggested primacy of practice in research, let us consider an example of what it means in actual research practice. Research into poverty provides a good illustration with which I am familiar through my own engagement in this field (Ulrich and Binder, 1998). Poverty researchers are often expected to tell politicians "objectively" how much poverty there is in a certain population and what can be done about it. But measuring poverty is not possible unless there are clear criteria of what standards of income, well-being, and participation in society (both material and immaterial) are to be considered "normal" for a decent life and accordingly should be available to all members of that population. If poverty research is to be done in a competent way, so that it can tell us who and how many of us are poor and what are their needs, there must first be a concrete vision of the kind of just society to be achieved! This is what I mean by the primacy of practice in research.

The Pragmatic Maxim The orientation provided by a well-understood primacy of practice must not be confused with mere "pragmatism" in the everyday sense of orientation toward what "works" or serves a given purpose. The point is not utilitarianism but the clarity of our thinking that we can obtain through clarity of purpose. This idea was first formulated by Charles S. Peirce (1878) in his *pragmatic maxim*, in a now famous paper with the significant title "How to make our ideas clear":

Consider what effects, which might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object. (Peirce, 1878, para. 402)

The pragmatic maxim requires from us a comprehensive effort to bring to the

surface and question the implications (i.e., the actual or potential consequences) that our research may have for the domain of practice under study. Contrary to popular pragmatism, according to which "the true is what is useful," the pragmatic maxim for me represents a critical concept. The true is not just what is useful but what considers all practical implications of a proposition, whether it supports or runs counter to my purpose. Uncovering these implications thus becomes an important virtue of competent inquiry and design in general, and of critical systems thinking in particular.

Pragmatism calls for a critical stance There is a crucial *critical kernel* in the pragmatic maxim that we need to uncover and move to the center of our understanding of pragmatism. I understand it as follows. Identifying the actual or conceivable consequences of a proposition, as Peirce requires it of a pragmatic researcher, is not a straightforward task of observation and reasoning but raises difficult theoretical as well as normative issues. Theoretically speaking, the question is, what can be the empirical scope of our research? Normatively speaking, the question is, what should we consider as relevant practical implications? Peirce's solution is of course to consider *all* conceivable implications; but for practical research purposes that answer begs the question. The question is, how can we limit the inquiry to a manageable scope yet claim that its findings and conclusions are relevant and valid? The quest for comprehensiveness is reserved to heroes and gods; it is beyond the reach of ordinary researchers. What we ordinary researchers recognize as relevant implications depends on *boundary judgments* by which we consciously or unconsciously delimit the situation of concern, that is, the totality of "facts" (empirical circumstances) and "norms" (value considerations) that determine the definition of "the problem" and its conceivable "solutions." The response to Peirce's challenge can thus only be that we must make it clear to ourselves, and to all others concerned, in what way we (or they) may fail to be comprehensive, by undertaking a systematic critical effort to disclose those boundary judgments.

Systematic Boundary Critique In *Critical Heuristics* (Ulrich, 1983, see esp. Chapter 5), I conceived of this critical effort as a process of systematic *boundary critique*,⁵⁾ that is, a methodical process of reviewing boundary

judgments so that their selectivity and changeability become visible. **Table 1** shows a list of boundary questions that can be used for reviewing a claim's sources of selectivity; you'll find elsewhere more complete accounts of the boundary categories that inform these questions, and of the underlying framework of critical systems heuristics (CSH).⁶⁾

**Table 1: Sources of selectivity:
The boundary questions of critical systems heuristics**

(Adapted from Ulrich, 1984, p. 338-340; 1987, p. 279; 1993, p. 597;
1996a, pp. 24-31; 2000, p. 258)

SOURCES OF MOTIVATION

- (1) Who is (ought to be) the **client**? That is, whose interests are (should be) served?
- (2) What is (ought to be) the **purpose**? That is, what are (should be) the consequences?
- (3) What is (ought to be) the **measure of improvement**? That is, how can (should) we determine whether and in what way the consequences, taken together, constitute an improvement?

SOURCES OF POWER

- (4) Who is (ought to be) the **decision maker**? That is, who is (should be) in a position to change the measure of improvement?
- (5) What **resources** are (ought to be) controlled by the **decision maker**? That is, what conditions of success can (should) those involved control?
- (6) What conditions are (ought to be) part of the **decision-environment**? That is, what conditions does (should) the decision maker not control (e.g., from the viewpoint of those not involved)?

SOURCES OF KNOWLEDGE

- (7) Who is (ought to be) involved as a **professional**? That is, who is (should be) involved as an expert, e.g., as a system designer, researcher, or consultant?
- (8) What **expertise** is (ought to be) consulted? That is, what counts (should count) as relevant knowledge?
- (9) What or who is (ought to be) assumed to be the **guarantor**? That is, what is (should) be considered a source of guarantee (e.g., consensus among experts, stakeholder involvement, support of decision-makers, etc.)?

SOURCES OF LEGITIMATION

- (10) Who is (ought to be) **witness** to the interests of those affected but not involved? That is, who is (should be) treated as legitimate stakeholder, and who argues (should argue) the case of those stakeholders who cannot speak for themselves, including the handicapped, the unborn, and non-human nature?
- (11) What secures (ought to secure) the **emancipation** of those affected from the premises and promises of those involved? That is, where does (should) legitimacy lie?
- (12) What **world view** is (ought to be) assumed? That is, what different visions of improvement are (should be) considered and somehow reconciled?

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For me this critical effort of disclosing and questioning boundary judgments serves a purpose that is relevant both ethically and theoretically. It is relevant theoretically because it compels us to consider new "*facts*" that we might not consider otherwise; it is relevant ethically because these new facts are likely to affect not only our previous notion of what is empirically true but also our view of what is morally legitimate, that is, our "*values*."

To be sure, what I propose to you here is not as yet a widely shared concept of competence in research, but I find it a powerful concept indeed. Once we have recognized the critical significance of the concept of boundary judgments, we cannot go back to our earlier, "pre-critical" concept of competent research in terms of empirical science only. It becomes quite impossible to cling to a notion of competent research that works in just one dimension. This is so because what we recognize as "facts" and what we recognize as "values" become interdependent.

The question of what counts as knowledge, then, is no longer one of the quality of empirical observations and underpinning theoretical assumptions only; it is now also a question of the proper bounding of the domain of observation and thus of the underpinning value judgments as to what *ought* to be considered the relevant situation of concern. What counts as knowledge is, then, always a question of what *ought* to count as knowledge. We can no longer ignore the practical-normative dimension of research or relegate it to a non-rational status.

What Ought to Count as Knowledge? Research is usually undertaken to increase knowledge. A typical dictionary definition explains that research is "to establish facts and reach new conclusions" (*Concise Oxford Dictionary of Current English*). This is not a bad definition. Counter to the frequent, often tacit identification of research with empirical research, the Oxford definition tells us that research requires two kinds of competencies:

- observational skills to "establish facts," and
- argumentative skills to "reach new conclusions."

