Towards a Theory of Knowledge Systems for Integrative Socio-Natural Science

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Abstract

Knowledge system theory (KST) seeks to close the rift between hard and soft methods by reimposing the von Bertalanffy definition of a system while allowing that systems are epistemological constructs. KST is located within the broad thrust of western philosophy by association with classical rationalism, modernism and with the 20th century rediscovery of emergence. KST blurs the conventional distinctions of research management from research method and of science from metaphysics.

A research team is an appreciative system whose purpose is to create useful new knowledge within time and budgetary constraints. An undisciplined research team can destabilize the human ecosystems it is contracted to serve. There are ethical and pragmatic reasons for accepting certain disciplines. One way of avoiding these ethical problems is to study academic knowledge creation both as a source of practical insights that can be used to manage the research process more effectively and of theoretical insights that provide a wider understanding of cultural ecodynamics.

In general, the larger the group, the less it can be said to know. This suggests resolution into small workgroups with relatively simple and infrequent information flow between them. A provisional typology of knowledge communities and research problems is presented and practical advice for the organization of integrative research offered. This advice challenges the popular view that communication is best served by removing the boundaries between epistemic communities. Intellectual diversity is an asset that must be conserved.

Keywords: *integrative research, knowledge creation, modernism, innovation, cultural ecodynamics*

Introduction

A research team in Human Ecology is a manageable microcosm of the wider socio-natural world it is constituted to study. The team comes together to create and integrate knowledge that helps it cooperate in a complex socio-natural domain. That domain already contains other people (residents, if you will) themselves creating and integrating useful knowledge. The process of research and the subject being researched are qualitatively similar.

However, researchers often leave at the end of the project, but residents, one hopes, will not be forced to do so. Researchers have (or should have) well-structured aims and flexible knowledge bases; they can tune their knowledge to serve those aims. Residents often face weakly structured problems and may be unable to compromise without violating their own sense of identity or risking social exclusion. Their aims, broadly speaking, are to continue doing what they are already doing while achieving (or maintaining) a desired quality of life.

Researchers in applied or *policy-relevant* projects are there to reflect on the consequences of human behavior and maybe help people search for new, more sustainable lifeways. This is a tremendous responsibility. Researchers are willing participants in a process that determines the course of history. In a democratic society they should not have a disproportionate role.

Research is often directed across epistemic boundaries and one of the key tasks of a research coordinator is to coax colleagues into a more reflexive mode. Many academics do not enjoy this. Just as motorists sometimes curse and lean on the horn when other road users force them to take conscious control, so academics sometimes become aggressive when tacit beliefs are challenged. This may be acceptable in a pure research setting, but it raises ethical dilemmas when livelihoods are at stake. Tensions within the team may amplify stresses experienced outside. Some fragile rural ecosystems and deprived urban neighborhoods have been so disrupted by academics, residents have become openly hostile.

All human beings, whether they are conscious of it or not, are making history. Human knowledge (the shared beliefs of a community) determines human behaviors, which, in turn, impact on the biophysical environment creating opportunities and threats to which humans respond by renegotiating knowledge. These cultural ecodynamics are at once an important research domain for Human Ecology, and a valuable source of managerial insights.

Indeed, we can turn the self-referential nature of Human Ecology to good advantage. We can learn a lot about cultural ecodynamics without braving the ethical challenges of sociological or anthropological fieldwork simply by studying academic knowledge creation, both in the past and the present. A strong case can be made for equipping large research projects with a skilled participant observer whose task is to assist in the process of knowledge creation and prepare a research report on the process. Where a biologist may use a laboratory or greenhouse to test theories in a way that would be ethically indefensible in the field, Human Ecologists can test theories on themselves.

Cultural Ecodynamics and Appreciative Systems

Knowledge creation is a social activity. We may be astonished that Mendel discovered the principles of particulate inheritance and Leonardo da Vinci understood the cause of arterio-sclerosis, but these were never communicated to a wider community and had to be rediscovered in the 20th century. Clever recluses intrigue historians, but history itself ignores them. This is why, in the jargon of contemporary politics, the word *innovation* is not applied to the moment Archimedes got into the bath and had a bright idea about specific gravity. Innovation relates to the moment he shouted "eureka!" and ran naked down the road to launch it into the public domain. There is no innovation without communication.

Modern knowledge communities use a mixture of written and verbal communication to maintain and develop knowledge. However, as communities became global, the output of literature exploded and the impact of any given document became ephemeral. Like specks of plankton sinking into the abyss, books and articles that lie unread in the library stacks are the smudgy artifacts of old knowledge systems. Scholarship is the process of bringing those traces back into the light and forcing new life into them.

Knowledge is not data, scholarly literature or technical know-how. It is a living tradition: the shared beliefs that enable people to communicate clearly and cooperate effectively. There is no knowledge in a library, only texts. Knowledge is carried in and out by those who read and interpret these artifacts. Some philosophical arguments are of such antiquity they can be used as a backdrop against which changing patterns of discourse can be interpreted. One has to understand the substantive issues, of course, and humanists often provide a simple introduction in first-year lectures and tutorials before moving onto more interesting material, which deals with the way words are redefined and questions fade and reemerge through time.

Natural scientists, on the other hand, are less interested in communities of scholars than humanists because their primary focus is the problem itself. The community working on the problem is important, but secondary. In unpublished tutorials and lectures, students often get a whistle-stop tour of the history of a scientific field but this is background information. Between leaving school and starting their professional careers, natural scientists immerse themselves in a problem domain and develop a real working familiarity with problem-solving methods.

Consequently, a natural scientist and a humanist can be talking about the same issues, using the same words and yet fail to communicate because habitus leads them astray. The scientist wants to specify the problem and then solve it. The humanist wants to know about the social and cultural factors that conditioned the way the problem is restated and reinterpreted through space and time. Little flips and shifts in usage add interest to the narrative and are part of the fun.

Novices often get lost in this thicket of crossed purposes. Perhaps a natural scientist reading introductory textbooks finds some of the problems interesting. In the sciences, the only reason for specifying a problem in the opening chapter is that you will use the rest of the book to solve it. From the scientist's perspective, then, the humanist has done a poor job. Some scientists actually solve the problems, publish the results and are hurt when humanists rubbish their work. Alternatively, a humanist gets hold of popular quantum or chaos theory and realizes that, as a piece of philosophy or critical scholarship, what has been written is crass. One writes earnest papers explaining that all scientific knowledge is socially constructed. The only people impressed are other humanists.

People find it easier to communicate if they have common interests. This is why teleology is such a powerful scholarly tool. The trick is to look for coherence between words and interests. Named groups like "historians" or "revolutionaries," for example, resist teleological analysis because their stated beliefs and interests vary from place to place and generation to generation. The sense of continuity we get from the persistent name is illusory.

However, when people communicate effectively or use language in the same way they are probably acting harmoniously. One empathizes with the intelligence behind those actions and understands why people did what they did. Empathy is one of the most useful tools of critical scholarship. As the historian R.G. Collingwood explained:

> *You are thinking historically...when you say about anything, 'I see what the person who made this*

> *(wrote this, used this, designed this, etc.) was thinking.' Until you can say that, you may be trying to think historically but you are not succeeding.* (Collingwood 1939)

The work is harder if we know what people did but not what they believed (as in prehistory) or if we know what people believed but not what they did (as in much of ancient philosophy). For this reason an archaeology department often houses prehistorians taking a natural science approach and proto-historians who favor a humanistic approach. You will be confused and disappointed if you imagine archaeology to be a coherent intellectual discipline. Sometimes it feels more like a scholarly battleground. All becomes clear if you apply the teleological principle because you see that archaeology is an uneasy alliance of at least two knowledge communities, each with different beliefs and purposes.

