

Beyond Checkland & Scholes: Improving SSM

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Abstract

This paper examines Checkland and Scholes (1990) discussion of soft systems methodology (SSM). Predictable difficulties that novice users have with using SSM for research purposes are identified and explored to clarify problems, inconsistencies and omissions in the text. Topics include a) designing a system to use SSM; b) defining and implementing validity criteria; c) what model-building is intended to produce; and d) carrying out social and political analyses of the environments of SSM projects. The paper concludes with several recommendations for improving systems methodologies as applied to human systems.

Soft Systems Methodology in Action, by Checkland & Scholes (1990) is one of the most often cited texts in systems research. The authors' extensive work on the development of methods for using systems theory in human action settings has been invaluable to thousands of students of systems design, behavior and change. The 1990 text provides theory, modeling techniques and practical examples that can be of use to organizational consultants, managers and researchers.

It is the research use of soft systems methodology (SSM) that I wish to discuss in this paper, not because research is more important than the practical problem solving that drives consulting and managing, but because research is a special kind of problem solving—one that has particular constraints and imposes particular demands not found in other kinds of problem solving. Hopefully, the unique constraints and demands of using SSM as a research methodology will become clear by the end of this paper, so I will not iterate them now since each requires background discussion. Let me simply note that my concern with SSM as a research methodology has arisen chiefly through my involvement over the past 25 years with postgraduate dissertation research, in which student applications of various qualitative methodologies, including SSM and other systems approaches, have posed challenges. As Checkland & Scholes note in their chapter 3 (p. 60), one learns a great deal about systems methodology by observing other people trying to do it, particularly when one is in the responsible role of trying to help them do it “right.”

Embedded in all systems studies, regardless of whether the task is consulting, managing or researching, is the question, “When we have all done our best here, how do we decide if what we have done has value?” Checkland and Scholes might want me to add the question, “How might we improve on what we have done?”

There is a great range of criteria that can be evoked to define “what we have done”, “what is valuable”, and what constitutes “improvement”. What is unavoidable is the fact that we need evaluative criteria. Decisions and actions flow from our evaluations of systems studies. For example, a corporate manager is asked by a CEO to take on oversight of systems planning work and, as a result, may be either promoted or discreetly forgotten for further career advancement depending on performance; a legal suit arises as a result of dissatisfaction on the part of public agency clients with consultant performance using systems methods; a systems research dissertation is either accepted or rejected by university faculty; systems research articles may or may not find a publisher. Quality matters. Consequences require some thought about the criteria we use to judge the processes and outcomes of SSM applications. These judgments in aggregate reflect our sense of the value of the methodology itself.

Checkland and Scholes (1990) are impatient with evaluations of their method. They claim that people’s interest in whether or not SSM “works” is impossible to determine because it is impossible to attribute successes or failures to the method itself and not to the user of the method or the organizational environment in which it is used. They declare the “does it work?” question to be “undecidable” (p. 299). However, observers, participants and users can tell the difference between competence and incompetence in carrying out the design and implementation of interventions based on SSM. Checkland and Scholes themselves suggest a way to do this using the criteria of efficacy, efficiency and effectiveness which, if incorporated into the overall SSM design as part of monitoring and control systems, serve as a basis for judging in an on-going manner how competently or incompetently the various stages and activities that characterize SSM are being carried out (p. 39; 294). They suggest adding, but do not themselves explicitly employ criteria of ethicality and elegance to the list (p. 42). As result, the suggestion that we cannot differentially evaluate the methodology and the competence of the designated user(s) of the methodology is not a tenable position for Checkland and Scholes to take.

Second, we can compare SSM as an “ideal type of construct” (p. 58) with the various instantiations of it in different settings and with different users. Moreover, we can make judgments about the fit or lack thereof between ideal SSM and actual SSM. Checkland and Scholes themselves do this in their numerous examples of SSM applications in actual settings (chapters 3-9). In addition, they provide the criteria for how we may do the same with reference to the constitutive rules of SSM (pp. 286-7) and its epistemology (pp. 288-289), as well as requiring on-going analyses of the social and political environment of the study as part of SSM. Consequently, the suggestion that we cannot differentially evaluate the method and the contribution of the environmental context in which it is applied is not tenable.

Third, we can compare SSM with other methods. Checkland and Scholes, rather peevisly, claim that such comparison is “misguided zeal of academic comparers...and students with dissertations to write” (p.299). However, the continued zeal (of which I am apparently guilty) that disturbs Checkland and Scholes arises from actual, human problem situations where decisions must be made about actions to take in the real world of a university, given the university’s cultural imperative to engage in the production of knowledge. I do not find comparisons of

methods an undecidable issue; students and faculty make such decisions routinely in order to fulfill their roles within a university system. Other systems thinkers have developed schemas for comparisons (e.g., Jackson & Keys, 1984; Ledington & Watson, 1998).

The actual form that the comparison question might take in a real setting is one that I hear often from students: “I’m considering using SSM for my dissertation research, and I need to get advice on what the pros and cons are of working with this methodology so that I can make a decision about whether or not I should, or can, use it. What will I be getting into as compared to other approaches?” We can talk about the tradeoffs of various methodologies in terms of the kinds of user skills that are prerequisite, the time and resources required in order to carry out essential tasks, challenge points inherent in the methodology, success rates of previous students in meeting those challenges, etc. Instead of declaring the comparison question moot, I have found it more useful to look at the student/faculty decision process with the help of SSM. One strategy for doing this is to ask the potential user of SSM to produce a model of “a system to use SSM,” a task Checkland and Scholes recommend for all users regardless of their particular purpose (op.cit. pp.290-1). I ask the potential user to be sure to locate him/herself in relation to components of the model, as well as locate the requisite university faculty committee that must judge the dissertation as an outcome of the research. I tell them that an explicit model of SSM as a dissertation study does not come ready-made in the Checkland and Scholes text, and that they will have to do some SSM work of their own in order to produce a model for their particular situation. This modeling exercise has been a pedagogically useful way to engage students in answering questions about what using SSM entails and in structuring a decision process about whether or not to use it.