The first kind of skills refers to the ideal of *high-quality observations*, that is, observations that are capable of generating valid statements of fact. This

ideal is traditionally but rather inadequately designated "objectivity"; it requires our propositions or claims to possess observational qualities such as intersubjective transferability and controllability, repeatability over time, adequate precision, and clarity with respect to both the object and the method of observation.

The second kind of skills refers to the ideal of *cogent reasoning*, that is, processes of (individual) reflection and (intersubjective) argumentation that generate valid statements about the meaning (interpretation, justification, relevance) of observations. This ideal is traditionally designated "rationality"; it requires our propositions to possess communicative and argumentative qualities such as syntactic coherence, semantic comprehensibility, logical consistency with other statements, empirical content (truth), pragmatic relevance and normative legitimacy (rightness).

Both requirements raise important issues for the concept of research competence. How can we know whether we "really" know, that is, whether our observations are high-quality observations or not? And if we can assume that they are, how can we know whether we understand their meaning correctly and draw the "right" conclusions, that is, that we reason and argue correctly?

Competent observation and argumentation require one another A particular difficulty with the two requirements is indeed that they are inseparable. This becomes obvious as soon as we consider the nature of the "facts" that quality observations are supposed to establish:

Facts are what statements (when true) state; they are not what statements are about [i.e., objects]. They are not, like things or happenings on the face of the globe, witnessed or heard or seen, broken or overturned, interrupted or prolonged, kicked, destroyed, mended or noisy. (Strawson, 1964, p. 38, cf. Ulrich, 1983, p. 132)

That is to say, facts are not to be confused with objects of experience; they cannot be experienced (they are statements rather than objects), just as objects of experience cannot be asserted (only statements can). Facts, because they are statements, need to be *argued*. Accordingly observational and argumentative competencies must go hand in hand; they are but two sides of one and the same coin. (**Fig. 1**)

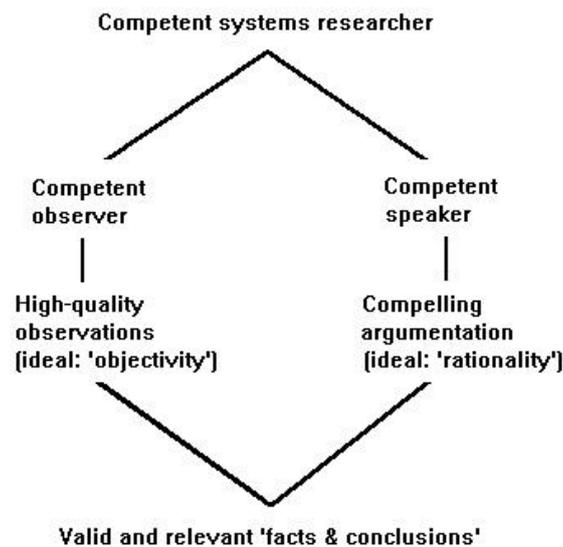


Fig. 1: Two dimensions of competence in systems thinking and research: observational and argumentative.

Each dimension entails specific validity claims, the redemption of which may, however, involve claims that refer to the other dimension.

Let us consider some of the specific requirements on each side of the coin. On the argumentative side, Habermas' (1979; 1984-87) well-known model of rational discourse gives us a framework for analyzing the difficult implications of the quest for compelling argumentation or, as he puts it, "communicative competence."

What makes a good argument? According to this model, a competent speaker would have to be able to justify (or "redeem," as Habermas likes to say) the following validity claims that all rationally motivated communication entails and which together amount to a "universal validity basis of speech":

1. *Comprehensibility*: a claim that entails the obligation to express oneself so that the others can hear and understand the speaker; it cannot be redeemed discursively but merely through one's communicative behavior.
2. *Truth*: a claim that entails the obligation to provide grounds for the empirical content of statements, through reference to quality observations and through theoretical discourse.
3. *Rightness*: a claim that entails the obligation to provide justification for the normative content of statements, through reference to shared values (e.g., moral principles) and through practical discourse.

4. *Truthfulness*: a claim that entails the obligation to redeem the expressive content of statements by proving oneself trustworthy, so that the others can trust in the sincerity of the speaker's expressed intentions; again this cannot be redeemed discursively but only through the consistency of the speaker's behavior with the expressed intentions.

(adapted from Habermas, e.g., 1979, pp. 2-4, 63-68)

Since these validity claims are always raised simultaneously in all communication, whether explicitly or implicitly, it becomes apparent that a competent researcher must be prepared to substantiate statements of fact not only through credible reference to quality observations but also through theoretical and practical discourse, so as to convince those who doubt or contest the "facts" in question of the validity of their underlying, theoretical and normative presuppositions.

What makes a good observation? Similar difficulties arise with the requirement of substantiating the quality of observations. Observations – or more precisely, observational statements – always depend on the construction of some sort of objects that can be observed and reported upon. Depending on the situation, these constructions may need to rely on different notions of what kinds of objects lend themselves to quality observations. A conventional notion of "objects" assumes that the objects of observation can be construed to be largely independent of the purposes of both the observer and the user of the generated knowledge. In such a conventional account, a claim to quality observations will entail the obligation to redeem at least the following requirements:

1. *Validity*: the observation observes (or measures) what it is supposed to observe (or measure).
2. *Reliability*: the observation can be repeated over time and provides (at least statistically) a stable result.
3. *Transferability*: the observation can be repeated by other observers and in that sense proves to be observer-independent (a validity claim that is often subsumed under 2).
4. *Relevance*: the observation provides (together with other observations) information that serves as a support for a statement of fact, or for an argument to the truth of some disputed fact.

The "challenge of the user": Towards a richer construction of high-quality observations and arguments Historically speaking, these or similar assumptions characterized the rise of the empirical sciences (especially the natural sciences) about three centuries ago. More recently, however, with the extension of scientifically motivated forms of inquiry to ever more areas of human concern, competent research increasingly faces the difficulty that contrary to the original assumptions, quality observations cannot be assumed to be independent of either the observer or the user or both. As for instance de Zeeuw (1996, pp. 2f and 19f /2001, pp. 1f and 25f) observes, science is now more and more faced with the *challenge of the user*, that is, the task of constructing quality observations so that they allow users to have a voice inside science. This is different from conventional science which, because of its underlying notion of non-constructed, observer- and user-independent objects, depends on the exclusion of users.

Typical examples are research efforts in the domains of therapy (e.g., psychiatry), social work and social planning (e.g., care for the elderly or fighting poverty), business management (e.g., organizational design, management consultancy), and public policy-making (e.g., policy analysis, evaluation research). "Patients," "clients," and "decision-makers" increasingly claim a voice in the making of the observations of concern to them, so that "diagnoses," "help" or "solutions" are not merely imposed upon them without considering *their* observations. What does it mean for a researcher to assure high-quality observations under such circumstances?