This begs questions about where new beliefs come from, and the natural answer is that beliefs change when people's interests change. Under certain circumstances beliefs can be modified and the new beliefs may change the course of history, but only if they "strike a chord" with an influential sector of society and are widely adopted. When this happens, a new congruence of interest and belief will be manifest.

Communicating new beliefs requires that one party be trying to transmit information and others willing to receive it. Often this is not the case and an influential section of society will find its interests threatened by the new perspective. Rhetorical appeals to common sense, common knowledge and conventional moral standards are the most common defense mechanisms of an unreceptive audience.

Sir Jeffrey Vickers (1965) made a special study of knowledge dynamics in policy-relevant domains. Here it is reasonable to assume an awareness of political and ethical issues and to expect knowledge communities to act in a rational, responsible manner. Of course, it is not reasonable to expect unanimity or an absence of conflict. Vickers called these rational, reflexive communities *appreciative systems*, systems whose purpose is to negotiate and maintain shared beliefs about the world. These beliefs not only determine the way people behave; they can recreate the socio-natural world in a new form. In less than 60 words Vickers captures the spirit of cultural ecodynamics perfectly:

> *The sanest like the maddest of us cling like spiders to a self-spun web, obscurely moored in vacancy and fiercely shaken by the winds of change. Yet this frail web, through which many of us see only the void, is the one enduring artifact, the one authentic signature of humankind, and its weaving is our prime responsibility.* (Vickers 1965)

The Natural History of Knowledge

For those with an interest in historical matters, the teleological principle raises an interesting question: If, as I assert, people believe what it suits them to believe, why do so many hold passionate opinions about universal truth? Surely, they should temper their pragmatism with a little honesty.

A trite answer to this question would be that it is in their interest to believe in universal truth and in themselves as the custodians of that truth. This is one of the principal axioms of post-modernism, the idea that unitary models of reality and truth promoted by political and intellectual elites have locked human ecosystems into a self-destructive trajectory. This critique has much to recommend it, but has fueled a rather silly, tub-thumping debate about intellectual hegemony and analytical rigor that only demonstrates how completely communications have failed.

There is more to consider than the calculated self-interest of priests and politicians. Young people with dependent children have accepted a martyr's death rather than renounce deeply held beliefs and that obliges us to contemplate some unfamiliar and possibly shocking ideas about what a person's "interests" may be. From an evolutionary perspective, one might imagine interests that kill people would be eliminated by natural selection, but this is not so. All over the world people lead impoverished, foreshortened lives and suffer social exclusion or imprisonment for refusing to renounce what they believe are universal truths.

To understand the natural history of knowledge is to add an additional layer of detail to cultural ecodynamics. Just as the evolutionary constraints imposed by the vertebrate body plan interacted with environmental challenges caused ichthyosaurs and dolphins to develop similar morphologies, so the constraints imposed by our cognitive equipment, in conjunction with socio-natural challenges, produced epistemological resonances in the history of western thought.

Early western philosophy provides a good basis for exploring continuity and change in cultural ecodynamics. The picture becomes more complicated as the volume of literature increases. Plato and Aristotle were grappling with deep philosophical issues about the distinction of ontology (the branch of philosophy dealing with existence and reality) and epistemology (the study of how humans know).

Plato thought some concepts static and dependable, while the world of sensory experience was dynamic and unreal. He believed the static categories had an ideal, immutable template or *form* that existed independently of mundane experience. His philosophy, therefore, was inferential — philosophers must expect earthly experience to be misleading and try to apprehend the forms by a mixture of observation and disciplined contemplation. For Plato, forms were more real than things. Mathematical structures like numbers, for example, were not human artifacts; they were real and existed independently of human experience. If all the humans in the world were destroyed by some cosmic disaster, two plus two would still equal four.

Plato's ideas were very attractive to later Jewish, Christian, and Moslem thinkers. The neo-Platonic conception of God was as unlike those dissolute hooligans on Mount Olympus as one can imagine. Under Plato's influence, God became the author of a set of ideas or forms imperfectly manifest on earth. Humans could apprehend those forms through prayer and philosophy.

By the late 11th century in western Europe the realists were those who believed that human knowledge could be ontologically real — that it was humanly possible to make statements that were always true everywhere. When the evidence of our senses and our knowledge were inconsistent, it was knowledge and not evidence that had priority. The realist idea became a conservative force in religious politics and seems to have been justified with reference to Platonic formalism.

In western Europe at this time, much of Aristotle's work had been lost and he was seen as an interesting footnote to Plato. Nonetheless, a small, but vocal opposition to realism was influenced by his work on syllogistic logic. Their arguments focused on Aristotle's theory of universals — named classes or "species" of things whose existence was implied by the use of nouns like "man" or adjectives like "red." The latter, for example, implies the existence of a set of all the red things that ever were, are, or will be. Can we, who are trapped in space and time, be sure that this universal is real?

Those who denied the reality of universals were mediaeval skeptics. Probably the most famous of these was Peter Abelard, who irritated the dogmatic hierarchy and was driven into obscurity. After his death a more complete corpus of Aristotle's work found its way back to the West from the Crusades. When political tensions eased it became clear that Abelard had been onto something.

Skeptics found Aristotle's philosophy exciting because he distanced himself from Plato's belief that ideas existed independently of the substance of individual things. It was not disciplined contemplation, but science and reason that led us to a more complete understanding of categories — the species and genera of things. Aristotle provided a philosophical justification for reform.

Aristotle believed that, when statements or their corollaries appeared incoherent or contradictory, one or more of the statements must be false. Most of his logical research focused on the syllogism, which he privileged as the key to linguistic reasoning. Thomas Aquinas undertook the task of reconciling Plato and Aristotle and his moderate realism became the mainstay of scholastic theology.

It is not self-evident that Aristotle himself was a moderate realist. Indeed, Aquinas was possibly more interested in closing the rift between old-guard Platonists and Aristotelians than in historical veracity. However, his influence on western scholarship was enormous. The cracks in Aquinas's work did not really appear until the 14th century when resistance to Roman authority in England and the emerging Germanic states could not be contained. As in the early 12th century, the debate between skeptics and realists reflected political tensions between conservatives and reformers in the church.

As the Roman Church lost political control of parts of northern Europe, William of Ockham and others went back to Aristotle to recover syllogistic method. Ockham drew a sharp line between ontological reality and epistemological belief by adopting a nominalist position; arguing that universals were mere names and the classes they represented only existed outside the individual's mind by negotiation. As we would say today, universals are socially constructed.

Ockham was a rational skeptic who championed a pure form of Aristotelian reasoning that gave logic absolute priority in science but denied it any bearing on key matters of belief (Moody 1935). Skepticism and realism were philosophical tendencies that represented liberal and conservative tendencies in religious politics. Ockham was excommunicated from the Roman Church. However, Reformation scholars in northern Europe could have chosen between "ancients" (who followed Aquinas's model) and "moderns" (who favored the Ockhamite approach). The latter, of course, were modernists.