Working with somewhat naïve and novice users of SSM, rather than experts, has served to highlight for me a number of issues that have caused me to question the integrity of SSM as a methodology in its current (1990) form. There is precedent in the history of SSM development for the productive use of pedagogical concerns as a way to raise substantive concerns about SSM. In particular, Checkland and Scholes have acknowledged the value of Naughton’s (1977) question: “How can we know that what someone may *claim* to be a use of SSM is legitimately so described?” (Cited in Checkland and Scholes, 1990, p. 285). This is a question we might legitimately expect a research user of SSM to answer. Naughton’s interest in coherently teaching SSM to students at the Open University led him to see the necessity for constitutive and strategic rules to clarify definitions of SSM—a need to which Checkland and Scholes responded (ibid. pp. 284-287). Similarly, my experience of finding that there are consistently occurring problems and areas of failure in novice’s applications of SSM has led me to question whether or not the failures can be blamed exclusively on the novice user. We must interrogate the methodology as well. That this interrogation is important is accentuated by Checkland and Scholes’ claim that SSM does not require highly experienced analysts, nor does it depend on high intellectual input. “SSM can work with whatever ‘intellectual input’ is available!” (p. 10). While this may be true of participants in an SSM project, I do not believe that it is true of the user(s) of SSM who must take administrative responsibility for the design and implementation of an SSM project as a whole. Thus, I have found it useful to add a distinction to

Checkland and Scholes' global use of the term "users" (p. 28). *Primary* users are either a small team or a single person taking overall operational responsibility for carrying out SSM; *collaborating* users are those who may have consultative or participatory roles. Researchers, particularly novices, need to see themselves as primary users regardless of how much collaboration or participation they may seek to engender as they carry out their study. Why this is so will be more apparent as I turn to discussion of the problems that novice users of SSM predictably encounter.

Recurring problems in novice's applications of SSM

I wish to briefly list four problems that I have encountered with novice primary users of SSM and then go back to each for a fuller discussion. This step will serve to introduce the problems as a systemic whole and make it possible to keep in mind the extent to which the problems are often interactive and interdependent. While I have encountered more than four problems, I have selected those for discussion here that are not easily remedied by recourse to other existing methodologies and which thus constitute a general challenge to systems researchers.

Novices have difficulty with the following, which thus become major challenges in using SSM for research purposes:

- 1) Designing a system to use SSM that is specific about the distinctions and relationships between consulting, managing and researching.
- 2) Defining outcomes, defining criteria for claiming that outcomes are valid and implementing criteria for research validity as part of the system to use SSM.
- 3) Focusing on what model building is intended to produce—"debate" (Checkland & Scholes, 1990, p. 42-43) and "accommodation" (ibid. p. 29), and, instead get caught up in overvaluing model building as an activity.
- 4) Carrying out Analyses Two (of the social system) and Three (of the political system) to describe the environment in which the SSM project is embedded (ibid. pp.48-51).

I will discuss each of these points more fully in turn.

Designing a system to use SSM. Checkland and Scholes (1990) assist novices in the form of a visual model (See reproduction of Figure 10.1 from p. 278). However, this model is little more than a warning that it is valuable to think about the SSM study *as a study* before one gets too involved in carrying out cycles of SSM itself. What the components might look like in more detail is elaborated in Figure 10.7 (from p.293). A final version (Figure 10.8), shows a complete system to use SSM, according to Checkland & Scholes, "which maps all the experiences of using it" (p. 294).

During discussion, novices have to think about what it means to carry out SSM in two different organizational settings simultaneously; both of which have different cultures and definitions of what the relevant tasks are. They must clarify that there are several activities central to the use of SSM for research purposes, all of which require conceptual modeling, discussion and reflection:

- managing SSM activities;
- identifying user learning;
- identifying participants' individual learning as well as collective organizational learning;
- selecting the learning that has general applicability either to improving SSM or to improving our understanding of human activity systems or both;
- identifying change or improvements in the setting;
- evaluating the effectiveness of improvements; and,
- writing an account of all of this that meets criteria that are ancillary to SSM, but which are central to one of the settings the SSM research process must include—the university as degree grantor and knowledge producer.

As I will later show, how one includes a university setting and its requirements in an SSM model for doing SSM research is related to how researchers appreciate on-going international debate about what constitutes the validity of research outcomes.

Clearly, the SSM process itself can be of great help in structuring a “debate” between students and faculty focused on a decision about whether or not to use SSM in dissertation research, how to use it, and how research outcomes may be defined and evaluated. In principle, all the required activities involved in using SSM for dissertation research can be modeled using SSM concepts, although multiple subsystems are needed to reflect the different levels of responsibility for monitoring and appreciating. However, what has alerted me to the existence of a failure on the part of Checkland and Scholes is the predictability of novices' confusion when they attempt to model a system to use SSM. I find that the confusion originates in the Checkland and Scholes text because of their consistent blurring of the distinction between “using SSM” and “research on SSM”. Interestingly, this is a blurring pointed out to them by a student and which they acknowledge as a real concern (op.cit. pp. 297-298). Their response, however, is to claim that there is no firm line between “using SSM” and “research on SSM”. They state “Every sophisticated use of methodology *needs to be* research on its use in a particular context” (p. 298). They suggest that the blurring of use and research simply extends the range of SSM to cover a greater variety of real life situations than was possible with former pre-

1990 versions of the methodology. SSM has simply become “more complicated” (p. 298) and “richer” (p. 299). In its 1990 version, SSM now includes two modes: Mode 1 for highlighted studies and Mode 2 for managing (pp. 163-164; 280-284).