Three phases of science: expanding the reach of high-quality observation De Zeeuw has discussed this issue extensively (e.g., 1992, 1995, 1996/2001, 2005). He distinguishes three notions of "objects" that allow quality observations responding to different circumstances (the examples are mine): *non-constructed objects* (e.g., the seemingly given, observer-independent objects of astronomy such as the celestial bodies and phenomena),⁷⁾ *constructed objects* (e.g., groups such as "the poor" or "the upper class" as objects of the social sciences, or "systems" as objects of the systems sciences), and *self-constructed objects* (e.g., expressions of human intentionality as objects of study in social systems design, organizational analysis, environmental and social impact assessment, action research etc., where the construction of the

objects to be observed is left to those who are concerned in the observations at issue, be it because they may be affected by them or because they may need them for learning how to achieve some purpose, or else because they may be able to contribute some specific points of view). These three notions of objects give rise to three developments of science which de Zeeuw calls "first-phase," "second-phase," and "third-phase" science.

If I understand de Zeeuw correctly,⁸⁾ the constructed objects of second-phase science distinguish themselves from objects of first-phase science in that they depend on the observer's purpose (e.g., the improvement of some action or domain of practice); the self-constructed objects of third-phase science depend, moreover, on the full participation of all the users of the knowledge that is to be gained.

The emancipatory turn The notion of competent research that I propose here and which is also contained in my work on critical heuristics (CSH), critical systems thinking (CST) for citizens, and critical pragmatism, is certainly sympathetic to the idea of combining the "challenge of the user" with an adequate notion of (objects of) high-quality observations, a notion of quality that – in my terms – would give a competent role to all those concerned in, or affected by, an inquiry. I thus quite agree with de Zeeuw (1996, p. 19, 2001, p. 24) when he refers to CSH as an example of a type of inquiry that focuses on "the need to give users in general a voice inside science," so as to overcome the conventional limitation of quality observations to objects that are constructed by researchers without the full participation of users. It should be noted clearly, however, that CSH aims beyond the instrumental purpose of improving the quality of "scientific" observations; it also aims at *emancipating* ordinary people from the situation of incompetence and dependency in which researchers and experts frequently put them in the name of science. It aims at the earlier-mentioned insight that what in our society counts as knowledge is always a question of what *ought* to count as knowledge, whence the issues of democratic participation and debate and of the role of citizenship in knowledge production become essential topics. That is why I find it important to associate the challenge of the user with the goal of allowing citizens (as well as researchers) to acquire a *new competence in citizenship* (Ulrich, 1995,

1996a, b, 1998b, 2000, 2012a).

Towards a new symmetry of critical competence For me, a fundamental source of such competence consists in learning to handle the boundary judgments that inevitably underpin all application of research and expertise. The crucial point is that when it comes to boundary judgments, researchers or experts have no in-principle advantage over ordinary citizens:

When an expert, by reference to his theoretical knowledge, defines "the problem" at hand or determines "the solution," he must always presuppose such boundary judgments. To define the problem means, in fact, to map the social reality (or the social system) to be dealt with; to determine the solution means to design a better social reality (or social system). And since every map or design depends on previous boundary judgments (or whole systems judgments) as to what is to be included in it and what is to belong to its environment, it is clear that no definition of "the problem" or "the solution" can be objectively justified by reference to theoretical knowledge. It can only be critically justified by reference to both the transparency of values and the consent of all the affected citizens.

The first implication is trivial: *no amount of expertise (theoretical knowledge) is ever sufficient for the expert to justify all the judgments on which his recommendations depend.* When the discussion turns to the basic boundary judgments on which the exercise of expertise depends, the expert is no less a layman than are the affected citizens.

The second implication is less trivial, in that it seems to contradict common sense: *no expertise or theoretical knowledge is required to comprehend and to demonstrate that this is so.* The necessity of boundary judgments can be intuitively grasped by every layman: since no one can include "everything" in his maps or designs, he cannot help presupposing some boundaries.... Anybody who is able to comprehend the [relevance of such] boundary judgments is also able to see through the dogmatic character of the expert's "objective necessities." (Ulrich, 1983, p. 306, italics original)

To be sure, experts are still needed to inform all those without special expertise in an issue at hand (and virtually all of us find ourselves in this position most of the time) about the likely or possible consequences of different boundary assumptions, and thus about the options for efficacious action and resulting kinds of improvement, side-effects, and risks. But they have no privileged position when it comes to *choosing* among these options, and thus among the competing boundary judgments:

Experts may be able to contribute to the task of anticipating the practical consequences of alternative boundary judgments; but they cannot delegate to themselves the political act of sanctioning the normative content of these consequences. (Ulrich, 1983, p. 308)

This explains why professionals, counter to what one might expect, have no natural advantage over ordinary citizens with respect to boundary judgments. There is, in principle, a *symmetry of critical competence* (Ulrich, 1993,

p. 604) between citizens and professionals, as both sides have an equal chance of handling boundary questions in self-reflective and transparent ways (for fuller accounts, see Ulrich, 1983, entire ch. 5, esp. pp. 305-310; 1987, p. 281f; 1993, pp. 599-605, 2000, p. 254). The need for a careful and open handling of boundary judgments thus translates into a new skill of *boundary critique*, a skill that in principle is available equally to citizens and to professionals as it does not depend on any special expertise that would be beyond the comprehension of ordinary people. Once we understand this implication, our concept of high-quality observation of situations will change, as will also our concept of compelling argumentation.

Limitations of science theory and research methodology But of course, giving users a more competent voice within research does not answer all the questions raised by the search for valid and relevant "findings and conclusions." The deeper reason for this is that we are dealing with an ideal. A competent researcher will always endeavor to make progress toward it, while never assuming that he or she has attained it.

Wanting theories of truth and rationality We do not currently have, and chances are we will never have, *operational* theories of "true" knowledge and "rational" argumentation. Given this situation, along with the ideal character of the quest for scientific validation, we should not expect philosophers of science and theorists of research methodology, either, to come up with safe and sufficient guidelines, not any more than practicing researchers.

As far as the problem of ensuring high-quality observations is concerned, the basis for such guidelines would have to be some sort of a practicable *correspondence theory of truth*. Such a theory would have to explain how we can establish a "true" relationship – a stable kind of "correspondence" – between statements of fact and reality. But then, "reality" is not accessible except through the statements of observers who, apart from being human and thus imperfect observers, construct it dependent on their particular views and interests and corresponding boundaries of concern (i.e., boundary judgments). It is thus clear that such a theory is basically impossible. The ideal – if indeed it is a proper ideal – will remain just that, a mere ideal.

