If Systems Thinking is the Answer, What is the Question?

The Quest for Competence in Systemic Research and Practice

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Abstract

This working paper originates in a seminar with research students and staff of the Lincoln School of Management in Lincoln, UK, on the topic of how we can develop competence in (systemic) research. The seminar was held to guide the students towards reflection on their personal notion of competence, both in their research and in their (future) professional practice. With the present paper, the author hopes to offer some help to readers who seek orientation in formulating or advancing their dissertation project, or who wish to clarify their personal notion of professional competence.

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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. 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?

I think it is indeed important for you to ask yourself this question. To understand the kind of competence you are aiming at is vital for you as a research student. 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. You may want to deepen your knowledge and expertise as a (future) professional, e.g. by acquiring some of the specific skills and competencies that you expect from systems thinking. Or you feel a need to strengthen your capabilities in general rather than simply as an expert. 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. I cannot give you the answer, and the purpose of this working paper is not to give you any answer. The purpose is, rather, to help you find *your own* individual answer. Thus I want to guide you toward a few possible topics for reflection, towards meaningful questions to ask yourself. As far as the paper also offers some considerations as to how you might deal with these topics, please bear in mind that my purpose is merely to turn your attention to some questions that you might find relevant but not to give you *the* answers; that is to say, I do not claim that the considerations I offer are the only possible ones or even the only valid ones. 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 all different persons and will therefore have to pursue your quest for competence in your own unique way.

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As a last preliminary remark, since nobody has a monopoly or a natural advantage in knowing what the right answers are for you, you should feel free to engage in this process of reflection and to share your thoughts and feelings with others, so as to clarify them further. Everybody is entitled to have differing views on what the quest for competence means. Nobody is entitled to prove you wrong. 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 research students 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 good 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 they are valid.

The job of 'becoming a competent researcher' is likely to sound like a tall order to you.

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 choice. 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 systems 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 each other.

As a second example, 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 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, e.g. by people in an organisation whose problems you study, so that they accept your findings or conclusions and implement them in practice?

As a fourth and final example, 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.

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 worries as a good sign that you are on your way toward 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 Expert

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 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, what role would you rather not play as a competent (systems) researcher? In what way would you rather not claim expertise, i.e., limit your claims to expertise? Where do you see dangers of ceasing to be self-critical?

In what way do you aim to be an 'expert' of systems methodologies, and in what way do you not?

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! Basically, we do not want to ignore ('forget') or even hide (know but not make explicit) the *limitations of our methods* — 'methods' in the widest possible sense of any systematically considered way to proceed — on which our competence depends.

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. From a critical point of view, no human method should ever be assumed to be sufficient for dealing with all aspects of a problem; only gods (perhaps) know omnipotent methods. 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 depend on the specific method we use, in the sense of being built into that method; others may arise rather through the (imperfect) way we use it or the (inappropriate) purpose for which we use it.

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Perhaps an even more basic assumption is that the expert, by virtue of his expertise, has a proper grasp of the situation to which he wants to apply his expertise, so that he can properly decide what method is appropriate and this choice can then ensure valid findings and conclusions. Experts often seem to take such assumptions for granted, or else tend to cover them behind a façade of busy routine.

To the extent that we 'forget' 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 his routine which stops him from revealing to himself and to other concerned persons the specific assumptions that flow into every concrete application of systems ideas. Even so, this is probably not what you would like to understand by 'competence'.

The earlier-mentioned questions and doubts that plague many a Ph.D. student are then perhaps a healthy symptom that your research competencies have not yet reached the stage of routine where this lack of reflection threatens. This danger is more of a threat to established researchers who have already become recognised 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 unrecognised or incorrectly diagnosed, perhaps because it causes an almost invisible death, one that often enough is hidden by the vigorous and impressive behaviour patterns of those who have developed the disease.

Routine implies economy, not competence. When we forget this distinction, the death of the expert threatens. In our contemporary society, the death of the expert has taken on epidemic dimensions!

But 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 underpinning of his or her competence and makes an appropriate effort to make it 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.

When it comes to that sort of issue, the researcher has no advantage over other people. In that case, competence in research gains another meaning, namely, that of the *self-limitation of the researcher*. 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.

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 <u>not</u> answer.

The number of questions that may be asked is, of course, infinite. 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.

Competence depends more on the questions we ask than on the answers we find.