When the Reformation began to encounter resistance, the Protestant hierarchy lurched back towards biblical realism. Ockhamite philosophy was abominated by conservative Protestants and Catholics alike. Modernism survived as a diverse, iconoclastic, skeptical tendency sustained by the humanist Popes and wealthy patrons of the Renaissance. It became a secular movement. However, it did not lead neatly to what we now think of as "modern" science.

Western thinkers were imprisoned by the notational clumsiness of ancient mathematics. Ancient science could not handle dynamics and never progressed beyond ontological problems. Aristotle's complicated ideas about causality (reviewed in every good philosophy of science textbook) are irrelevant to the practice of science. His only lasting contribution was to the life sciences, particularly systematic biology (taxonomy), which is arguably the only contemporary science explicitly focused on ontological questions. Taxonomists still use a neo-Aristotelian terminology of species and genera in their work.

The development of algebra was such a significant step that scientists maintain a sharp distinction between the old (static, descriptive) and new (dynamic, predictive) approaches with the cusp somewhere between Galileo and Newton. It is hard to over state the importance of algebra and calculus in science. The new approach cleared a logjam of unsolved problems.

Scientists were no longer obliged to deal with accusations of heresy or argue about whether species and genera had a timeless, universal form that would exist even in a world from which all traces of them had been expunged. Ockham won that argument posthumously (Russell 1961) because scientists now had better things to do than argue with mediaeval crackpots. It was possible to boast of a great scientific enlightenment and a new line was drawn, this time between physics and metaphysics and, by implication, between science and humanities.

In the aftermath of so many religious wars, the enlightenment approach seemed a welcome return to sanity and common sense that would liberate humanity from the demands of dogmatists and scholastic theologians. To use an evolutionary metaphor, the attenuation of religious wars and the discovery of algebra coincided with the collapse of an old order in Europe. A host of cultural niches became vacant. This may have been the origin of C.P. Snow's "two cultures" — the "high" culture of the humanist tradition and the "low" culture of the emergent technical class.

As the two cultures began to contend in earnest, many scientists and engineers rediscovered the "reality" of human knowledge and the socio-economic benefits of appointing themselves its custodians. As the new elite took its place an astonishing amount of semantic slippage occurred. The word "realist" was redefined to mean, "One who believes the world of which our senses speak is real." This naïve realism makes nonsense of history and consigns the giants of mediaeval scholarship to the scrapheap. It turns the sadistic inquisitor Torquemada into an anti-realist, while Ockham, the skeptic who argued tirelessly against mediaeval realism, is reinvented as a realist. It is clearly a rhetorical gambit that allows people who question the ontological reality of scientific knowledge to be dismissed as idiots who believe torture is a figment of the imagination.

Philosophically speaking, these were elementary mistakes as Aristotle, Abelard, and Ockham had shown, but it was not in the interests of the new class to take ancient philosophers seriously. Naïve realists are clearly not modernists if the normal scholarly rules of priority are respected. Like Aquinas they are rational realists or antiquists. Unfortunately, most self-styled "post-modernists" have ignored the older literature, too, and entities now multiply unchecked. One can distinguish two broad tendencies within the post-modern movement: Those who deny the legitimacy of all scientific endeavor (relativists or irrational skeptics) and those who oppose naïve realism on philosophical and methodological grounds. The latter are neo-modernists trying to rehabilitate ideas about the independence of rational method and belief.

The Modern Way of Reasoning and the Boundaries of Policy-Relevant Science

Greek philosophers and mediaeval schoolmen tried to develop knowledge communities in which skeptics and realists could coexist. They favored a dialectic (question and answer) approach to knowledge creation that was intended to give reason priority over rhetoric. In practice, of course, there was always an audience inclined to keep score and the process often degenerated into bullfighting.

Nonetheless, at its best, modernism undoubtedly made room for diverse opinions, provided all those involved accepted the guiding principles of rationalism. Ironically, rationalism's strength as a guardian of disciplined diversity stems from the limitations we explored in the previous section. Logic cannot prove truth or falsity but can test beliefs for logical coherence. Any belief system that satisfies rational criteria merits consideration whether we find the ideas congenial or not. Furthermore, any logically incoherent belief system is flawed, common sense notwithstanding.

To be a rationalist is to believe that every logically incoherent set of statements must contain at least one false statement. This axiom, frequently misunderstood, is not equivalent to saying that every coherent set is true. Consider the statements: All birds can swim, Harry the haddock is a bird, Harry the haddock can swim. The set is internally coherent but hardly plausible.

Rational method can be used to test the consistency of any set of propositions. For example, if I believe that all swans are white, that the object at my feet is a swan and that it is also black, I know that one (or more) of my beliefs is false. Perhaps some swans are not white, the object at my feet is not a "true" swan or it is not "truly" black. Rational method does not tell me which statement is false but it does alert me to a flaw.

Testing for coherence is a two-stage process. One must first convert knowledge and beliefs into formal statements and then check for coherence. People often take the first step unconsciously, remaining unaware that the phrase "this is a black swan" is as much a statement of belief as "all swans are white."

Karl Popper (1959) made this error when he suggested that the difference between a science and a non-science was that science used the particular observation to refute the universal generalization. That this was a mistake can be demonstrated using Popper's own method. Consider the universal statement: "Science is the investigation of falsifiable propositions," and the particular "comparative anatomy is a science."

Popper had to resort to special pleading when confronted with mainstream scientific activities that violated his general rule.

Rationalism is much weaker than many imagine because it is usually impossible to express a non-trivial belief as an unconditional assertion. Illustrative examples can be found in any textbook of artificial intelligence. Consider, for example, the statement: "All mice have tails." How many tails? If every mouse has one tail, what can we infer from this about the statement: "All swans have feathers"? How about: "Every fish has a tail." Does this mean that there exists a tail which every fish has?

Even simple statements contain implicit references to common knowledge and their truth is contingent on that knowledge. As Popper explained, exhaustive checks for logical consistency lead to infinite regress. If every mouse has a tail, what is a tail and what is a mouse? If a mouse is a furry rodent, what is a rodent and what does it mean to be furry? Every definition contains adjectives and species names and these have to be defined. Soon the process of checking explodes beyond reasonable limits and we have either to accept that non-trivial beliefs have hidden contingencies or get lost in endless discussion.

However, rationalism is also much stronger than many of the relativist critics of scientific method believe. Relativists are those who believe that there are no universal truths, only opinions constructed within some cultural context. A rationalist, however, cannot possibly agree. The proposition "no statement is true" is a statement about statements and rationally incoherent. If it is true, it is necessarily false, therefore it is false. No competent rationalist is a relativist. This simple reasoning trick, based on Epimenides's paradox, sets a natural limit on policy-relevant science. Relativism lies outside that boundary because it is rationally incoherent.

Although this paper is written from a scientific perspective, I do not condemn relativism or champion science, but rather wish to present the post-modern debate to you in the form of a dilemma:

- The modern way of reasoning conserves disciplined diversity by giving reason priority over ideology. To modernists (physicists and metaphysicists alike) relativism seems like a return to the Dark Ages — a world where the triumphs of science are denigrated and there is nothing between dissidents and the rack but rhetoric and good intentions.
- Relativists recognize that rationalism has nothing to offer them and so give ideology priority over reason. The triumphs of science have been overvalued and rationalism has sold out to rhetoric so many times, it has shown itself an unworthy champion of dissident rights.