Similarly, with regard to the problem of securing compelling argumentation, the necessary basis would consist in a practicable theory of "rationally" argued consensus. Such a *discourse theory of rationality* would have to explain how a consensus can be shown to be justified rather than merely factual, that is, what kind of arguments are necessary to support it and what conditions could ideally warrant these arguments. But as we have learned from Habermas' (1979) analysis of the "ideal speech situation," such a theory cannot make those ideal conditions real. This is a topic that I consider essential for developing our contemporary concept of science so as to meet the challenge of the user, but it leads far beyond the scope of the present, introductory essay. Interested readers can find some of my efforts to come to terms with the difficult path to communicative rationality elsewhere (e.g., Ulrich, 1983, Ch. 2; 2009a, b; 2013a)

Insofar as the methods of natural science appear to provide a proven tool for ensuring scientific progress, many natural scientists may disregard the lack of philosophical grounding without worrying too much. The social sciences and the applied disciplines are in a less comfortable position, however. The way they deal with these issues is bound to affect the findings and conclusions they will be able to establish. Applied researchers need to be especially careful as to what their quest for competence means and in what respects it can or cannot be grounded epistemologically and methodologically. As students of the applied disciplines, how can you square the circle and hope to become a competent researcher or professional *despite* the lack of sufficient epistemological and methodological guidelines?

Methodological pluralism The unavailability of a satisfactory answer is probably responsible for the postmodern rise of *pluralism* in epistemological and methodological thought, sometimes also called "methodological complementarism." Since there is no single theoretical and methodological framework that would be best for all research tasks and circumstances, so goes the reasoning, the value of research depends on careful choice and combination of methodologies and conforming methods. Accordingly, meta-level frameworks for selecting proper research approaches need to be developed to support sound practice. In the management sciences, for

example, this so-called "methodology choice" approach has been heralded particularly in the writings of Jackson (e.g., 1987, 1990, 1991, 1997, 1999), Midgley (e.g., 1992, 1997), and Mingers (Mingers and Brocklesby, 1996; Mingers and Gill, 1997). In different ways and partly critical of this meta-level approach, which raises unsolved theoretical problems of its own, methodological pluralism or complementarism also informs the work of other authors in the field, including my own work (e.g., Linstone, 1984 and 1989; Oliga, 1988; Ormerod, 1997; Ulrich, 1988, 2003; 2012c, d, e, 2013b, 2017; White and Taket, 1997).

But of course, the call for epistemological and methodological pluralism, justified as it is by the lack of satisfactory theories of knowledge and of rationality, merely makes a virtue of necessity. It cannot conceal the fact that if by "competent" research we mean a form of inquiry that would give us sufficient reasons to claim the validity of our findings and conclusions, the quest for competence in research remains chimerical. The methodology choice approach, as we already found above in a different context (discussing the mistaken idea that theoretically grounded methods can justify practice), just doesn't carry far enough.

The ongoing quest for good practice For a tenable practice of research, we still need additional guidelines. Two sources of guidelines have become particularly important for my understanding of competence in research:

(a) *Rethinking the relationship of theory and practice*: Instead of seeking a basis for claims to knowledge and rationality in the scientific qualities of research alone, we might be better advised to seek to base them in a proper integration of research and practice. The issue that comes up here is the precise *model for mediating theory and practice*, or science and politics, that should underpin our understanding of competence in applied research.

(b) *The critical turn of practice*: Instead of seeking to validate claims to knowledge and rationality positively, in the sense of ultimately sufficient justification, we might be better advised to defend them critically only, by renouncing the quest for sufficient justification in favor of the more realistic quest for a sufficient critique, that is, for a systematic effort of laying open justification deficits. The issue here is what in my writings I describe as the

"critical turn" (or, in some more specific contexts, also as the "critically-heuristic," "critically-discursive," and "critically-normative" turns) of our notions of competence, knowledge, science, rationality, good practice, and so on, and as the consequent quest for an at least *critical solution* to the problem of practical reason, along with the critical significance of the systems idea for such a solution.⁹⁾

Mediating theory and practice Ever since the rise of science, there has been a hope that political practice, that is, the use of power, could be enlightened by science. At the bottom of this issue lies the question of the proper relationship between science and society, between technically exploitable knowledge and normative-practical understanding (and improvement) of the social life-world, between "theory" and "practice."¹⁰⁾

From Aristotelian praxis to decisionism Until the rise of science, Aristotle's (1981, 1985) view of rational practice (*praxis*) as a non-scientific domain that was to be grounded in the ethos of the *polis* and in the model of proper conduct or "excellence" (*arete*) provided by virtuous individuals, was generally accepted. The crucial link between reason and practice consisted for Aristotle in his belief that "we cannot be intelligent without being good" (1985, Book VI, Ch. 12). Virtues of character and of thought were the human qualities that mattered most for proper *praxis*, much more than reliance on theoretical knowledge (*theoria*) and technical skills (*poiesis*). Interestingly, these virtues were not simply given to individuals but were the result of hard work and of a persistent, life-long quest for excellence or, as we say in this essay, for competence. Modern as this Aristotelian concept is, there is a basic difference to the quest for competence that inspires the present essay: Aristotle saw the task and virtue of excellence (or competence) in its supporting the traditioned, conventional way of life of the community and thus would hardly have expected it to pursue a critical or even emancipatory intent along the lines of "boundary critique."

For some one and a half millennia after Aristotle, this conventionalistic, but ethically grounded notion of rational practice prevailed. The alternative idea of grounding it in science and research did not arise before the modern age. It was the English political philosopher Thomas Hobbes (1588-1679) who in

the middle of the seventeenth century proposed a first design for the scientization of politics. His insight was that practical issues raise questions that are scientifically accessible insofar as they require theoretical or technical knowledge. Once these theoretical or technical questions had been identified, the remaining questions would then properly remain inaccessible to science as they required genuinely normative, subjective *decisions* that lie beyond rationalization through theory or technique. Thus *decisionism* was born, the doctrine that practical questions allow of scientific rationalization as far as they involve the choice of means; for the rest, they can only be settled through the (legitimate) use of power. *Auctoritas, non veritas, facit legem*, became Hobbes' motto: "Power rather than truth makes the law." The limited function of science, then, consists in informing those in a situation of (legitimate) power about the proper choice of means for their ends, according to the guideline: "Knowledge serves power."

From the decisionistic to the technocratic model For the Enlightenment thinkers, this could not be the last word on the matter. *Veritas, non auctoritas, facit legem*, that is, "Truth rather than power makes the law," was postulated by the French Enlightenment philosopher Jean-Jacques Rousseau (1712-1778) as a counterpoint against Hobbes. It was to take nearly two centuries for Rousseau's postulate to acquire some empirical content (descriptive validity) in addition to its normative content. The growth of administrative and scientific tools for rationalizing decisions, exemplified by the development of computers, decision theory, and systems analysis in the middle of the twentieth century, led to a partial reversal of the relationship between the politician and the expert or researcher: the researcher's understanding of real-world issues now increasingly tends to determine the need and criteria for political action. One need only think of environmental issues to realize how much science nowadays defines the factual constraints to which politicians must succumb.