Unfortunately, Checkland and Scholes, without resolving potential contradictions arising from the blurring of SSM-to-solve-problems and SSM-to-research-SSM, lay blame for resulting confusion at the door of classrooms of management schools where simpler versions of both SSM and of the real world are taught. They say,

“We can forgive the pedagogues’ simplification: after all, the true complexity of the real world *has* to be simplified for classroom consumption. But in the real world outside of classrooms, if we want seriously to bring about improvements, we need to work with the richer account of SSM given above” (p. 299) “...a mature methodology is defined by the Mode 1-Mode 2 distinction, the new Constitutive Rules and the system to use SSM shown in Figure 10.8.” (p. 298)

What is rather sad here is that the authors’ rhetoric is guaranteed to seduce the novice user, who naturally does not wish to adopt a version of SSM taught in classrooms by pedagogues who are out-of-date and stuck in “simplification” about what SSM is and isn’t. They much prefer to side with Checkland and Scholes who promise “the real world outside of classrooms” and serious “improvements” in real life settings.

Meanwhile, what gets occluded is that Checkland and Scholes make absolutely no differentiation between research on SSM and research on human systems using SSM. Apparently, Checkland and Scholes do not consider SSM a methodology for learning about the real world of human organizations. Rather, it is a methodology for studying methodology. What we learn about in their text is not more about the characteristics and processes of human (soft) systems. Rather, we learn about how SSM has evolved in complexity, range of use and level of detailed articulation as a methodology. Characteristics and processes of human systems are indeed discussed, but they appear as the set of initial theoretical assumptions that justify the need for SSM (pp. 1-8), not as a potential source of issues for study, debate and further development. Predictably, novices who use the Checkland and Scholes (1990) text are understandably confused about just what it is they actually will be researching when they elect to use SSM.

Defining and implementing validity criteria. My concern with the validity of SSM as a research methodology has arisen out of some painful struggles through dissertation documents flawed by confusion about what constitutes a successful outcome of SSM, and about the relationship between SSM outcomes and research outcomes using SSM. I have experienced encounters with angry students who say, for example, “What do you mean, faculty won’t accept my dissertation? My project was a brilliant success! I got hired by the company, for God’s sakes! They are still talking about what an incredible learning experience it was for them!” Similarly, I have sat with students who report that they cannot write anything because their projects failed: “Participants didn’t do anything as a result of it, a lot of people were unhappy with the whole thing, and I don’t see any organizational improvements.”

To which my reply, “This doesn’t necessarily mean your dissertation will be a failure,” usually produces a blank stare of disbelief. These kinds of encounters have led me to question, first, the stance taken by Checkland and Scholes regarding what constitutes a successful outcome of SSM, and, second, the way the authors position the outcomes of SSM in relation to on-going academic discussion about the validity of research outcomes.

First, let me discuss definitions of successful SSM outcomes, and how one verifies them. For Checkland and Scholes (1990), SSM doesn’t necessarily produce outcomes. Instead, SSM is a “learning cycle which is ideally never-ending” (p. 28). They caution against being too rooted in hard engineering obsessions with finding the single best solution to a defined problem. They state that SSM has “moved experientially from an approach aimed at optimizing a system to an approach based on articulating and enacting a systemic process of learning” (p. 15). The learning processes of SSM do not necessarily produce action, although they can. Rather, SSM processes “are used to initiate and orchestrate the debate about purposeful change” (p.7, emphasis mine). “What is looked for in the debate is the emergence of some changes which could be implemented in the real world and which would represent an *accommodation* between different interests” (p.29). Implementing changes necessitates another iteration of SSM because “[t] his implementation is, of course, itself ‘a problem situation’, and it is not unusual to use SSM to tackle it” (p. 52).

Enacting changes (using SSM) must, according to Checkland and Scholes (p. 52), produce plans for action that are both “systemically desirable” and “culturally feasible.” Potential changes are “systemically desirable if these relevant systems [for implementing the changes] are in fact perceived to be truly relevant” (p. 52). Moreover, changes will take place “only if they are perceived as meaningful within that culture, within its worldview” (p. 52). In other words, a change must be judged applicable (relevant) to the problem situation at hand, and it must be acceptable within the cultural meaning structures of the people who have a stake in improvements. Neither aspect can be reduced to the other.

As to verification, the authors present a very simple, straightforward strategy: they stress the importance of relying on participants’ perceptions, reflected in what they reveal through verbal debate during iterations of SSM cycles (p.6). Outcomes are verified and validated as successful by the people who are participants in the process, who have been experientially affected by it, and who have a stake in particular kinds of outcomes (e.g., changes, learning). In other words, the SSM user can verify by checking with participants that outcomes actually occurred, that they occurred in relation to the SSM process in the manner described by the user, and that they have importance to participants.

Participant verification is a common technique among qualitative researchers for establishing the accuracy of interpretation of participants’ intentions and meaning. SSM can be viewed as one of many varieties of qualitative research methodologies because it shares the defining feature of qualitative research: Its data to be analyzed are verbal rather than numerical. To simplify rather too much, qualitative, as opposed to quantitative, research relies primarily on strategies of interpretation rather than strategies of mathematical summary and statistical

analysis. There is a kind of inevitable self-reflexivity to qualitative analysis that is unavoidable in human systems where people must judge meaning and the value of their own and each other's actions in a collective way. These self-reflexive judgments are neither transparent nor free of complexity as I have noted elsewhere (Salner, 1996). Suffice it to say here that the technique offered by Checkland and Scholes (which forms the core of their SSM verification and validation strategy) is a commonly used way to validate the accuracy of researcher interpretations of the meaning of verbal communications in individual and group interviews or focus group situations (Denzin & Lincoln, 1994). Thus, Checkland and Scholes base a part of their validity argument on a strategy that, though it is not alone sufficient, is thoroughly within the mainstream of acceptable research practices.