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 usually severely constrained. It usually stops as soon as answers are found that serve the given purpose. As a competent researcher you must seek to put more emphasis on the limitations of the answers and less on limiting the questions. As a competent researcher, you will want to shift the main focus of self-limitation from the questions to the answers.

Your tentative first definition of competency in research, then, might be something like this (modify as necessary): competence in research means pursuing a *self-reflective, self-correcting,* and *self-limiting* approach to inquiry. This means that I seek to question my inquiry in respect of all conceivable sources of possible deception, e.g., its (my) presuppositions, its (my) procedures, its (my) findings and the way I translate them into practical recommendations. (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.)

A major implication of this preliminary definition is the following. Competence in research means 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 — in the double sense of 'understanding' *and* 'respecting' — of the built-in limitations of the chosen research approach, both in principle and in the specific situation of interest; and

(b) even more importantly and more radically, that we 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 practical implications of the *selectivity* of those findings, which is the inescapable consequence of the limitations of the methods chosen (which is not to say that there are no other sources of selectivity).

This is bad news, I fear, for some of you who base their search for competence on the idea of a theoretically based choice among (systems) methodologies. To be sure, there is nothing wrong with this idea — so long as you do not expect it to ensure critical inquiry. I know that this notion of securing critical systems inquiry through theoretically based methodology choice is currently prominent in systems research, but I invite you to adopt it with caution. It does not carry far enough.

The question then is, 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'; and
- recognising 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 science and politics; and finally, the 'critical turn' that is at the core of my work on Critical Systems Heuristics (CSH).

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 usually, 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 (organisational, 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 organisations belong to the intended 'client' (beneficiary) of a research project, and what other people or organisations might effectively 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; that is, when we seek to identify the people or organisations 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 organisations constitute the domain of practice that I will consider as relevant for understanding the meaning of 'improvement'.

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, i.e., answer the right questions, and valid, i.e., give the right answers.

But how can we, as researchers, claim to 'know' (that is, 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'? 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' (validity and relevance) 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.

The competence of a research expresses itself not by its sheer beauty but by its value to the practice it is to support.

If you have experienced blockages in advancing your project, e.g. 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.

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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 other concerns, 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:

$$\mathbf{P = f\,(R)}$$

Proper 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 good practice is? 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:

$$\mathbf{R = f\,(P)}$$

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. *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 himself or herself and to all those concerned, what might be the practical implications of this research, i.e., what kind of practice the research is likely, or might help, 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 legitimising 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!

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In competent research, the choice of research methods and standards is secondary, i.e., 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, not the other way round.

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 quite analogous to Kant's (1787) postulate of the '*primacy of practice',* by which he meant that practical (ethical) reasoning is more important than theoretical-instrumental reasoning; for practical reasoning leads us beyond the limitations of theoretical knowledge. I would therefore like to think of our conclusion in terms of a *primacy of practice in research.*

This stipulation seems aptly to remind us that the concept of competent research which I suggest here is based on Kant's two-dimensional concept of reason. This distinguishes it from the concept of competent research that is implicit in contemporary science-theory or theory of knowledge, which unfortunately has lost sight of the indispensable normative dimension of rationality. I will deal a little more with this fundamental issue under the next heading; for a more complete discussion, see Ulrich, 1983, 1988a, and 1994).

Competent research is a function of the practice to be achieved.... Let your concern for good research follow your concern for understanding the meaning of good practice.

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 (see, e.g., Ulrich and Binder, 1992 and 1998). Poverty researchers are often expected to tell politicians how much poverty there is in a certain population and what can be done about it. But the measurement of poverty is not possible unless there are clear criteria of what standards of participation in society (both material and immaterial) are to be considered 'normal' and hence to be promoted, if not assured for 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 which 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.'

The pragmatic maxim thus 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 becomes an important virtue of competent inquiry and design in general, and of critical systems thinking in particular.

The pragmatic maxim for me represents a critical concept.

The critical kernel of the pragmatic maxim as I understand it is this. Identifying the implications of a proposition is not a straightforward task of observation 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 quest for comprehensiveness is reserved to heroes and gods; it is beyond the reach of ordinary researchers. What we ordinary researchers recognise as relevant implications depends on *boundary judgements* by which we consciously or unconsciously delimit the situation of concern. 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 may fail to be comprehensive, by undertaking a systematic critical effort to disclose those boundary judgements.

Systematic Boundary Critique

In *Critical Heuristics* (Ulrich, 1983, see esp. Chapter 5), I have conceived of this critical effort as a process of systematic *boundary critique*, i.e., a methodical process of reviewing boundary judgements so that their selectivity and changeability become visible.