What remains to politics, then, is paradoxically the choice of the means that are capable of responding to the needs that have been defined by the experts. As a former chief evaluator in the public sector, I have often experienced this peculiar *reversal of roles*: I was expected to come up with "scientific" findings and conclusions as to what needed to be done, so that the politician

could then justify his chosen measures (or his inactivity) by referring to the recommendations of the evaluation researcher. The danger is that the genuine function of politics, to ensure legitimate decisions on issues of collective concern, is in effect delegated to researchers who, because they hold no political mandate, are not democratically accountable.

To the extent that this reversal of roles takes place, the decisionistic model of the mediation between science and politics becomes technocratic. In the *technocratic model*, political debates and votes are ultimately replaced by the logic of facts; politics fulfils a mere stopgap function on the way towards an ever-increasing rationalization of power (Habermas 1971, p. 64). Knowledge no longer serves power, as in the decisionistic model; knowledge now *is* power.

Max Weber's solution attempt The German sociologist and philosopher Max Weber (1864-1920, see 1991) foresaw this tendency. As a bulwark against technocracy, he sought to strengthen the decisionistic model by reformulating it more rigorously. He tried to achieve this by conceiving of an *interpretive social science* that could explain (and thus rationalize) the subjective meaning of individual actions or decisions in terms of underlying motivations of people, *without* thereby presupposing value judgments of its own. Interpretive social science was to *describe* and *explain* value judgments but not to *make* or *justify* them. In this limited sense it could then support subjective decisions or actions and promote their rationality. Rather like Hobbes, Weber thus found that decisions or actions indeed admit of scientific explanation, namely, insofar as they can be shown to represent a "purpose-rational" pursuit of motivations.

At the bottom of this view is a concept that has remained very influential to this day, Weber's *means-end dichotomy*. It says that decisions on ends and the choice of means can be separated, in that the latter do not require value judgments of their own and hence are accessible to scientific support. This concept of *purposive-rationality* permits a rational choice of efficacious means; but it cannot deal with the rationality of the purposes they serve, much less ensure it. In this respect it falls back onto the very decisionism it was meant to overcome or to "rationalize."

Quite in the tradition of Hobbes, Weber in effect relegated the choice of ends once again to a domain of genuinely *irrational* – because subjective and value-laden – political and ethical "decisions." Weber was willing to pay this price since he hoped to achieve a critical purpose: lest it become technocratic, science should not misunderstand itself as a source of legitimation for value judgments on ends. That was the essential concern that his famous slogan cited above meant to capture: "Politics is out of place in the lecture-room." (Weber, 1991, p. 145).

The problem with this self-restriction of science is not only that the question of proper ends remains unanswered – the effectiveness and efficiency of means, when used for the wrong ends, brings about not more but less rational practice. The problem is also, and more fundamentally, that it does not achieve its critical intent, as self-restriction to questions of means does not in fact keep research free of value implications. The reason is that alternative means to reach a given end may have different practical implications for those affected by the measures taken. For example, alternative proposals for radioactive waste disposal may impose different risks and costs on different population groups, including future generations. That is to say, decisions about means, just like decisions about ends, have a value content that is in need of both ethical reflection and democratic legitimation. Whether or not their claim to purposive-rationality is backed by science makes little difference in this regard.

Weber's conception of a value-free, interpretive social science breaks down as soon as one admits this implication. Once this is clearly understood, it seems almost unbelievable how uncritically a majority of contemporary social and applied scientists still adhere to the dogma that means and ends are substantially distinct categories, so that only decisions on "ends" are supposed to involve value judgments while the choice of "means" is understood to be value-neutral with regard to given ends, that is, to be the legitimate business of science (cf. Ulrich, 1983, p. 72).

The pragmatist model of Jurgen Habermas In order to overcome the shortcomings of both the decisionistic and the technocratic models of relating theory and practice, we need another model. Such a model will have

to replace the faulty means-end dichotomy by a *fundamentally complementary* understanding of means and ends, just as of theory and practice (cf. Ulrich, 1983, pp. 222 and 274; 1988, pp. 146-149; 1993, p. 590; 2011, pp. 13-18). In this model, the selection of means and the selection of ends are not separable, for the rationality of either depends on the rationality of the other. Moreover, each decision has a value content of its own, although this value content again is not independent of the value content of the other decision. It is the merit of Jurgen Habermas (1971) to have elaborated a model that conforms to these requirements. He calls it the *pragmatist model*.

In the pragmatist model, neither politicians nor researchers possess an exclusive domain of genuine competence, nor can either side dominate the other. Caught in an intricate "dialectic of potential and will" (Habermas, 1971, p. 61), they depend on each other for the selection of both means and ends. The strict separation between their functions is replaced by a critical interaction, and the medium for this interaction is *discourse*. Its task is to guarantee not only an adequate translation of practical needs into technical questions, but also of technical answers into practical decisions (cf. Habermas, 1971, p. 70f).

In order to achieve this double task, the discourse between politicians and researchers must, according to Habermas (1979), be rational (or "rationally motivated") in the terms of his ideal model of rational discourse. That is, the discourse must be "undistorted" and "free from oppression." The difficulty is, once again, that we are dealing with an ideal. Even where the discourse between politicians and experts occasionally results in an undisputed consensus, how can we ever be sure that the consensus is not merely factual rather than rational? Realistically speaking, we can never be sure; for the discourse would then have to include not only the effectively involved politicians and researchers but all those who are actually or potentially concerned or affected by the decision in question, including the unborn or other parties that cannot speak for themselves. Moreover, it would have to enable all of them to play a competent role. The pragmatist model thus leads us back to the fundamental concern of critical systems heuristics, namely, that we need to develop a practicable and non-elitist "critical solution"

(rather than a complete "positive solution") to the unachievable quest for securing rational practice.

Before we turn to this idea of an at least critical solution of the problem of practical reason, let us summarize our findings with respect to a competent researcher's understanding of the relationship of theory and practice: a competent researcher will (1) examine critically the role she or he is expected to play in respect to practice; (2) analyze which model of the relation of theory and practice is factually assumed in her or his mandate, and which model might be most adequate to the specific situation at hand; and (3), where the appropriate answer appears to consist in working toward a pragmatist model, a competent researcher will seek to consider all those people actually or potentially affected and, to the extent that their actual participation is feasible, will also seek to put them in a situation of competence rather than their usual situation of (supposed) incompetence.

The Critical Turn The "critical turn" is the quintessence of much of what I have tried to say in this essay. The quest for competence in research and professional practice entails epistemological and ethical requirements that we cannot hope to satisfy completely. I am thinking particularly of requirements such as identifying all conceivable "practical implications" of a proposition; assuming proper boundary judgments; securing high-quality observations as well as compelling argumentation; dealing properly with the practical and normative (ethical, moral) dimension of our "findings and conclusions"; mediating adequately between research and practice; and facing the "challenge of the user."