So, what is the problem? Following Checkland and Scholes, we can identify outcomes (e.g., learning and/or improvements), verify them (with participants), and evaluate their relevance and meaning (with participants). Why then do novices so often get lost when asked to identify and defend the research outcomes of their SSM project? Why do they have predictable difficulty identifying just what constitutes relevant and meaningful learning or change in the context of research using SSM?

Part of the problem has been discussed earlier: Checkland and Scholes completely fail to acknowledge that SSM is a methodology for 1) facilitating organizational improvements; and 2) improving SSM. It is nothing more.

A second problem stems from Checkland and Scholes' stance toward validity issues in general. However, before I discuss this point, I would like to clarify my own stance vis-à-vis what constitutes acceptable research using SSM because I can hear lurking in the background a suspicion on the part of readers that perhaps I am holding users of SSM to an undeclared standard of validation that may be inappropriate. Since any particular standards of validation are rooted in an epistemological framework, or *Weltanschauung*, I need to be clear here about what my criteria are.

First, I believe that criteria for validity are contextual which means they follow from the particular philosophical assumptions (both ontological and epistemological) on which the methodology rests. Whether or not I agree with the philosophical assumptions is moot when it comes to supervising student research. As a matter of pedagogical practice, I do not impose my philosophy on my students; my own views are available elsewhere (e.g., Salner, 1990). Rather, I insist that students think through the implications of the methodology they have chosen in order to articulate and defend its integrity. If they cannot do this, they need to get another methodology or a better defense. Thus, I am concerned chiefly with criteria for deciding whether or not a user is actually employing the declared methodology or, unwittingly, is doing something else. The criteria for what constitutes the methodology of SSM has been carefully set forth by Checkland and Scholes in their new (1990) constitutive rules (pp. 286-287), so thus far, we have no disagreement.

My second criterion is that research outcomes must be verifiable by fellow researchers; they are communal property. This criteria does not necessarily imply a correspondence theory of truth, which Checkland and Scholes are at pains to reject as inappropriate to the world of human interaction (op.cit. p. 1-8). The authors

repeatedly stress (e.g., p. 41) that SSM models are not versions of reality; their use is to facilitate debate and accommodation leading to communal decisions and actions in the face of problem situations. I am entirely sympathetic to Checkland and Scholes' interest in making it clear that SSM is not a species of experimental science. Instead, I believe that verification criteria SSM must meet are the same ones that apply to other common situations where the only data we have are verbal, and the goal is understanding via reconstruction and construction of meaning. One such example lies in the codes of procedure and action that support responsible journalism; another is derived from several centuries of British common law that has established a praxis by which barristers argue a case before a jury. Thus, a juridical standard of truth (in which truth is a public construction based on documented sources of evidence) can be said to be a more authentic standard to apply to human action than one that requires correspondence with empirical reality. Readers who have served on a jury will recognize that the question of "what *really* happened" is usually unanswerable, but a verdict can be rendered nevertheless. The emphasis is on agreed upon standards about what constitutes "evidence" and "inferences from evidence".

What follows from this human science criteria as opposed to natural science criteria is that any conclusions set forth using SSM must be based on a recoverable data trail coupled with a logical chain of inferences that can be followed by any interested party. Checkland and Scholes do not stress this point, though one can find evidence that they accept its inevitability. For example, in describing an SSM intervention in the Product Marketing Division of ICL, the laboriously recorded data analysis of more than 100 interviews is reviewed so that the authors can state with some confidence that such analysis "made it possible to mount an argument for change which was not simply an intuitive reaction to the conversations held; it was an argument which could be explicitly retraced at any time with links to supporting evidence" (pp.198-199). With this criterion, I have no conflict with Checkland and Scholes, although I recognize the difficulties that novices have in actually gathering useable data records while under the press of orchestrating a complex intervention in a complex social setting with a large cast of characters! Dissertations do get rejected because they do not demonstrate sufficient evidence to support conclusions.

My third criterion is that a research user of SSM must be able to point to evidence-based conclusions that constitute a new contribution to knowledge. This, after all, is what a dissertation purports to do. No new knowledge equals no thesis. Here is where I do have difficulty with Checkland and Scholes because of where they tend to lead readers of their work. On the surface, the authors present SSM as a way to learn. It is a methodology of and for inquiry. However, the learning the text presents is always the users' learning albeit derived in relation to participants' learning, but user learning nevertheless. In the worst case scenario, novices become convinced that their own personal learning in and of itself constitutes a valid research outcome. Alternatively, at one level of remove, if their participants testify as a group to having learned something, no matter how trivial, they believe that a research outcome has been assured. In these cases, novices need to be asked about the difference between writing a diary, writing autobiography, and writing research. We need to address the same question to Checkland and Scholes.

Soft Systems Methodology in Action might be described as intellectual autobiography. It chronicles Checkland's career effort (with, in this case, the assistance of Scholes) to find a way to help human organizations make more effective decisions in the face of uncertainty and complexity. This is noble goal, worth writing about, and their text supplies plenty of evidence that the goal has been successfully achieved in many cases. Nevertheless, the book is primarily a narrative of how Checkland and Scholes have designed a successful consulting practice rather than a contribution to a new understanding of human systems. A potential conflict of interest lurks in the background here and is revealed in a comment by the authors. From within the context of a company where they staged an SSM intervention, they wonder about whether changes can be attributed to their methodology or are due to something else. They say, "A practical consideration for the authors was that the 'end' of each piece of work generated requests for follow-up work" (pp.214-215). Here they imply that one can evaluate the effectiveness of a methodology on the basis of consultants' skill in keeping the conversation about future business contracts going as long as possible!