Table 1 shows a list of boundary questions that can be used for this purpose; you'll find a more complete account of the underlying conceptual elsewhere (see Ulrich, 1983, pp. 240-264; 1987, p. 279f; 1993, pp. 594-599; 1996a, pp. 19-31 and 43f).

Table 1: Critically-heuristic boundary questions for reviewing the situation of concern

(1)	Who is (ought to be) the ***client*** of the inquiry into, or design of, the situation of concern? That is, whose interests are (ought to be) served?
(2)	What is (ought to be) the ***purpose***? That is, what are (ought to be) the consequences of the inquiry or design?
(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?
(4)	Who is (ought to be) the ***decision maker****?* That is, who is (ought to 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 are (should be) under his control?
(6)	What conditions are (ought to be) part of the ***environment****?* That is, what conditions does (should) the decision maker *not* control (e.g., from the viewpoint of those not involved)?
(7)	Who is (ought to be) involved as ***researcher / designer****?*
(8)	What ***expertise*** is (ought to be) brought in? That is, who is (should be) considered an expert and what is (should be) his role?
(9)	Who or what is (ought to be) assumed to be the ***guarantor****?* That is, where do (should) those involved seek some guarantee that their findings or proposals will be implemented and will secure improvement?
(10)	Who is (ought to be) ***witness*** to the interests of those affected but not involved in the inquiry or design process? That is, who argues (should argue) the case of those who cannot speak for themselves but may be concerned, including the handicapped, the unborn, and non-human nature?
(11)	To what extent and in what way are those affected given (ought they be given) the chance of ***emancipation*** from the premises and promises of those involved? That is, how does the inquiry or design treat those who may be affected or concerned but who cannot argue their interests?
(12)	What ***world view*** actually underlies (ought to underly) the inquiry or design? That is, what are (should be) the different visions of 'improvement' among both those involved and those affected, and how does (should) the inquiry or design deal with these differences?

For me this critical effort of disclosing and questioning boundary judgements 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, i.e., our '*values'.*

The question of what counts as knowledge, 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 recognised the critical significance of the concept of boundary judgements, we cannot go back to our earlier 'pre-critical' concept of competent research, e.g., in terms of empirical science. It becomes quite impossible to cling to a notion of competent research that works in only one dimension. This is so because what we recognise as 'facts' and what we recognise as 'values' become interdependent.

then, is no longer one of the quality of empirical observations and underpinning theoretical assumptions only; it now is also a question of the 'proper' bounding of the domain of observation and thus of the underpinning value judgements 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.

Once we have recognised the critical significance of the concept of boundary judgements, we cannot go back to our earlier 'pre-critical' concept of competent research

What Ought to Count as Knowledge?

Research is usually undertaken to improve knowledge. A typical dictionary definition explains that research is 'to establish facts and reach new conclusions' *(The Concise Oxford Dictionary of Current English).* This is not a bad definition. Counter to the frequent 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 statements 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 statements 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?

A particular difficulty with the two requirements is 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*. Both observational and argumentative competencies must thus go hand in hand in competent research; they are but two sides of one and the same coin. (Figure 1)

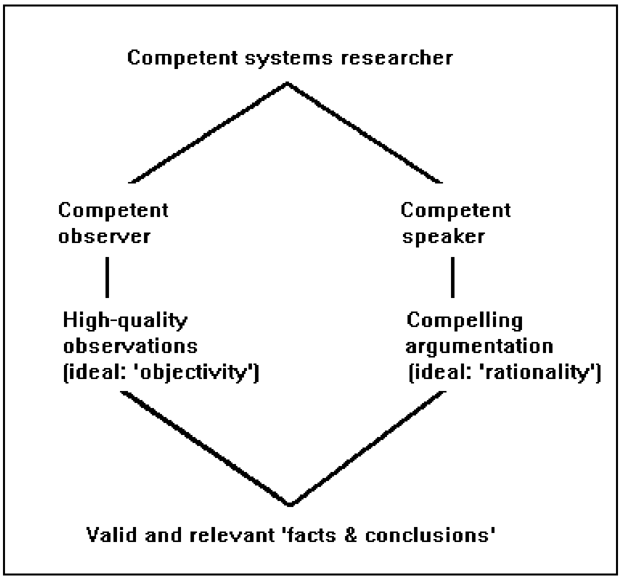

Fig. 1: Two dimensions of competence required in (systems) research: observational and argumentative competence. Each dimension entails specific validity claims, the redemption of which may, however, involve claims that refer to the other dimension.