Thesis and antithesis are both consistent with the evidence: modernism has failed *and* succeeded, science has had its triumphs *and* its disasters. I can find no synthesis. Go one way, you are a scientist. Go the other, you are a relativist.

Realism and Policy Relevant Research

A similar rational analysis can be used to evaluate the relationship between realism and policy-relevant science. Consider the proposition: "It is possible for humans to change the course of history." This axiom is not self-evidently true. It is certainly conceivable that we live in a clockwork world where every event and process is predetermined and nothing we do can change what is to happen, but it is hardly in the interests of applied scientists to believe this. When we apply rational principles to this axiom we can prove that some statements, though true in practice, are not necessarily true — they may be logically unconnected to any set of true axioms at our disposal.

This seems a little abstract, but is quite easy to illustrate. Suppose two people, X and Y, are playing a game of "scissors, paper, stone" and have a shared knowledge base that allows them to deduce what one of them (X, say) will do. Y can deduce when X will choose "scissors" and choose the "stone" that blunts them. But X could use the same knowledge to deduce his own bid, infer Y's counter-bid of "stone" and choose the "paper" that wraps it. However, if X does this, Y's deduction led to a conclusion that was not consistent with observation and the axiomatic basis of that deduction must be flawed.

This argument, which is loosely based on a passage in Aristotle's *Posterior Analytics*, leads naturally to *Jonah's law* (Winder 1999) which states that humans can only predict the course of history if no human action can change it, and can only change the course of history if our predictions are irreducibly uncertain. This is so because any prediction we make has a hidden contingency: "X will choose 'paper'" (provided no one uses this prediction to change the course of history).

Jonah's law explains why *a posteriori* and *a priori* views of history feel so different. As we look into the past we see a chain of events leading inexorably to our present socio-natural configuration. When we look forward, however, we see many possible futures manifest in a host of unresolved contingencies.

For a rationalist engaged in policy-relevant research, Jonah's law is highly significant. Every prediction we make about the likely effect of policy instruments has a logically irreducible uncertainty. This applies, for example, to predictions about the likely impact of carbon taxes, treaties to control emissions, the relative importance of socio-natural adaptation or mitigation strategies and so on. When our predictions go wrong, therefore, we have to ask ourselves whether this is because we got the science wrong, or whether it was simply the effect of Jonah's law.

It is possible to make the predictions of policy-relevant science empirically testable, but there are no objective rules. Instead, a knowledge community has to negotiate the standards by which a theory is judged. These standards are socially constructed. Often, when scientific theories collapse, they do so not because new evidence comes to light, but because the knowledge community has reset the standard.

Changing consensus can lead to the scientific revolutions and dramatic "paradigm shifts" so ably described by Thomas Kuhn (1962). In these contexts, issues of self-interest are at least as significant as scientific evidence. Although theory testing can often be made quantitative (an interesting approach developed as part of the adaptive management of natural resources is described in Walters 1986), even mathematical approaches cannot be made objective. Sooner or later a community has to decide whether the discrepancy between observed and predicted behavior is significant, and that decision is always informed by self-interest. The standards set for deciding whether a new drug is safe to release are very different from those that were set for deciding whether tobacco sales should be restricted. We will return to the issue of empirical testing after we have explored the concept of a knowledge community in more detail.

Disciplines and Communities

Science has brought unprecedented prosperity in the West, but this has coincided with a growing fear that our collective life support systems have been compromised. Many people look to the human and natural sciences for solutions and are disappointed with what they see.

Over the last few centuries scientists and humanists have formed factions that argue interminably about differences of emphasis, method, philosophy, and belief. Even such words as "science," "academic," or "intellectual" have different meanings for different people and in different countries. The phrase "human and social science," increasingly heard in Europe, sounds perfectly natural in France where a post-modern anthropologist can also be a scientist, but jars on the ear in Germanic countries and North America where the boundaries between the humanities and the sciences are sharp wedges riving the fabric of university life.

There are academic factions wherever there are universities but disciplinary boundaries are in different places in different countries. It is difficult, therefore, to resist the conclusion that some factions are functionally irrelevant and the boundaries between them only exist so academics have something to hide behind when they snipe at each other.

To many Human Ecologists, particularly those at the start of their careers, the boundaries between epistemic communities appear as obstacles to empowerment or even as part of a conspiracy to sustain the status quo. However, the situation is more complex than this. Experienced socio-natural researchers are impatient with disciplinary infighting too, but are seldom cynical about the boundaries between intellectual communities. Academic disciplines and intellectual communities are different.

Disciplines are administrative inventions. The boundaries between history and classics, chemistry and biology, sociology and anthropology, for example, are locally permeable and many people cross them freely. The boundaries between intellectual communities, however, are cultural, not administrative artifacts and much harder to traverse. It is often very difficult to get pure and applied mathematicians to work together. Bayesian and frequentist statisticians may have terrible problems getting onto the same wavelength. Similarly, taxonomists and biochemists, biological and social anthropologists, physicists, and humanists often experience genuine problems of communication.

The difference between a discipline and an intellectual community corresponds, in broad terms, to that between etic and emic distinctions as drawn in linguistics, anthropology, or sociology. An etic distinction is pragmatic and imposed on a social group from the outside. The distinction of biology from chemistry, for example, is an etic distinction because it is focused on external observables at the expense of personal motivation and beliefs. An emic distinction, however, may be very difficult to make on external criteria alone. It calls for a teleological analysis that takes account of an actor's social environment, explicit and tacit beliefs. It must also interpret those beliefs in the light of behaviors that give them significance or value to the actor.

Research that crosses disciplinary boundaries is often straightforward, but integrative research that crosses knowledge communities is very demanding. Knowledge communities are often logically unconnected and even incoherent in relation to each other. Even reconcilable knowledge communities may address the same issues on different spatial and temporal scales.

For example, a political geographer interested in inter-urban migration must treat cities as identifiable types of things and model migration processes in terms of distance between cities and some measure of migratory "attractivity." This is a very mature body of modeling method that originated with the work of E.G. Ravenstein in the mid 1880s. The models are often called "gravity models" because of their resemblance to methods used in physics. Some of the more sophisticated approaches borrow from non-equilibrium thermodynamics to ensure that the microscopic uncertainty imposed on the system by stochastic processes operating at the individual level is adequately represented. They work very well on a macro-scale (Sanders 1999).

However, policy-relevant research on a regional scale often demonstrates consistent mismatches between prediction and reality. These discrepancies are easily explained in terms of local circumstances; established trading links between cities that facilitate trade and migration between them. You cannot deduce those meso-scale factors from the macro-scale model (they are contingent on localized accidents of history). Moreover, you cannot quantify meso-scale effects reliably unless you "partial out" the effects of macro-scale processes and you cannot deduce macro-processes from meso-level observations — the two are logically unconnected — both explanations are possible in the same universe of discourse, but neither is contingent on the other.

When policy-relevant research comes down to small communities, another set of patterns emerges. Once again micro-scale insights do not connect neatly to meso or macro-models. The likelihood that an informant will say: "I specified the equations of motion for an individual actor, constructed a master equation model, made allowances for the traditional trade-links between each city and all its neighbors, and deduced that Clochemerle-Les-Bains was the place for me" is not very high. Once again, micro-scale processes are contingent on localized accidents of history that meso- and macro-models cannot explain or even represent.