From this example, I conclude that we must define validity criteria that are appropriate for each context and each methodology, then model the relevant human activity systems out of which concerns with validity arise. In short, the difference lies in the shift from the universal evaluative question, "How do we decide if what we have done here has value?" (which if one's goal is to be successful consultants can lead to specific monetary valuation) to the more specific question (which helps to define and demarcate research problem solving from problem solving in general), "How does what we have done here lead to a valid conclusion on which we can base a new systemic understanding of some aspect of human action in organizations?"

To summarize here, I have presented an argument to clarify that my reservations about the integrity of SSM as set forth by Checkland and Scholes (1990) do not reduce to a collision of paradigms and hence to a dispute about differing worldviews grounded in incompatible epistemologies. My argument is based on three evaluative criteria to which I hold the work of dissertation research students: 1) fidelity to the philosophical foundations on which a methodology rests; 2) publicly verifiable evidence to support conclusions; and, 3) identification of a new or unique contribution to knowledge.

I find no reason to think that Checkland and Scholes would take issue with these criteria or judge them to be unfairly reflective of epistemic prejudice. I believe that we do need a *human* science approach in order to overcome the limits of natural science approaches which have been so amply demonstrated in systems research during the past half century—a point stressed by Checkland and Scholes themselves (ibid. pp. 17-27). My dispute with Checkland and Scholes centers entirely on what I perceive to be a misrepresentation of SSM as a research methodology. Such misrepresentation not only has dire consequences for future researchers, but it retards general effort behind the development and advancement of methodologies, epistemologies, validity criteria, and research praxis that can enlighten us further about the nature and dynamics of human systems.

I cannot leave the topic of research validity without a probe into the epistemology reflected in definitions of validity (Salner, 1986c). We need to be

clear about how Checkland and Scholes position SSM in relation to on-going discussion and debate reflecting various epistemological positions. Here is another source of problems.

Checkland and Scholes fall back on the simplest and least persuasive of debating techniques, the strawman argument. Rather than concede that the activity of identifying standards for distinguishing valid from invalid conclusions is an essential and integral aspect of using SSM for research, they seek to position concern about research validity in the (hard systems) engineering community—the “strawman.” This community is portrayed offhandedly throughout the text as unilaterally adhering to rigid, technical frameworks rooted in an outdated positivism, rationalism or misunderstanding of the salient characteristics of the human world (e.g., op.cit. pp. 4-5). Rather than make common cause with other qualitative researchers in the social sciences who try to be true to the special features of the human world (e.g., Polkinghorne, 1980), they merely polarize the popular distinction between hard (engineering-oriented) and soft (management-oriented) systems approaches. They state, “Within systems thinking there are two complementary traditions. The ‘hard’ tradition takes the world to be systemic; the ‘soft’ tradition creates a process of inquiry as a system” (p. 25). What Checkland and Scholes are in effect contrasting is a tradition rooted in realist ontology with a tradition rooted in constructivist ontology. However, they then claim that “SSM is a systemic process of enquiry which also happens to make use of systems models. It thus subsumes the hard approach, which is a special case of it, one arising when there is local agreement on some system to be engineered” (op.cit. p. 25). This statement simply betrays their lack of understanding of the philosophical ground on which they tread. I will amplify this point in the next section.

What model building is intended to produce. Part of Checkland and Scholes’ position is based on making a clear distinction between models as representations of reality (ontological realism) and models as a strategy for producing focused debate about differing human perceptions of the real world (interpretation as epistemology) (p. 41).

Nowhere do Checkland and Scholes define “debate” fully enough for them to claim that this concept can serve as: a) the cornerstone of their epistemology, b) a differentiating factor between soft and hard systems approaches, c) an outcome of SSM activities, and d) a source for verifying and validating other outcomes (e.g., learning and/or situational improvements).

Debate is used interchangeably with the term *discussion*, and they add that debate should be “well-structured and coherent” (p. 42). The means for debating depends primarily on “formal questioning” (p. 43). That debate, as a construct, is underdeveloped by Checkland and Scholes is revealed in their loose description: “It may be carried out by a group of people gathered in one place at one time to have discussion, or carried out in one-to-one interviews or dialogues spread over a period of time” (p. 43). In one reported study, “the debate took place at one meeting between Scholes and the initiator of the study” (p. 43).

What apparently escapes Checkland and Scholes is the potential here for rank manipulation of participants and of the conditions under which they participate

in SSM debates. How effective is participation that takes place during one meeting between the user of SSM and the initiator of the study? To what extent can the authors claim that participants' testimonials about their learning were freely obtained, when there are such opportunities for the baldest exercise of position power and/or charismatic power? Manipulation can and does occur during SSM projects. The authors confess that "it became a workshop joke among participants that Leemhuis [one of the primary users in the SSM application] was 'manipulating' them, but they were indulgent as long as what was happening was clearly relevant to issues they regarded as important" (p. 242). One might wonder if a relevant participant issue might be keeping one's jobs by cooperating with an SSM project in which one played no role as initiator.

Given a certain blindness here to subtle issues of institutionalized influence, the authors' final paragraph is no surprise.

"Finally, it seems relevant to the authors to remark that the very best uses of SSM seem always to exhibit a certain dash, a light-footedness, a deft charm. In this sense the role of the approach is akin to that of the cavalry in nineteenth century war: it can add a certain tone to what might otherwise be a vulgar brawl" (p. 302).