In view of these and other requirements that we have briefly considered, the usual notion of competent research becomes problematic. I mean the notion that as competent researchers we ought to be able to justify our findings and conclusions in a definitive, compelling way. As an ideal, this notion of justification is certainly all right, but in practice it tempts us (or those who adopt our findings and conclusions) into raising claims to validity that no amount of research competence can possibly justify.

I suggest that we associate the quest for competence with a more credible

notion of justification. First of all, let us acknowledge openly and clearly that we cannot, as a rule, sufficiently justify the results of our research. This need not mean that we should renounce any kind of validity claims, say, regarding the quality of our observations or the rationality of our conclusions. The fact that we cannot fully justify such claims does not mean that we cannot at all distinguish between higher and lower quality, or more or less compelling argumentation. It means, rather, that the manner in which we formulate and handle validity claims will have to change. We must henceforth qualify such claims very carefully, by explaining to what extent and how exactly they depend on assumptions or may have implications that we cannot fully justify as researchers, but can only submit to all those concerned for critical consideration, discussion, and ultimately, choice.

Towards a new ethos of justification It is the researcher's responsibility, then, to make sure that the necessary processes of debate and choice can be handled by the people concerned in as competent a way as possible. To this end, a competent researcher will strive to give those concerned all the relevant information as to how her or his findings came about and what they may mean to different parties. Moreover, it becomes a hallmark of competence for the researcher to undertake every conceivable effort to put those concerned in a situation of meaningful critical participation rather than of incompetence.

This is the basic credo of the critical turn that I advocate in our understanding of research competence. It amounts to what elsewhere (Ulrich, e.g., 1984, pp. 326-328, and 1993, p. 587) I have called a "new ethos of justification," namely, the idea that *the rationality of applied inquiry and design is to be measured not by the (impossible) avoidance of justification deficits but by the degree to which it deals with such deficits in a transparent, self-critical, and self-limiting way.*

Since in any case we cannot avoid justification deficits, we should seek to understand competence as an effort to deal self-critically with the *limitations* of our competence, rather than trying to avoid or even conceal them. The critical turn demands from researchers a constant effort to be "on the safe side" of what they can assume and claim in a critically tenable way. It

demands a Socratic sense of modesty and self-limitation even where others may be willing to grant the researcher the role of expert or guarantor. Once you have grasped this meaning of the critical turn, it will become an irreversible personal commitment. Kant, the father of Critical Philosophy, said it well:

This much is certain, that whoever has once tasted critique will be ever after disgusted with all dogmatic twaddle. (Kant, 1783, p. 190).

I invite you to "taste critique" and to give it a firmly established place in your notion of competence!

Tasting critique As students of systemic research and practice, you might begin this critical effort by understanding the *systems idea* critically, that is, using it as a tool of reflective research and practice rather than a basis for claiming any kind of special rationality and expertise (e.g., in handling tasks of systems analysis, design, and management, or any kind of professional intervention with a systemic outlook). Thus understood, the critical turn will change the way in which we employ systems concepts and methodologies and in fact, any other methodologies. Rather than understanding systems thinking as a ground for raising claims to rationality and expertise, or even some kind of superior "systemic" rationality, we shall understand it from now on as tools for critical reflection. In other words, we will use it more for the purpose of finding questions than for finding answers.

A crucial idea that can drive the process of questioning is that of a systematic unfolding of both the empirical and the normative selectivity of (alternative sets of) boundary judgments, that is, of how the "facts" and "values" we recognize change when we alter the considered system (or situation) of concern. I have referred to this process earlier in this paper as a process of *systematic boundary critique*.

Two core concepts of boundary critique that I have often used to explain the idea are the "eternal triangle" of observations, valuations, and boundary judgments, and the related concept of a "systemic triangulation" of our findings and conclusions (or related claims). Interested readers will find two introductory essays that are written for a wide audience of researchers, professional people, decision-makers, and citizens in Ulrich (2000 and

2017).

A third key concept of boundary critique that I would like to mention here concerns the way boundary critique can help promote a better "symmetry of critical competence" among people with different backgrounds and concerns – those who in a project have the say and those who don't; those involved and those affected but not involved; experts and non-experts; professionals and the citizens they are supposed to serve. The basic point should by now be clear: whatever skills in the use of research methods, theoretical knowledge, and professional experience or any other kind of expertise a researcher may possess – when it comes to boundary judgments, researchers are in no better position than other people. Whoever claims the (objective) validity of some findings or the (superior) rationality of the conclusions derived from them *without* at the same time explaining the specific boundary judgments on which these claims depend, can thus be shown to be arguing on slippery grounds.

Boundary critique for citizens Based on this concept of a fundamental symmetry of competence in regard to boundary judgments, boundary critique can also serve as a restraint upon unwarranted claims on the part of researchers or other people who do *not* employ systems concepts and methodologies (or any other methodologies) as self-critically as we might wish. If reflective research practice is not to remain dependent solely on the good will of researchers and professionals, it is indeed important that other people can challenge their findings and conclusions by making visible the boundary judgments on which they rely. See Ulrich (1993) for a fuller account of this important implication of boundary critique. Readers will also find this tool described in my writings in terms of an "emancipatory employment of boundary judgments" or shorter, of "emancipatory boundary critique" (e.g., 1996a, 1987, 2000, 2003).

I believe that ordinary people, provided they receive an adequate introduction to the idea of boundary judgments, can understand the conditioned nature of all findings and conclusions and can then also learn to challenge unwarranted claims on the part of experts in an effective way, without depending on any special expert knowledge that would not be

available to them. No special expertise is required because no positive claims to validity are involved; it is quite sufficient for such critical argumentation to show that a claim relies on some crucial boundary judgments (say, as to what "improvement" means and for whom it should be achieved) that has not been laid open and for which there are *options*.

The employment of boundary judgments for merely critical purposes has this extraordinary power because it is a perfectly rational form of argumentation: its relevance and validity cannot be disputed simply by accusing the critic of lacking expert knowledge. For this reason I am convinced that it is able to give not only researchers and professionals but also ordinary citizens a *new sense of competence*. I have explained this emancipatory significance of the concept of boundary judgments elsewhere in more detail and in various terms, partly also in terms of Kant's (1787) fundamental concept of the "polemical" employment of reason (see, e.g., Ulrich, 1983, pp. 301-314; 1984, pp. 341-345; 1987, p. 281f; 1993, pp. 599-605; 1996a, pp. 41f; 2000, pp. 257-260; and 2003, pp. 329-339). But as I just hinted, you do not need to become an expert of CSH to understand and practice the idea of boundary critique.