The logical unconnectedness of these knowledge domains does not prevent Human Ecologists making use of them. If we need to explain patterning at the meso-level, we call in a regional geographer. If we need to know about very local events, we call in an anthropologist or sociologist. But the knowledge does not form a neat axiom system from which we can generalize. We just set these insights alongside each other, check them for logical coherence and take such account of them as seems appropriate in the circumstances — tailoring policy instruments to local circumstances.

Logical unconnectedness imposes a significant burden on a scientific team because it must monitor processes at a wide range of spatial and temporal scales. Each of these scales is the specialism of a different knowledge community. Decisions about what spatial and temporal levels will be investigated are pragmatic. Moreover, accidents of history do not stop happening simply because there is a group of scientists in town. Even the best policies will have unforeseen and possibly undesirable consequences (by Jonah's law) and must be monitored and adjusted continually. A policy that works well on a macro-scale may be a disaster on the meso- or micro-level.

This being so, Human Ecologists must be a little careful about breaking intellectual boundaries down. There is a danger that the over-zealous destruction of boundaries will turn Human Ecology into a reactionary omni-science in which intellectual diversity is suppressed and the undoubted advantages of working simultaneously on many scales will be lost. We need to be bold, but not too bold, intolerant of rhetoric and respectful towards genuine intellectual diversity. This is not merely an ideological statement. It is a pragmatic judgment consistent both with practical experience and rational analysis.

Some Definitions for Knowledge System Theory

In this section and the one that follows, I introduce some definitions of terms that describe the operation of an appreciative system. Most of the key ideas have already been outlined, but here I wish to formalize them. My reasons for doing this are two-fold:

- To put a little distance between my usage and that of other authors, I am using familiar terms in a slightly unfamiliar way to encourage readers to reevaluate their own tacit knowledge. This is particularly true of my use of words like "complexity," "openness," "boundaries," and "information" which would not be endorsed by all systems theorists.
- Knowledge system theory may have mathematical applications. I have no intention of developing a formal mathematical model here, but this will serve as a positioning paper for future work.

Humans negotiate knowledge by communicating with each other. We seem to be programmed to try to understand sensory experiences, especially those associated with other people. Where other great apes build trust by grooming, humans do so by talking and listening. We use our highly developed empathic skills to negotiate a congruence of beliefs with our neighbors. Young people are more receptive to unfamiliar beliefs than older people so linguistic structures are conserved and transmitted between generations. Errors are made and ideas communicated imperfectly in this process. Knowledge is not static.

Human beliefs are conditioned by basic human needs. Certain types of situation recur, and humans can be expected to gravitate towards similar beliefs in those situations. These common interests create consistent associations of personal interest and shared belief. If circumstances are persistent, linguistic and epistemological structures will be conserved from generation to generation. However, if they disappear and then return, the same structure will be expressed in very different terms. These epistemological resonances (ideas) can indeed be apprehended by disciplined contemplation. However they are not divine templates that exist independently of human belief, but normal cognitive responses to recognizable types of circumstance.

Knowledge is shared belief. My personal knowledge consists of all the beliefs I share with myself. Yours is defined similarly. Our (collective) knowledge consists of all the beliefs we share. Our beliefs are shaped by our experiences. As your beliefs or mine change, the knowledge we share re-forms at the intersection of our respective belief-sets. This is so by definition and can be extended to communities of three or more people without loss of generality. If beliefs diverge, trust can break down and social exclusion results. This is a common experience among the children of blue-collar workers in higher education, for example.

Observations are sensory experiences articulated with prior knowledge. A lot of sensory stimuli are systematically ignored or baulked. Sometimes we baulk observations because we do not know what to make of them, or they challenge cherished beliefs. Often we baulk experience that seems insignificant. For many people routine journeys are forgotten within seconds of arrival, for example.

Information consists of observations that change knowledge. Some information is consistent with preexisting beliefs and simply adds knowledge to the store. Other information challenges preexisting beliefs. Information that challenges belief is called *innovation* (Hägerstrand 1953) because it changes people's understanding, expectations and behavior.

In the political science literature, the word "innovation" is often used to represent socio-economic change. National and supra-national investment in research is often funded because people believe activities that add new knowledge can be expected to enhance economic performance. The evidence does not support this view, as debates in the U.S. about the future of space research and in Europe about the low level of economic spin-off from successive Framework Programs have highlighted.

Rapid economic development seems invariably to follow innovation, and innovation often arises from research. However, many research projects do not innovate and most innovations have no economic spin-off. It is possible to manage research projects so as to maximize the likelihood of innovation and, once an opportunity has been recognized, to design technical projects that exploit it. However, it is often impossible to move from innovation to exploitation on demand. It takes time for new beliefs to be communicated to and assimilated by an influential group of people (Hägerstrand 1988). Notwithstanding the protests of politicians and scientists with a vested interest, the dynamic linkages are weak.

Innovation implies relative, not absolute novelty. What matters is that the information changes the recipient's mindset. Swamping the recipient with data dulls the senses and may actually reduce the likelihood of innovation. The distinction is that between "*So What?*" and "*Aha!*" When I tell you that scientists have found a blade of grass growing between two paving stones, you may baulk this as a useless or trivial communication. *So what?* If, for some reason, you were interested in these paving stones, you might note the presence of grass but it would not challenge your beliefs.

However, if I were to tell you that scientists had found a blade of grass growing on Mars (and you believed me) your beliefs about life on Mars might be challenged. The moment that challenges belief is seldom repeated in a simple way because knowledge is dynamic: yesterday's *Aha!* is tomorrow's *So what?*

There are many *Aha!* moments in the first decade of life, rather fewer in the seventh. As we become elders of the communities that sustain us, our beliefs cease to be monitored and knowledge becomes fixed. We baulk experience that might lead to innovation and switch from a learning into a teaching mode.

Teachers often try to challenge beliefs, but the graveyard slot in the early afternoon, often finds students unreceptive. Half of them are asleep; the rest are daydreaming (technically awake but operating with too many cognitive filters off-line to make sense of anything). We complain of course, but suspect that adult creativity is nourished by such childish joys as watching the summer grass grow, or dust spinning in sunlight. If humans couldn't lower their cognitive filters from time to time we would be trapped by our beliefs and unable to observe anything that did not make sense immediately. Many people can testify that childhood experiences often lie dormant in memory until the penny drops and we make sense of them as adults. Such memories are not always traumatic (repressed, in the Freudian sense) they are merely uninterpreted observations.

It is helpful to contemplate a spectrum of belief from culture to theory. Cultural Beliefs are so deeply engrained in us that we do not monitor them. In practice, culture is best identified negatively in terms of the things we do not think of doing and the observations we are not capable of making. When we receive information that challenges cultural beliefs, we often respond defensively because culture defines our identity. People who challenge cultural beliefs are always mistrusted and often disliked. Cultures are usually closed to information.

Creedal Beliefs are deeply embedded but explicit. Information that challenges creedal beliefs is often baulked. Theories are a weaker type of belief, created in provisional form and monitored for coherence, consistency, and utility. We often depend on theories in our daily work while recognizing that they are not secure. Information that challenges a theory is less likely to elicit a hostile response. People are open to information that challenges theories.