The rhetoric linking theatrical performance, battle charges and social class aside, it seems preposterous that Checkland and Scholes can be unaware of the impact of their own power to structure SSM debate. Yet, they disguise this power, actually render it unseeable, in their metaphor-laden sentence. They collapse *users of SSM* into "uses of SSM", and the *role of users* into "the role of the approach". In this deft and charming way, they attribute the charismatic power of the users to the power of the approach itself, and thus light-footedly escape the consequences of the kind of self-insight that is required of valid qualitative, interpretive, self-reflexive science. They may indeed be nothing more than charming song and dance men, but there is a possibility that they remain blind to their aggression (made quaint by reference to long dead 19th century cavalrymen) and their class bias (of "tone" as opposed to "vulgar brawl") that defines their stance toward the "best use" of SSM.

The effect of undervaluing debate and of naïvely failing to comprehend the enormous opportunities for abuse via the "structuring" of debate in the service of corporate/user interests, is to make it impossible to carry out relevant or meaningful modeling of the political context of SSM, which the authors insist must be done, with cooperation of participants (pp.48-51). The authors seem to be partially aware of these limitations.

"There is a natural reluctance to be blunt about the crudities of power, and there is sense in which the real politics of a situation, not publicly acknowledged, will always retreat to a tacit level beyond whatever is the explicit level of analysis" (p. 51).

Here we find what appears to one of the better justifications in the authors' own words for the addition of a critical theory perspective (Connerton, 1978) to the epistemology of SSM as a means for dealing with latent and tacit power in settings where SSM is carried out.

Checkland, in particular, was repeatedly criticized throughout the 1980s for the lack of a critical/ethical component in SSM. This criticism, and Checkland's responses to it, has been explored in a thorough review of the pre-1990 literature by Jackson (1991) who concludes:

“The kind of open, participative debate that is essential for the success of the soft systems approach, and is the only justification for the results obtained, is impossible to obtain in problem situations where there is fundamental conflict between interest groups that have access to unequal power resources. Soft systems thinking either has to walk away from these problem situations, or it has to fly in the face of its own philosophical principles and acquiesce in proposed changes emerging from limited debates characterized by distorted communication. (p. 198)

What I wish to add to Jackson's analysis is to stress the disastrous consequences for the validity of SSM outcomes that comes about from the failure of a critical sensibility on the part of users who are themselves part of the power equation. If it cannot be demonstrated that participants' statements of verification and validation are offered without constraint and without the influence of structural sources of power (no matter how “light-footed”), then there is no verification or validity at all! It is difficult to see how we can accept the authors' claim that via debate, which “allows arguments to be made explicit,” SSM “brings a little of the rigour...entailed in natural science into applied social science” (Checkland & Scholes, 1990, p. 302).

Checkland and Scholes go on to claim that, in addition, “SSM can help the creation of *shared* appreciations or the recognition that endemic conflicts have to be *accommodated*” (p. 302). The authors define *accommodation* as what debate aims to bring about as a prerequisite for problem solving. They stress that it is not consensus. Instead, it is a way that interests in competition for power can be brought into harmony (p. 327). There are a lot of ways to bring about accommodation within groups, none of which are discussed by the authors: for example, formal bargaining, win-lose competition, coercion, win-win tradeoffs, manipulation and obfuscation. What assurances do the authors provide us that agreements resulting from their accommodation process can be relied on? None.

Consequently, novice users of SSM tend to focus on the quality of the modeling process as if the quality of debate and accommodation depend upon it. What the text emphasizes is the structure of models not the structure of debate. In other words, debate in SSM becomes another latent social construct. Meanwhile, *accommodation* can scarcely be conceptually distinguished from conformity to the imperatives of the particular organizational culture set against the background of national and local social habits. Accommodation strategies brought forth by SSM may lead to positive purposeful actions. Or, they may lead to nothing more than negative exercises in futility for participants who never quite understand what game they are playing. In any case, what is clear is that SSM, as portrayed by Checkland and Scholes (1990), is a methodology of the status quo.

Carrying out social and political analyses. Given the above discussion, it is not hard to see why novices predictably do a superficial job with the task of modeling the social and political contexts into which they take SSM.

SSM places a number of conditions on users. First, users must accept the particular worldview that is put forth by Checkland and Scholes (pp. 1-5; 23-27) as the foundation for SSM. This view is consistent with constructivist thinking and includes both ontological and epistemological assumptions (cf., Lincoln, 1990). I will amplify what this means further on. To this is added a set of assumptions referred to as “systems thinking” (Checkland & Scholes, 1990, pp.18-27). I am not questioning here the appropriateness of a constructivist, systemic worldview as a way to think about the human world or the need for a unique methodology to respond to the special characteristics of this world. What is of concern to me instead is a common enough failure to give sufficient thought to what it is we are endorsing by way of philosophical assumptions, or to the compatibility of these assumptions with the way we actually carry out inquiry. SSM can be espoused as a mental framework but without the accompanying internalized support of the assumptions that would ensure competent use of the methodology. The problems that come about as a result of inconsistencies between espoused theories and theories-in-use has been explored by Argyris (1970), for example.