Let us consider some of the specific requirements on each side of the coin. On the argumentative side, Habermas' (1979, pp. 2f and 63f) well-known model of rational discourse gives us a framework for analysing the difficult implications of the quest for communicative competence. 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:

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 behaviour.
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 behaviour with the expressed intentions.

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 contest the 'facts' in question of the validity of their theoretical and normative presuppositions.

Similar difficulties arise with the requirement of substantiating the 'high quality' of observations. Observations always depend on the construction of some sort of objects that can be observed and reported upon; dependent on the situation, these constructions may need to rely on different notions of what kinds of 'objects' lend themselves to quality observations.

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.

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 re-peated 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'.

Historically speaking, these or similar assumptions characterised 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 G. de Zeeuw (1996, p. 3 and p. 19f) observes, science is now more and more faced with *the challenge of the user,* that is, the task of constructing quality observations that 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 domain of therapy (e.g. psychiatry), social intervention (e.g., care for the elderly or fighting poverty), and organisational design. '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?

Science is now more and more faced with the challenge of the user, that is, the task of constructing quality observations that allow users to have a voice inside science.

De Zeeuw has discussed this issue extensively in recent publications (e.g. 1992, 1995, and esp. 1996). He distinguishes three notions of 'objects' that allow quality observations under 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)[1],

[1] 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

'*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, organisational 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, either 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' for any other reasons). The 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, the constructed objects of second-phase science distinguish themselves from the non-constructed 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 notion of competent systems research that I pursue in this Working Paper and which is also contained in my work on *Critical Heuristics* 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 agree with de Zeeuw (1996, p. 19) when he refers to Critical Systems Heuristics (CSH) as an effort to concentrate 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 Critical Heuristics 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 to acquire *a new competence in citizenship* (Ulrich, 1995, 1996a, b, 1998, 2000a, b).

I believe to have found one fundamental source of such competence in the unavoidable boundary judgements that underpin all application of research and expertise to real-world issues but in respect to which researchers or experts enjoy no advantage over ordinary citizens (Ulrich, 1983, esp. pp. 305-310; 1987, p. 281f; 1993, pp. 599-605). Once we acknowledge the unavoidability and meaning of boundary judgements, not only will our concept of high-quality observation change, but equally our concept of compelling argumentation. I hope to clarify this issue further in my current project *Critical Systems Thinking for Citizens*.

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 do not necessarily exist as such in the actual practice of science. Nor would I equate them with philosopically 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; even the celestial bodies of astronomy are constructed as 'stars', 'moons', '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.

I find it important to associate the 'challenge of the user' with the goal of allowing citizens to acquire a new competence in citizenship.

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 'facts and conclusions'. The deeper reason for this is that we are dealing with an ideal. A competent researcher will always endeavour to make progress toward it, while never assuming that he or she has attained it.

Given the ideal character of the quest for validation, we should not expect philosophers of science, either, to come up with safe epistemological guidelines. 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 since the latter is not accessible except through the statements of observers who, apart from being human and thus imperfect observers, construct 'reality' dependent on their particular viewpoints and purposes, it is clear that such a theory is not available on principle.

Similarly, with regard to the problem of securing compelling argumentation, the necessary basis would consist in a practicable theory of 'rationally' argued consensus. A theory of rational discourse may be able to demonstrate the conditions for a rationally defendable (rather than merely factual) consensus; but, as we have learned from Habermas' (1979) analysis of the 'ideal speech situation', it will not enable us to make those ideal conditions real.

In so far as the methods of natural science appear to provide a proven tool for ensuring scientific progress, many natural scientists may disregard this 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 'facts' and 'conclusions' they will be able to establish.

As applied researchers we should therefore deal especially carefully with the epistemological requirements of 'competence'. But how can we square the circle and become epistemologically competent without sufficient epistemological guidelines?

How can we square the circle and become epistemologically competent without sufficient epistemological guidelines?