Beliefs enable us to make sense of our experiences and cooperate with others by forming coherent knowledge communities. Different knowledge communities often have logically irreconcilable belief systems and members vary in their openness to external information by age and temperament. Integrative research requires representatives of two or more communities to cooperate, but they can only do so if they are prepared to make temporary compromises and learn from each other. The people best qualified to represent a knowledge community are usually mature, but mature people are not necessarily those best equipped to learn and compromise.

Problem-Oriented Research and Empirical Testability

A well-posed problem is a problem whose solution exists and is unique. A problem can only be declared well- or ill-posed with respect to a definite belief system: we need axioms to decide whether a problem is well-posed.

When we choose axioms, we split the world into a well-understood core (the axioms and their corollaries) and an ill-understood periphery. We do not deny the existence of factors beyond our understanding and control, we simply reduce them to a source of exogenous noise that we cannot, or cannot be bothered to consider. We handle these noisy factors by negotiating boundary conditions, a set of rules that allows us to specify a knowledge domain within which the problem at hand appears well-posed. Any solution we propose to this problem is contingent on those boundary conditions.

For example, we may agree to predict the behavior of an economic sector subject to the assumption that buyers make decisions that optimize cost-benefit ratios. Of course, real people do not always do this, so we set boundary conditions in the form of thresholds. If, in practice, real buyers make decisions so far from those that optimize cost-benefit that the boundary conditions are violated, we can no longer depend on the model's predictions. However, as long as the boundary conditions are satisfied, we may feel justified in using the model as a first approximation of our beliefs about market behavior.

Of course, my boundary conditions and yours may not coincide. Perhaps cost-benefit optimization theory is culturally embedded in me, or I believe the solution is unlikely to be effected by slightly sub-optimal behavior, while you think the method very sensitive to sub-optimal choices. If so, I may continue using the theory long after you consider it discredited.

One can easily see how Kuhn's paradigm shifts are represented in system theory. Boundary conditions are set by an (often implicit) consensus, but the knowledge community is not static: people reconsider their beliefs, die, or retire. As a result, consensus may change quite rapidly, boundary conditions are revised and an established belief is suddenly abandoned. This happened in biology, for example, when the boundary conditions of biblical creationism shifted and people realized humans had been on the planet for much longer than had previously been thought.

When boundary conditions are violated, emergent information forces people to innovate. This conception of emergence is slightly non-standard and demands a definition. A phenomenon is emergent if it is not logically entailed by the axiom system we have selected to tackle a given problem. Having defined emergence, the notion of complexity follows naturally. A complex world is one in which emergence is possible. In a complex world, boundary conditions are explicit and actively monitored, so that knowledge domains are open and innovation can occur.

The relationship between complexity, innovation, and the openness of a knowledge domain is significant. When a complex system receives information that violates boundary conditions, it innovates, changing those beliefs. Since boundary conditions are socially constructed, it follows that *complexity must be socially constructed too*. When a system is declared complex, the knowledge community studying it has taken a skeptical position in respect to its own beliefs. To the best of my knowledge and belief, the only communities that do this routinely are those committed to rational methods. Modernists (rational skeptics) seem to have an affinity for complexity.

Applied mathematicians and natural scientists often confound emergence with self-organization. Self-organization occurs when micro-scale behavior (at atomic or cellular levels, for example) produces spatial or temporal patterns on a macro-scale. Some patterns are emergent; the precise form of a snowflake, for example, is unpredictable. Others, like the sound waves caused by air vibrating in a tube, are highly predictable. The latter are autopoietic (literally "self-writing") patterns. Some emergent patterns are simple. Bénard or convection cells, for example, are just circulating blobs of liquid (Nicolis and Prigogine 1989). Autopoietic structures like the human body can be very intricate indeed. Complexity and intricacy are not autocorrelated.

The physicist Ernest Rutherford used to joke that all science was physics or stamp-collecting. He was a reductionist who believed all problems could (in theory, not in practice) be solved in terms of Newton's laws. One could, in theory, predict the GNP of Albania from Newton's laws and the positions and momenta of all the atoms in the universe. However, by the time Rutherford retired (1937) developments in quantum mechanics had forced physicists to acknowledge emergence. Problems involving the position and momentum of small particles were ill-posed; their solution, if it existed at all, was certainly not unique. Biologists and historians had been aware of emergence since the mid-19th century but the idea really caught on in the late 1920s and 30s.

Jonah's law and Gödel's incompleteness theorem to which it is related (Chaitin 1982) are local manifestations of a growing consensus that every finite axiom-set is too weak. It seems problem domains as stable as Newtonian physics are exceptional. We can no longer think of science as a Newtonian puzzle — laying down axioms by induction from empirical observation before using deduction to join up the dots and make a picture. The dots are the picture; a picture that swirls and re-forms as knowledge changes.

Science has become an odyssey — we navigate against the fixed stars of logic and mathematics, not merely manipulating events and speculating about causes, but influencing the very fabric of reality. Engineers, physicists and chemists prefer quiescent regions where the laws of cause and effect are clear. Biologists and social scientists usually hover close to dynamic regions, studying complexity on the margins, as it were. But humanists and socio-natural scientists fare through the most dynamic regions of all, trying to understand and even facilitate processes in the vortex of history.

Many attempts have been made to develop formal methods for handling such complexity; among the survivors, the mathematical theory of dynamical systems, Operational Research, Cybernetics and General System Theory are significant because practitioners crossed the boundaries of the social, life, and physical sciences. By the 1960s and 70s this was sexy science and, under generous funding regimes, coalesced into a more or less coherent methodological tendency, the systems movement.

Following Ludwig von Bertalanffy (1968), I define a *system* as a set of components in articulation. This definition is sufficient to underwrite all the mathematical and engineering systems methods used by the systems community. It is easy to convert a Bertalanffy system into a formal mathematical model or a computer simulation. Moreover, the classical definition can be used to motivate applications in the human and natural sciences.

Such a system can be represented without loss of generality by a Venn diagram that defines a set with a number of subsets and lines between them indicating axes of interaction. Every complex system has an axiomatic core that is open to perturbation across its boundaries. Within the core, all behaviors are deterministic, and all problems can be solved (at least in principle). Simple Venn diagrams can often be used to represent complex systems. Complexity (sensu stricto) implies that the system is open, i.e., subject to external perturbation, not that there are lots of rings and lines on the diagram.

Emergent behaviors are investigated by manipulating boundary conditions to see how sensitive the core solution is to external contingencies. Computer simulations are often very useful here. This approach sometimes enables us to specify problems that are almost well-posed.