I have written elsewhere (Salner, 1986a; 1986b) about how these difficulties play out in both systems learning and research learning. As a result of my own studies of problems that occur in novices’ applications of systems concepts and qualitative research methods, I can often attribute a student’s failure to inadequate epistemic development. I will not repeat my arguments here, but wish to point in their direction because I believe that the particular cognitive and epistemological demands that the theory of SSM makes on users of the methodology undermines Checkland’s and Scholes’ claim that no particular expertise is required to use SSM. I tend to disagree with Checkland and Scholes, and agree with Wang and Smith (cited by Checkland & Scholes, *ibid.* p. 10) that SSM is “very sophisticated and mature: it requires highly experienced analysts.” It is mature and sophisticated in the sense that it requires the development of highly complex and differentiated cognitive structures on the part of users. Research has demonstrated that the level of cognitive complexity that is required is relatively rare, not only among university students, but among their faculty as well! (Salner, 1986a). Educational efforts to accelerate students’ cognitive and epistemological development in relation to systems learning are uncommon, but Richard Bawden and the Centre for Systemic Development at University of Western Sydney’s Hawkesbury campus in Australia have achieved a notable level of understanding and success (See Bawden, 1990; 1995; Bawden & Macadam, 1990). My own experience has shown me that acceleration can be achieved albeit laboriously. What help, if any, do novices get from Checkland and Scholes when they turn to the 1990 text for help in clarifying and integrating assumptions about ontology, epistemology and social experience in SSM?

Unfortunately for their readers, Checkland and Scholes are not always philosophically consistent. For example, as I have noted above, their assumptions about the unique aspects of the human world are consistent with constructivist ontology. However, Checkland and Scholes continuously fall into subjectivist

ontology when dealing with epistemological issues. They stress repeatedly that each human being has a subjective perspective on life in the real world that can only be shared by talking about it (debating). No one has the true perspective and each person's perspective is as valuable as the next. In other words, the source and means of understanding (i.e., their understanding of epistemology) is grounded in subjectivity and is relativistic. It could be summarized as, "You have your perspective; I have mine; that's the end of it. Any judgments about which perspective is more valid are 'undecidable.'" Such a position has been thoroughly debunked by philosophers (e.g., Bernstein, 1983). It fails because it is a solipsistic position from which no judgments of worth or validity can be made about any actions, let alone those actions to improve human systems.

The position taken by Checkland and Scholes forces them to continuously stress that SSM deals only with perceptions of reality not with reality itself. However, they have difficulty sticking with this position. In places, they stray into an uneasy realism. For example, their definition of the "systems thinking world" is "the world in which conscious reflection on the 'real world' using systems ideas takes place" (p. 288). How is it possible then for them to argue that SSM creates systems models that are not reflections of the real world but are instead only models for talking about individual perceptions?

In contrast, constructivists acknowledge reality as more than simply "perceptions". In this assumption, they are not subjectivists. They are not objectivists either. Where they part company with objectivism is in their position that reality, while it exists, is fundamentally unknowable except through the means of human understanding and interpretation which is intersubjective in nature. Thus, for example, objectivists would tend to assume that an engineered bridge did not fall down because the engineer understood reality correctly. Subjectivists have no explanation for why the bridge either falls or stands, and might claim that there is no bridge unless there is a person there to perceive it. Constructivists accept a realist stance but make no claims that reality can be known in and of itself. Rather, they would claim that we understand nature in terms of our human interest in it, e.g., building a bridge. If the bridge stands, as we hope, we can be said to be in harmony with some aspects of reality, although our theories about why this is so are as likely to be wild fictions as accurate models. In other words, constructivists accept the inevitability of both an encapsulating reality and a hermeneutic (i.e., self-reflexive and interpretive) circle from which we cannot escape. That is, whatever reality may be, it is only understandable to us as a human reality. Moreover, constructivists accept the inevitability that an interpretivist epistemological stance follows from this view of reality.

In contrast, Checkland and Scholes are guilty of a mismatch or inconsistency between their ontology and their epistemology. They are "sort of" constructivist and "sort of" subjectivist and therefore lack a coherent philosophical position on which to ground a methodology or a social/cultural theory that will serve it. By drifting into the dead-end of subjectivism, they introduce inconsistency and unnecessary limitations on SSM.

The flaw in Checkland and Scholes' thinking is their tendency to polarize subjectivity and objectivity as though these two zones were the only relevant sources

for understanding. If a phenomenon cannot be demonstrated to be objective, that is, producing brute data that can be publicly verified, it is *ipso facto* subjective and in principle unverifiable except by personal testimonial.

Philosopher Charles Taylor has thoroughly explored the consequences to human science of this polarization: it is to render social reality invisible by reducing it to either “brute data” (Taylor, 1979, p. 30) or to opinions and attitudes (ibid. p. 42). For example, take the phenomenon of language. It cannot be conceptualized as falling into either a subjective or an objective zone. It is subjective in the sense that human individuals can express unique personal experiences by making use of the malleable, undetermined aspects of language, such as figures of speech (rhetorical tropes). We can play with, invent, and ultimately change the meanings of the words we speak. Consequently, language evolves with use. On the other hand, language is not subjective in that it exists prior to an individual’s entry into the world. By learning to speak a language--that is, learning its unique rules, structure and vocabulary--an individual is to a great extent determined in his or her mode of thought, perception and understanding. Any language has both a subjective and an objective dimension, and it stands in neither zone exclusively. Rather, it is comprehensible only as an intersubjective phenomenon. Through language we not only come to understand a pre-existing social/cultural reality into which we are born by learning to take part in its practices, but our social reality is the shared practices constituted for us through language. Language is the glue of community. It demarcates the zone of shared experience. The constructivist link between language and social reality is described by Taylor:

“The situation we have here is one in which the vocabulary of a given social dimension is grounded in the shape of social practice in this dimension; that is, the vocabulary would not make sense, could not be applied sensibly, where this range of practices did not prevail. Yet, this range of practices could not exist without the prevalence of this or some related vocabulary. There is no simple one-way dependence here. We can speak of mutual dependence if we like, but really, what this points up is the artificiality of the distinction between social reality and the language of description of that social reality. The language is constitutive of the reality, is essential to its being the kind of reality it is. To separate the two and distinguish them as we quite rightly distinguish the heavens from our theories about them is forever to miss the point.” (ibid. pp. 45-46)

The common tendency to see our world only in polar terms of objectivity versus subjectivity is the source for a number of the problems that beset Checkland and Scholes’ theory of SSM: the weakness of their strawman argument between hard and soft perspectives; a truncated method for describing and analyzing social and political contexts which is inconsistent with their constructivist assumptions; their failure to see the necessity for a critical perspective on communicative situations; their undervaluation of, and failure to define, debate and discussion; and their frustration with efforts to find a way out of the box of relativistic judgments about value.