The unavailability of a satisfactory answer is probably responsible for the current rise of 'pluralism' in epistemological and methodological issues. In the systems sciences, the rise of pluralism has been heralded particularly in the writings of M.C. Jackson (e.g. 1987, 1990, 1991, 1997a, b; see also Jackson and Keys, 1984, and Flood and Jackson, 1991), G. Midgley (e.g. 1992, 1995a, b, 1996) and J. Mingers (Mingers and Brocklesby, 1996; Mingers and Gill, 1997); in different ways, it also underlies the work of many other authors in the field (e.g. Linstone, 1984 and 1989; Oliga, 1988; Ulrich, 1983; 1988a). But the call for epistemological and methodological 'pluralism', justified as it is by the lack of a sufficient theory 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 'facts and conclusions', the quest for competence in research remains chimerical.

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

(a) 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 on a proper integration of research and practice. The issue that comes up here is the model of the relationship of '*theory' and 'practice',* or 'science' and 'politics', that should underpin our understanding of competence in (applied) research.

(b) 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, that is, by renouncing the quest for sufficient justification in favour of the more realistic quest for a sufficient critique (laying open of justification deficits). The issue here is what I have called '*the critical turn'.*

Mediating Between Theory and Practice

Ever since the rise of science, there has been a hope that political practice, i.e., 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'.[2]

Until the rise of science, Aristotle's view of practice *(praxis)* as a non-scientific domain of ethics and politics was generally accepted. It meant that practice could not be rationalised by means of theoretical knowledge *(theoria)* or technical skill *(poiesis).* In the middle of the seventeenth century, however, the English political philosopher Thomas Hobbes (1588-1679) proposed a first design for the scientisation of politics. His insight was that practical issues raise questions that are accessible to science (namely, insofar as they require theoretical or technical knowledge); once these questions have been identified, the remaining questions will then properly remain inaccessible to science, for they require genuinely normative, subjective *decisions* that lie beyond rationalisation through theory or technique. Thus *decisionism* was born, the doctrine that practical questions allow of scientific rationalisation 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, facet 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.'

For the Enlightenment thinkers, this could not be the last word on the matter. *Veritas, non auctoritas, facet legem,* i.e., 'truth rather than power makes the law', postulated 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 rationalising 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 increasingly tends to determine the need and criteria for political action. One need only think of environmental issues to realise how much indeed science nowadays defines the factual constraints to which politicians must succumb.

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2 The following account is based on my earlier discussion of 'The Rise of Decisionism' in Ulrich, 1983, pp. 67-79. See also Habermas, 1971, pp. 62-80.

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 administration, I have often experienced this peculiar reversal of roles: I was expected to come up with 'scientific' findings ('facts 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 evaluator. The danger is that the genuine function of politics, i.e., ensuring 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 rationalisation of power (Habermas 1971, p. 64). Knowledge no longer serves power, as in the decisionistic model; knowledge now *is* power.

The researcher's understanding of real-world issues increasingly tends to determine the need and criteria for political action.

The German sociologist and philosopher Max Weber (1864-1920) 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 rationalise) the subjective meaning of individual actions or decisions in terms of underlying motivations. Rather like Hobbes, he found that actions or decisions admit of scientific explanation insofar as they can be shown to represent a 'purpose-rational' pursuit of motivations. At the bottom of this concept is 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 judgements of their own and hence are accessible to scientific support. This concept of *purposive-rationality* thus permits a rational choice of (effective and efficient) means at the price of renouncing any attempt to ensure the rationality of the purposes they serve.

Quite in the tradition of Hobbes, Weber thus relegated the choice of ends 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 judgements on ends.

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 really 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.

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 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 judgements 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).

In order to overcome the shortcomings of both the decisionistic and the technocratic models of the relation of 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, that is to say, by taking them to be interdependent (cf. Ulrich, 1983, p. 222 and p. 274; 1988a, p. 147f; and 1993, p. 590). 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 Jürgen Habermas (1971) to have elaborated a model that conforms to these requirements. He calls it the *pragmatist model.*

It seems almost unbelievable how uncritically a majority of contemporary social 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 judgements.

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; 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 summarise 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) analyse 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 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 paper. As we have seen, the quest for competence in research 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 judgements, securing both high-quality observation and compelling argumentation, dealing properly with the practical (ethical) dimension of our 'facts and conclusions', mediating 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 highly 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 cannot raise any kind of validity claims, e.g., regarding the quality of our observations or the rationality of our conclusions. It means, rather, that the manner in which we formulate and justify 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.

It is the researcher's responsibility, then, to make sure that the necessary processes of debate and choice can be made by all those concerned in as competent a way as possible. To this end, the researcher will strive to give those concerned all the relevant information about 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, 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 rather as an effort to deal self-critically with the *limitations* of our competence. The critical turn demands from the researcher a constant effort to be 'on the safe side' of what we 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!