Conclusion At the outset I proposed that to "understand" means to be able to formulate a question. I suggested that in order to become a competent researcher, it might be a good idea for you to reflect on the fundamental question to which your personal quest for competence should respond.

I hope I have made it sufficiently clear in this paper that you will have to find this question yourself. Nobody else can do it for you. In order to assist you in this endeavor, I have tried to offer a few topics for reflection. There are, of course, many other topics you might consider as well; my choice may perhaps serve as a starting point for finding other issues you find important for developing your notion of competence.

I also proposed at the outset that for some of you, systems thinking might be part of the answer. But should it? Well, I am inclined to say, it depends: if you are ready to take the critical turn and to question the ways in which systems thinking can increase your competence, then it might indeed become

a meaningful part of your personal understanding of competence. By reflecting on what might be the fundamental question to which a *critical systems perspective* gives part of the answer, you might begin to understand more clearly what exactly you expect to learn from studying systems thinking and how this should contribute to your personal quest for competence.

I did not promise you that it would be easy to formulate this fundamental question. It may well be that only by hindsight, towards the end of your professional life, you will really be able to define it. In the meantime, it will be necessary to rely on some tentative formulations, and more importantly, to *keep searching*. Only if your mind keeps searching for the one meaningful question can you hope to recognize it when you encounter it. Sooner or later you will find at least a preliminary formulation that proves meaningful to you.

My basic question (an example) Perhaps you wish you had an example. Should I share my tentative question with you? At the end of this essay, I hope you are sufficiently prepared not to mistake it for your own question. I first encountered this fundamental question in the year 1976, when I moved to the University of California at Berkeley (UCB) to study with West Churchman, who had helped to pioneer the fields of operations research and management science in the 1950s and then, since the 1960s, became a pioneer and leading philosopher of the systems approach. Churchman used to begin his seminars with a question! He then asked his students to explore the meaning of that question with him, and that's what I have kept doing ever since. This is what Churchman wrote up on the blackboard:

*Can we secure improvement
in the human condition
by means of the human intellect?*

For Churchman, each one of the underlined key expressions in the question – "secure," "improvement," "human condition," and "human intellect" – pointed to the need for a *holistic* understanding of the systems approach. We cannot hope to achieve their fulfillment without a sincere quest for "sweeping in" (Singer, 1957; Churchman, 1982, pp. 117, 125-133; cf. my

appreciations in Ulrich, 1994 and 2004, pp. 1126-1128) all aspects of an issue that might conceivably be relevant, that is, ideally, for "understanding the whole system" (Churchman, 1968, p. 3). Churchman's life-long quest to understand the question thus led him to see the systems approach as a *heroic* effort. A systems researcher or planner who is determined to live up to the implications of the question is bound to become a hero!

My own endeavor to come to terms with the question was a little less heroic. For me, each of the question's key expressions points to the need for a *critical turn* of the systems approach. We cannot hope to do justice to them without a persistent, self-reflective effort to consider the ways in which we *fail* to be sufficiently holistic. Since boundary judgments are always in play, all our attempts to secure knowledge, understanding, and improvement are bound to be selective rather than comprehensive. We must, then, replace the quest for comprehensiveness with a more modest, but practicable, quest for *boundary critique*. This is why in my work on CSH, the principle of boundary critique had to replace the sweep-in principle as a methodological core concept of competent research and practice (Ulrich, 2004, p. 1128).

At least in hindsight, Churchman's question makes it easier for me to understand why I had to struggle so much to clarify my understanding of the systems idea, and why I ended up with something like critical systems heuristics and its central concept of boundary critique. It is because I tried, and still try, to understand systemic research and practice so that it responds to that fundamental question. There is no definitive answer to the question, of course; but that surely does not dispense me (or us, inasmuch as you agree) from struggling to gain at least some critical competence in dealing with it.

I wish you success in your quest for competence.

Notes

- 1) The British philosopher, historian, and archaeologist R.G. Collingwood (1939/1983, 1946) was perhaps the first author to systematically discuss the *logic of question and answer* as a way to understand the meaning of everyday or scientific propositions. As he explains in his *Autobiography* (1939):

I began by observing that you cannot find out what a man means by studying his spoken or written *statements*, even though he has spoken or written with perfect command of language and perfectly truthful intention. In order to find out his meaning you must also know what the *question* was (a question in his mind, and presumed by him to be in yours)

to which the thing he has said or written was meant as an answer.

It must be understood that question and answer, as I conceived them, are strictly correlative.... [But then,] if you cannot tell what a proposition means unless you know what question it is meant to answer, you will mistake its meaning if you make a mistake about that question.... [And further,] If the *meaning* of a proposition is correlative to the question it answers, its *truth* must be relative to the same thing. Meaning, agreement and contradiction, truth and falsehood, none of these belonged to propositions in their own right, propositions by themselves; they belonged only to *propositions as the answers to questions*. (Collingwood, 1939/1978, pp. 31 and 33, italics added)

While remaining rather neglected in fields such as science theory and propositional logic, it was in the philosophy of history (the main focus of Collingwood, esp. 1946), along with hermeneutics (Gadamer, 2004), and argumentation theory (Toulmin, 1978, 2003) that Collingwood's notion of the logic of question and answer was to become influential. In hermeneutic terms, the questions asked are an essential part of the *hermeneutical horizon* that shapes what we see as possible answers and what meaning and validity we ascribe to them. In his seminal work on hermeneutics, *Truth and Method*, Gadamer (2004) notes:

Interpretation always involves a relation to the question that is asked of the interpreter.... To understand a text means to understand this question.... We understand the sense of the text only by acquiring the horizon of the question – a horizon that, as such, necessarily includes other possible answers. Thus the meaning of a sentence ... necessarily exceeds what is said in it. As these considerations show, then, the logic of the human sciences is a logic of the question.

Despite Plato we are not very ready for such a logic. Almost the only person I find a link with here is R.G. Collingwood. In a brilliant and telling critique of the Oxford "realist" school, he developed the idea of a logic of question and answer, but unfortunately never elaborated it systematically. He clearly saw that ... we can understand a text only when we have understood the question to which it is an answer. (Gadamer, 2004, p. 363) [\[BACK\]](#)