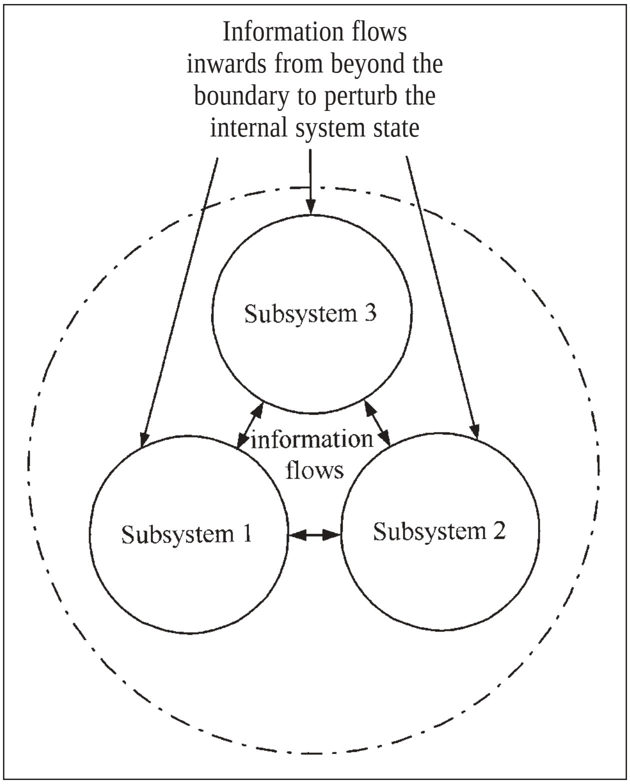

Figure 1. Venn diagram representation of a complex (i.e. open) system with three subsystems.

Science as the Rational Pursuit of Knowledge

Almost all the early applications of system theory assumed systems were ontologically real and their behavior time-invariant. However, after World War II, many system theoreticians became interested in managing human activity systems and ecosystems, so called soft systems. This paper has been influenced by Beer (1979), Boulding (1978), Checkland (1993), Churchman (1979), Flood and Jackson (1991), and Lee (1993), but there must be many others. Soft systems are not real because human beliefs condition human behaviors, which, in turn, change the systems themselves. In a world without humans they would not exist.

Human beliefs are key determinants of human actions and human actions, in turn, determine many of the properties of our natural and physical environment. The banking system, for example, is critically dependent on human consensus. If a group of economists constructs a model that changes the way people think about banks, the banks themselves may change to reflect this consensus.

The principal impact of soft system work has been to shift attention from the use of mathematical models to multiple perceptions of reality. Soft systems ideas have had more impact on the policy-relevant and management sciences than on the natural sciences and engineering. The systems movement is no longer a coherent methodological tendency. A schism is forming with one group becoming increasingly preoccupied with non-linear dynamical systems and the other more qualitative and discursive in approach. Many people are actively trying to bridge that gap, but extreme positions seem to be unreconcilable.

All system theorists would accept that the Venn diagram in Figure 1 could represent a system of some sort, but hard and soft theorists have different conceptions of a system. In hard system theory, Figure 1 is an abstract representation of an ontologically real system whose existence is independent of human knowledge and belief. In soft system theory, however, Figure 1 is a concrete representation (a picture) of an abstract system, our knowledge or shared beliefs about the world. The planets, moons and stars we think of when we speak of the "solar system" are real enough, but are not a system — a (soft) system is an epistemological construct.

Soft system theory, like Human Ecology, is self-referential and so abandons the comfortable boundaries of 19th century reductionism. When systems were ontologically real, scientists could present their work as a materialistic quest for timeless, objective truth. Once we acknowledge that all systems are knowledge-systems, however, the scientific subject and the ontological object depend upon each other.

Hard and soft methods are both part of an appreciative process that spins the web of beliefs that sustain us, yet they feel very different. In hard science the appreciative process has been temporarily suspended while scientists analyze a fixed belief system or theory. Their purpose is to still the babble of debate and use mathematical methods to solve an almost well-posed problem (AWPP).

In soft science, the discursive process continues unchecked and the state of knowledge changes while the work is in progress. Many soft scientists understand that systems become dysfunctional when multiple perceptions of reality and conflicts of interest are ignored or suppressed. They are skilled at helping people move from an ill-specified sense of unease to a more coherent understanding of socio-natural constraints and, occasionally, to negotiate a common purpose and specify an AWPP.

These approaches are clearly complementary; each has its particular strengths and weaknesses. However, if hard or soft methods become culturally embedded, researchers end up believing that colleagues are acting irrationally and cooperation becomes impossible.

Many hard scientists are hostile to the idea of social construction and argue persuasively that mastery of soft system method would not make them more effective in practice. They deal with a problem-set in which the assumption of reality causes no practical difficulties. Humans can launch space probes or mix chemicals, but we cannot change the gravitational constant or the reactive properties of hydrogen. Jonah's law suggests hard scientists may reasonably make predictions without worrying much about the social construction of knowledge.

Conversely, many soft scientists mistrust hard science method and see no need to understand it. They can only justify reality assumptions in situations where humans are operating under stress and obliged to adopt a common purpose (an integrative research project with a tight deadline, for example). Such circumstances are rare. Indeed, human scientists sometimes find themselves in situations so fluid that simply to have a researcher taking notes in the corner can change the course of history. They are not going to use hard science method in these contexts or take anyone seriously who does.

The case for a truly general systems approach is only strong in integrative, policy-relevant research where natural and human scientists join forces to work across intellectual boundaries. The minimum requirement for participation in this work is an acceptance that science is not, as naïve realists believe, a personal quest for objective truth. Science is a social activity, the rational quest for knowledge.

Under this definition, the domain of socio-natural science is remarkably broad. There is plenty of room here for mathematical case studies and participant observation. Only very extreme positions are excluded. Within this domain it is possible to recognize three knowledge communities, each distinguished in terms of their approach to AWPPs.

- *Reductionists* believe that research is the process of moving from a specified AWPP towards a definitive solution. Some engineers, neo-classical economists and technologists belong to this reductionist genus.
- *Constructionists* are theoreticians for whom research is a device for moving towards a defensible AWPP. Experience shows that many engineers, neo-classical economists, and technologists find this process tedious and do not wish to participate, though some social or political scientists and systems thinkers find it stimulating.
- *Deconstructionists* believe that any attempt to formulate an AWPP is ethically or intellectually problematic. The researcher's task is to provide evidence and criticize or comment on interpretations, not to synthesize or generalize. Many critical humanists, emancipatory system theorists, and scientific empiricists fall into this category.

The three communities are epistemological resonances in the history of western thought; they can be recognized as coherent associations of interest with beliefs that transcend linguistic and temporal boundaries and have been discovered and rediscovered many times. Critical humanists and scientific empiricists, for example, use different words to express their beliefs, but the roles they embrace in research teams are strikingly similar and based on a very similar ideology.

Each community has special strengths. Constructionists formulate new AWPPs and develop new ideas about the way the world works. Reductionists implement — they convert the AWPP into a procedure for exploiting the new knowledge. Deconstructionists evaluate — by resisting generalization, they remain alert to the dangers of over-simplification and continually monitor boundary conditions.

Managing Intellectual Diversity

A knowledge system (k-system) is a formal map of the knowledge communities contributing to a research activity. The prefix k- indicates that these systems are not ontologically real entities, but represent tasks requiring expert knowledge. This can be a great liberator in integrative research. Many people who question the ontological reality of classical systems participate freely when it is explained that k-systems are domains of expertise and interest.

Sometimes a k-system represents the knowledge domain of a research project and we populate it by recruiting people to represent interested knowledge communities. They are subdivided into k-sub-systems, each of which represents a recognizable theme or program of work. These k-sub-systems are often called *workpackages*. Some people serve on more than one workpackage; others specialize. Workpackages are often subdivided thematically into k-sub-sub-systems or *workgroups*. Workgroups are typically small and rather coherent in belief and purpose.