As systems researchers, we might begin this critical effort by understanding and using the *systems idea* critically, in the sense of making a personal commitment to *reflective systems research and practice.* Thus understood, the critical turn will change the way in which we understand the systems idea and, consequently, how we use systems methodologies. Rather than understanding them as a ground for raising claims to rationality, or even some kind of superior 'systemic' rationality, we shall understand them from now on as tools for critical reflection. In other words, we will use them more for the purpose of finding questions than for finding answers.

The critical turn will change the way in which we understand the systems idea and, consequently, how we use systems methodologies.

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 judgements, i.e., of how the 'facts' and 'values' we recognise 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.*

The process of boundary critique also serves as a restraint upon unwarranted claims on the part of researchers or other people who do not employ systems methodologies (or any other methodologies) as self-critically as we might wish. If reflective research practice is not to remain dependent on the good will of researchers alone, it is important that other people can challenge their 'facts and conclusions' by making visible the boundary judgements on which they rely. The point is of course that when it comes to these boundary judgements, researchers — whatever skills in the use of research methods, theoretical knowledge or any other kind of expertise they may possess — are in no better position than other people. Whoever claims the objective validity of some 'facts' or the rationality of some 'conclusions' without at the same time explaining the specific boundary judgements on which these claims depend, can be shown to be arguing on slippery grounds.

I believe that ordinary people can understand this, provided they receive an adequate introduction, and can then challenge unwarranted claims on the part of experts in an effective way, without depending on any special expert knowledge themselves. The employment of boundary judgements for critical purposes has this extraordinary power because it is a perfectly rational form of argumentation, it 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 but also ordinary citizens a *new sense of competence.* I have explained this emancipatory significance of the concept of boundary judgements elsewhere in more detail (see Ulrich, 1983, pp. 301-314; 1984, pp. 341-344; 1987, p. 281f; 1993, pp. 599-605; 1996a, pp. 41f).

The employment of boundary judgements for critical purposes is able to give not only researchers but also ordinary citizens a new sense of competence.

Conclusion

At the outset I proposed that to 'understand' means to be able to formulate a question: namely, that question which is answered accurately by what we (assume that we) know or which at least tells us accurately what we do not know. 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 important reflection, I have tried to offer a few topics for reflection. There are, of course, many other topics you might consider, too; those I have chosen may perhaps serve as a starting point from which to go on to whatever additional issues you think relevant for clarifying or enriching 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 systems thinking 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 our professional lives, we 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 recognise it when you encounter it. Sooner or later you will find at least a preliminary formulation that proves meaningful to you.

Perhaps you wish you had an example. Should I share my tentative question with you? At the end of this paper, I hope you are sufficiently prepared not to mistake it for your own question.

I first encountered 'my' fundamental question in the year 1976 when I moved to the University of California at Berkeley to study with West Churchman, who had helped to pioneer the field of Operations Research and Management Science in the 1950s and then, since the 1960s, has become 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' — points to the need for a *holistic* understanding of the systems approach, since we cannot hope to achieve their intent without a sincere quest for *'sweeping in'* all aspects of an issue, i.e., for 'understanding the whole system' (see Singer 1957; Churchman, 1968, p. 3, 1971, pp. 165-167, 1979, p. 45f, and 1982, pp. 12-15 and 130-132; Ulrich, 1994, p. 26f). His life-long quest to understand the question thus led him to conceive of 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![3]

For me, each of the key expressions in the question points to the need for a *critical*

3 For an appreciation of Churchman's contribution and the importance it had for me, see Ulrich, 1988b.

understanding of the systems approach, since we cannot hope to achieve their intent without a persistent critical effort to understand the ways in which we fail to be sufficiently holistic. My quest to understand the implications of the question thus led me from my earlier 'pre-critical' to a 'critical' (or 'critically-holistic', as distinguished from holistic, see Ulrich, 1993) understanding of the systems approach. It made me seek for ways to bring together the two previously separate traditions of systemic and of critical thinking in what has come to be called 'critical systems thinking' (CST), a project that is far from being completed.

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. *It is because I tried, and still try, to understand systems thinking so that it responds to that fundamental question.* There is no definitive positive answer to the question, of course; but that surely does not dispense me from struggling to gain at least some critical competence in dealing with it.

I wish you good luck in your quest for competence.

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