- 2) As I found out after writing the original working paper (Ulrich, 1998a), the phrase "death of the expert" is not mine. White and Taket (1994) had used it before. By the time I prepared the expanded version of the essay for *Systems Research and Behavioral Science* (Ulrich, 2001a), I had become aware of their earlier use of the phrase and accordingly gave a reference to it. My discussion here remains independent of theirs, but I recommend readers to consult their different considerations as well. [\[BACK\]](#)
- 3) We'll return to this issue under the heading of "methodological pluralism" below. For a systematic account and critique of the identification of critical practice with methodology choice in this strand of critical systems thinking (CST), see Ulrich (2003) and the ensuing discussions in several subsequent "Viewpoint" sections of the journal. Readers not familiar with CST may find Ulrich (2012e or 2013b) useful preparatory reading. [\[BACK\]](#)
- 4) I have given an extensive critical account of Weber's notion of "value-free" interpretive social science and his underlying conception of rationality elsewhere, see Ulrich (2012b). We will return to Weber's "interpretive social science" in the section on theory and practice below. [\[BACK\]](#)
- 5) I use the term "boundary critique" as a convenient short label for the underlying, more accurate concept of a "critical employment of boundary judgments." The latter is more accurate in that it explicitly covers two very different yet complementary forms of "dealing critically with boundary judgments." It can be read as intending both a self-reflective handling of boundary judgments (being critical of one's own boundary assumptions) *and* the use of boundary judgments for critical purposes against arguments that do not lay open the boundary judgments that inform them (arguing critically against hidden or dogmatically imposed boundary assumptions). By contrast, the term "boundary critique" suggests active criticism of other positions and thus, as I originally feared, might be understood only or mainly or only in the second sense. While this second sense is very important to me, the first sense is methodologically more basic and must not be lost. I would thus like to make it very clear that I always intend both meanings, regardless of whether I use the original full concept or the later short term.

Terms do not matter so much and represent no academic achievement by themselves, only the concepts or ideas for which they stand do and these should accordingly be clear. The concept of a critical employment of boundary judgments in its mentioned, double meaning

embodies the methodological core principle of my work on critical systems heuristics (CSH) and accordingly can be found in all my writings on CSH from the outset (e.g., Ulrich, 1983, 1984, 1987, 1988, 1993, etc.). Only later, beginning in 1995, I have introduced the short label "boundary critique" (see, e.g., Ulrich, 1995, pp. 13, 16-18, 21; 1996a, pp. 46, 50, 52; 1996b, pp. 171, 173, 175f; 1998b, p. 7; 2000, pp. 254-266; and 2001, pp. 8, 12, 14f, 18-20, 24). Meanwhile I have increasingly come to find it a very convenient label indeed, *so long* as it is clear that both meanings are meant (and in this sense I use it as a rule). Accordingly I am now employing it regularly and systematically (cf., e.g., Ulrich, 2002, 2003, 2005, 2006a, 2012 b, c, d; 2013b; and most recently, 2017). [\[BACK\]](#)

- 6) The boundary questions presented here are formulated so that the second part of each question defines the boundary category at issue. For introductions of varying depth and detail to the boundary categories and questions of CSH, see, e.g., Ulrich, 1983, esp. pp. 240-264; 1984, pp. 333-344; 1987, p. 279f; 1993, pp. 594-599; 1996a, pp. 19-31, 43f; 2000, pp. 250-264; 2001a, pp. 250-264; and 2001b, pp. 91-102. On CSH in general, as well as the way it informs my two research programs on "critical systems thinking (CST) for citizens" and on "critical pragmatism," also consult: Ulrich 1988, 2000, 2003, 2006a, b, 2007a, b, 2012b, c, d, 2013b, and 2017, and Ulrich and Reynolds, 2010. [\[BACK\]](#)
- 7) I should note that strictly speaking, observer-independence does not imply that objects are non-constructed; it only implies transferability in the sense of the above-mentioned requirement of conventional high-quality observations. I understand de Zeeuw's language as referring to ideal types of "objects" only, ideal types that may help us understand the historical and present development of science but which do not necessarily exist as such in the actual practice of science. Nor would I equate them with philosophically unproblematic notions of scientific objects. The notion of "non-constructed objects" in particular appears to be tenable only within a philosophically uncritical realism or empiricism. On more critical grounds, it would appear that all objects are constructed; indeed, even the celestial bodies of astronomy are constructed as "stars," "moons," "solar systems," "constellations," "comets," etc., before they are conceptually subsumed under one or several classes of celestial objects. Taking the example of "comets," they were not always construed as celestial bodies but earlier were seen as phenomena of the atmosphere, a change of conception that betrays the constructive side of objects. [\[BACK\]](#)
- 8) I have discussed de Zeeuw's ideas and the way I relate them to my work on CSH in a bit more detail in Ulrich, 2012a. Basically, I see in the two frameworks two complementary approaches to the need for extending and developing the contemporary concepts of science and research practice. [\[BACK\]](#)
- 9) The concepts of a "critical turn" of our understanding of competence, professionalism, rationality, and so on, and, related to it, of securing at least a "critical solution" to the problems of reason (particularly the unresolved problem of practical reason and the impossible quest for comprehensiveness or "systems rationality") are as fundamental to my work on critical heuristics, critical systems thinking for citizens, reflective practice, and critical pragmatism as is the concept of boundary critique (cf. note 5 above). See, e.g., Ulrich, 1983, pp. 20f, 36, 153-157, 176f, 222-225, 265f and passim; 1993, p. 587; 1996, p. 11f; 2001, pp. 8, 11, 14f, 20, 22-25; 2003, p. 326f; 2006a, pp. 53, 57, 70f, and 73-80; 2007a, pp. 1112, 1114; 2012c, pp. 1237, 1244; and 2012d, pp. 1313-1316, 1318, 1320). [\[BACK\]](#)
- 10) The following short account of the history of thought on the mediation of theory and practice (or science and politics) is based on my earlier, more substantial discussion of "The Rise of Decisionism" in *Critical Heuristics* (Ulrich, 1983, pp. 67-79). Readers who wish I had provided a more detailed account here in the present essay should consult that earlier text. In addition, a classical essay on the topic that I recommend, and which has strongly influenced by thinking on the matter, is "The scientization of politics and public opinion" by Jurgen Habermas in his *Toward a Rational Society* (see Habermas, 1971, pp. 62-80). [\[BACK\]](#)

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Note: The number of references listed in this bibliography to my own writings may suggest a lack of modesty that is not intended. It is motivated by the circumstance that this essay is of a didactic nature and that my teaching has always been based mainly on my own

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writings, as these are the ideas I can convey to the students in the most authentic and reflecting ways. As I hope the essay shows, no disregard for the ideas of other writers is intended; quite the contrary, it seeks to document its sources by providing the most relevant and accurate references of which I am aware, sources that have influenced me but also have been playing a seminal role in the history of ideas concerned.

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Picture data Digital photograph taken on 8 August 2010, around 4 p.m., in the author's garden. ISO 200, exposure mode aperture priority with aperture f/7.1 and exposure time 1/25 seconds, exposure bias -0.70. Metering mode center-weighted average, contrast soft, saturation high, sharpness low. Focal length 102 mm (equivalent to 204 mm with a conventional 35 mm camera). Original resolution 3648 x 2736 pixels; current resolution 700 x 525 pixels, compressed to 236 KB.

May-June, 2017



Competent practice involves a proper handling of boundary judgments

„**A**ppplied researchers need to be especially careful
as to what their quest for competence means.”

(From this essay on the nature of systemic research and practice)

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