To conclude, the cognitive and epistemological demands of SSM, coupled with its weaknesses, suggest that if we are to have adequate methodologies for soft

systems research that will be competently applied by future researchers, then we have work to do. I take up this task in the final section, next.

Improving Soft Systems Methodologies

First, it is important for me to acknowledge the importance of Checkland and Scholes' work, although I have been critical of it. They have focused needed attention on the limits of traditional natural science methodologies when it comes to using them to study the human world of culturally produced artifacts, including culturally produced forms of communication and language. Furthermore, they have actually done something about those limits by pioneering new approaches developed and tested in the on-going stream of human action in a variety of organizational settings. They have extended our repertoire of concepts and means for systemically modifying human action toward purposeful ends. However, it is necessary to shore up several weak spots in the methodology if it is to be productively used to further our understanding of the dynamics of human systems. I have pointed out several of these weaknesses:

- * An unnecessary polarization of so-called hard and soft thinking;
- * A limited theoretical foundation for creating social and political descriptions and analyses of human systems;
- * Absence of a critical perspective for defining both ethical and valid action in the study of human systems interventions;
- * Too little consideration of the basis for validating claims to research findings;
- * And a conflation of the structures and functions of managing, consulting and researching in human systems settings.

As a way to improve SSM and go beyond its current limitations, I wish to make the following points:

- 1) If we view SSM as a subset of the larger class of methodologies commonly referred to as qualitative methodologies, we can benefit from a rich infusion of ideas about how to improve systems research. A great variety of qualitative methodologies has arisen from the same set of professional experiences that have driven Checkland and Scholes—frustration with the limits of experimental science to yield useful information about the human world. This work crosses the full spectrum of academic disciplines and applied fields that focus on the nature of human life, thought and action (See Denzin & Lincoln, 1994). Systems conferences provide one of the few truly interdisciplinary avenues for exploring such work, because systems theory is one of the few truly interdisciplinary approaches to understanding. Therefore, systems researchers are uniquely placed to contribute to multidisciplinary syntheses and integration of ideas and practical approaches to human science research. Just as human life bridges the conceptual gap between the sciences and the humanities, systems

methodologies can potentially do the same. This hope is the reason why I am attending this conference.

- 2) We need to have more open, and less contentious, discussion of the bases for comparing, contrasting and evaluating competing methodologies for doing systems work. A recent paper by Ledington and Watson (1998) reviews various efforts by a number of people so do this, and points out areas where considerably more thought and discussion are needed. Their concept of “paradigm interplay” suggests ways that this task might be conceptualized.
- 3) We need to pay more attention to issues of the internal validity of systems methodologies. Furthermore, we have to look with more sophistication at the links between theories, methodologies, epistemologies, ontologies and axiologies because dead-end discussions, popularly known as “paradigm wars”, frequently occur because of failures to explore these links. Specifically, we need to pay more attention to the concept of discourse, to criteria for sound interpretation of everyday human interactions, and to the consequences of the self-reflexivity inherent in *the study of human systems by human systems*. From this hermeneutic circle, there is no exit, but there is still good research to be differentiated from bad research.
- 4) We need more exploration and evaluation of the pedagogical strategies that we are using to prepare systems thinkers, researchers, and designers. Such exploration will need to focus more attention on the particular competencies that are needed for systems work. It is dangerous for the various kinds of engineers who work on physical problem situations to assume that human systems work is someone else’s preserve or for human systems specialists to believe they have no need for technical know-how. There is no system that is not embedded at some point in a set of human systems, either at its starting point where a problem situation is first experienced, or at its end point of application in real world settings. By the same token, there are not many human systems that are not also technology using systems.

Based on what I have presented about novice failures to adequately grasp what is required for research in human settings, I can state that systems students need more applied exercises in social observation; interviewing; facilitating group participatory action; recording field data; analyzing a great variety of data—both quantitative and qualitative; thinking both systematically and systemically; drawing valid conclusions based on evidence; and, a more literate education that can better acquaint them with knowledge of human history, as well as human accomplishments of distinction drawn from the multiplicity of cultures around the world. This is a tall order, but I have become convinced that there is no future in the popular definition of learning as “technique plus information”.

The complexity of the events and entities that we seek to understand points in the direction of a more purposeful, intentional evolution to increase our own thinking power so that we do not just have more to think about. We need to think more effectively by meeting complexity with minds capable of complex thought processes. Here I wish to invoke Ashby’s (1956) *law of requisite variety* to make a plea for meeting the complexity of the modern world by

committing ourselves to the development of a more nearly equal cognitive complexity in our conceptualization of it. Instead of trying to manage the world, we may have to do a better job of managing our own evolution.

5) Because the complexity of the tasks tends to overwhelm us, more consideration of structures that can facilitate cross-disciplinary teamwork is worth considering. The ideal of the lone researcher carrying out a narrowly focused dissertation study is probably anachronistic. A better model might be based on interdisciplinary teams with differentiated individual responsibilities and coordinating systems. One finds such arrangements in various entrepreneurial or special purpose centers and organizations, but seldom in a university where much of the training for research work is conducted. Using SSM strategies to produce purposeful action, change and improvements in universities around the world might become one of the most useful activities in which systems practitioners could engage!

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