Recall that knowledge is shared belief. This means that as one moves up the system hierarchy from the workgroup to the project, the number of people working together increases but the amount of knowledge decreases. K-systems are like the biblical tower of Babel. The higher we climb, the harder it is to communicate and the less we know. Scientists can never construct a God-like omni-science, but, with careful design and thought, can sometimes get a useful picture of the world. However, every time one adds a level to the integrative hierarchy, or increases the size of a k-system, one makes the work of integration harder.

Clever research design can help, but there are structural constraints on knowledge creation that we ignore at our own risk. In practice, people have to accept some discipline to work in groups. Many students speak confidently about the obvious advantages of inter-disciplinary research and believe that high fliers are those who avoid getting corralled in narrow knowledge domains. These melting-pot ideas do not work very well in practice because many knowledge communities are logically unconnected. You actually need distinctive perspectives to get a clear understanding of a human ecosystem on many spatial and temporal scales.

If you really need to see across knowledge boundaries, you can do so and what you see can change the world, but you do not fly, you build. Knowledge systems are founded on trust, mutual respect and common purpose. If they are well designed people can move from one vantage point to another, speculating, theorizing, transmitting, and receiving information. This does not mean that researchers must discard all the specialist knowledge that cannot be transmitted across community boundaries. It merely requires them to think carefully about the signal to noise ratio when communicating with colleagues. You must take possession of (and responsibility for) your own specialism and communicate sparingly.

In recent years it has become fashionable to assume that the most useful research into socio-natural science is always integrative. However, both theory and practice suggest this is not so. Governance structures are k-systems too and the larger they are the less they know. They usually compensate for this by becoming increasingly sectorialized as one moves up the hierarchy from local to supra-national. National and supra-national agencies are constitutionally incapable of responding to integrative science. In practice, the deepest knowledge hierarchies are usually constructed by small teams working to inform policy on a micro- or meso-scale.

Although position in the knowledge hierarchy should not be equated with status or merit, there is little doubt that inexperienced researchers should spend most of their time working near the bottom. Younger researchers need to learn and this is the level at which knowledge pictures are richest and information flows very easily. A person cannot contribute meaningfully to integration who has not accepted the discipline of becoming well-educated and this is the best place to get that education.

By well-educated, I do not mean holding a lot of data, but having absorbed the core beliefs of a knowledge community and spent enough time applying that knowledge to have developed a mature intellectual position. Researchers need that education before they can serve effectively as ambassadors of a knowledge tradition. This is especially true if the work involves non-academic outreach. If you claim to know nothing, but seem to have opinions about everything, non-academic residents are not likely to take you seriously.

As scientists our task is to construct a strategic alliance that capitalizes on perceived strengths without compromising the rational coherence of the whole. In practice, this means that people (and k-sub-system boundaries) must be organized pragmatically and everyone must expect to pick up a reasonable share of the most difficult tasks. Rich information flows must be possible (and needed) within a workpackage or workgroup, but relatively poor and infrequent information flows suffice across boundaries.

Wise designers remember that one person's information is another's background noise. An engineer designing a sewage treatment plant may be bored or irritated by questions about the social construction of meaning. Restricted information flows between workgroups so that the only messages passing are simple and potentially interesting to the recipient. Failure to constrain information flows can stifle innovation by forcing recipients to baulk unwanted communication. Stafford Beer's (1979) viable systems research applies cybernetic theory to these processes in an interesting way.

Once information constraints are in place, people can focus their energy on tasks involving easy communication. Endless culture shocks are exhausting and the irritation they engender in a research team may destabilize the human ecosystem it is to study. Of course researchers must come together to plan and integrate. An effective way of managing this is to arrange small milestone meetings for non-routine information flow between sub-systems.

The heart of this process is an appreciative cycle in which deconstructionists gather and interpret data, constructionists develop theories and specify AWPPs and reductionists convert AWPPs into policy options. In practice, of course, there are usually many appreciative cycles running in parallel. As soon as a policy option is chosen and implemented it becomes necessary to monitor it for unforeseen consequences — the deconstructionists come back in again. If boundary conditions are violated, the constructionists must rethink and this, in turn, creates more work for the reductionists. The project itself may have a start date and an end date, but the management of our cultural and natural life-support systems demands a repeating cycle (sometimes called "double-loop learning," see Lee 1993). It is not a once-for-all task.

The art of managing integrative research consists of bringing all these cycles into phase for the milestone meetings. Focus the meeting on a definite research product, an annual report, say, or a joint publication. Keep the group small (ideally no more than seven people). Exclude the press, spectators, professional facilitators, academic figureheads, and anyone else not actively involved in the work of knowledge creation.

Although many people imagine innovation to be a natural response to stress, experience suggests this is not so. People who feel their beliefs are threatened find themselves in a bind, under pressure to abandon the beliefs of the knowledge community that sustains them. They usually withdraw to their cultural high ground and lay down a defensive barrage of rhetoric and common sense arguments. Innovation takes time, demands trust and common purpose. Arrange informal social events, but avoid extravagant hospitality. Facilitate breakout meetings but never lose sight of the research product you are trying to deliver. Your aim is to help people feel comfortable and safe enough to initiate focused discourse across knowledge boundaries, not to demolish the boundaries themselves.

Three Types of Problems

There are three problem domains in integrative, socio-natural science, two of which we have already encountered:

- *Ontological* problems exercised ancient philosophers and continue to engage systematic biologists and humanists. Ontological problems provide rich opportunities for reductionists (building new classifications) and deconstructionists (checking and reevaluating them). However, they offer rather poor pickings for constructionists.
- *Dynamic* problems so delighted physicists that they repudiated epistemology and mediaeval philosophy. Here constructionism (theory building) really comes into play. Biological taxonomy, for example, became more attractive to constructionists from the 1940s on as evolutionary theory and genetics began to introduce a dynamic component into what had hitherto been a static, ontological science (Huxley 1940; Blackwelder 1964; Mayr 1972).
- *Historical* problems have exercised biologists and humanists since the 19th century but came into clear focus during the second half of the 20th century. In these, scientists no longer stand outside the domain they study and the boundary of ontology and epistemology is lost. Knowledge system theory was developed with these problems in mind.

Thus knowledge system theory is part of a wider push for a sound theoretical understanding of knowledge creation and socio-natural dynamics. It is not a monolithic solution, but a step towards a unified model of continuity and change. Knowledge systems display two types of behavior, one giving historians a sense of continuity, the other seeming to accentuate transience. Persistent epistemological resonances arise from common interests, environmental stress or recurrent desires, whilst transient discursive flows sustain living knowledge communities as generations march from birth to death. The dynamic interaction of purposive field with appreciative flow is manifest as cultural ecodynamics: conjoint patterns of cultural, behavioral and environmental change.

A teleological analysis of continuity and change *a posteriori* is certainly a useful historian's approach to cultural ecodynamics, but *a priori* or predictive studies require scientists to develop a new, pragmatic mindset, working across spatial and temporal scales and developing complex knowledge systems. Historical trajectories are contingent on innovations — emergent knowledge that causes people to see the world differently and, by changing their behavior, can change the course of history. Their future configuration is irreducibly uncertain because any prediction can lead to innovation that changes human behavior and refutes the prediction itself. To paraphrase Gregory Chaitin, Jonah's law demands a change in the daily habits of socio-natural scientists. Either we renounce our preoccupation with timeless truths, or we compromise rational principles and so renounce science itself.

Endnote

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