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Contending with Complexity: A Response to William H. Newell's "A Theory of Interdisciplinary Studies"

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Abstract: Newell has mounted a large, interesting, and deeply insistent argument to the effect that complexity theory should be adopted as both a rationale for interdisciplinary studies and a guide for its instructional, investigative, and interpretive activities and applications. I agree with him that those who take seriously the practice of interdisciplinary studies need theory, and that complexity theory has value in this regard. But I disagree with four crucial points in his argument:

I. I do not think, as he implies, that our need of theory is rooted in any lack of candidates capable of providing a rationale for interdisciplinary studies and of guiding its activities, including integration.
II. I do not think, as he suggests, that there is a collectivity of interdisciplinarians whose professional callings can all be usefully described as "necessitated by complexity."
III. I do not think, as he insists, that complexity makes interdisciplinarity necessary, or that interdisciplinarity is not required when complexity is absent.
IV. I do not think his analogy between complexity theory and interdisciplinary process justifies his claim that the former is an adequate rationale and guide for the latter.

These disagreements grow out of a basic difference of view about what makes interdisciplinary study necessary: For Newell, interdisciplinarity is required by the complexity of its subject matter. For me, interdisciplinarity is made necessary by the tendency of specialized inquiry to produce knowledge about parts that is too often used as if it were about wholes.

NEWELL HAS MOUNTED A LARGE, interesting, and deeply insistent argument to the effect that complexity theory should be adopted as both a rationale for interdisciplinary studies and a guide for its instructional, investigative, and interpretive activities and applications. I agree with him that those who take the practice of interdisciplinary studies seriously, need theory,[1]

and that complexity theory has value in this regard. But I disagree with much else that is crucial to his argument.

I

Throughout his paper, Newell asserts that interdisciplinary studies has until now lacked a theoretical rationale capable of validating and guiding its procedures, justifying its practices, and providing both meaning and an explanation for its "distinguishing but elusive characteristic of … synthesis or integration" (p. 1). While I agree with the needs expressed, I do not think they are rooted in any lack of candidates capable of serving them.

The organization and synthesis of knowledge were already matters of concern to such nineteenth-century worthies as Comte and Spencer. A more important example for present purposes is the early twentieth-century work of the Vienna Circle. For all that its reductionism may rankle, the group nevertheless offered important synthesizing ideas, e.g., that apparently different phenomena may be manifestations of the same underlying processes, which, once grasped, would provide a single way of understanding many different classes of phenomena that had been pursued in very different domains of knowledge. This is, I think, not far from Newell's opening claim that "acid rain, rapid population growth, and the legacy of *The Autobiography of Benjamin Franklin* . . . [can all be] understood as behaviors of complex systems" (p. 1).

Granting that most of the Vienna Circle's product pertained to the domain of physical science, its discourse took place in the language of philosophy in a way that helped to break the barrier between the sciences and the humanities—a development ably reconstructed and advanced in Hans Reichenbach's *The Rise of Scientific Philosophy* (1951). An outgrowth of these activities, the *International Encyclopedia of Unified Science,* declared on the covers of all of its publications that its purpose was to "explore the foundations of the various sciences and to aid the integration of scientific knowledge" (Neurath, O., Carnap, R., and Morris, C., 1955). And something of Newell's reach for inclusiveness, expressed in his discussion of "anticipated objections" (p. 3) inheres in the fact that the *Encyclopedia* included among its volumes on the physical sciences both Otto Neurath's *Foundations of the Social Sciences* (1944), and a projected volume by Abraham Kaplan called "The Humanities and Unified Science."

Another and different sort of example is to be found in the efforts of the Social Science Research Council to promote integrative work among the disciplines that fell under its aegis, including its justly famous bulletins (e.g.,

Bulletin #64: *The Social Sciences in Historical Study* [1954]). Caroline Ware's important edited volume, *The Cultural Approach to History* (1940), comes to mind here, and because I wrote a master's thesis on it, so does the remarkable journal, *Explorations in Entrepreneurial History*, a series of theoretical and empirical papers published by the Harvard University Research Center on entrepreneurial history between 1948 and 1958. The journal and an equally remarkable array of books that flowed from it were centered upon something described as problem-oriented interdisciplinary scholarship.

Coming forward into the 1960s, I think of Thomas Kuhn's profoundly influential *Structure of Scientific Revolutions* (1962), originally a volume of *The International Encyclopedia of Unified Science*, and a book that achieved its effect in no small measure by bringing ideas and information from sociology, psychology, and political science to bear upon the very complex process of historical change in the physical sciences. Alfred Kuhn's *The Study of Society: A Unified Approach* (1963) is also of this period, as is Muzafer Sherif's and Carolyn W. Sherif's important edited volume, *Interdisciplinary Relationships in the Social Sciences* (1969).

By the 1970s, this whole development was commonplace enough to prompt the Bibliography Committee of the American Studies Association to invite me to prepare an essay on it for their journal, *American Quarterly*—"The Social Sciences in American Studies: An Integrative Conception" (1974). And, jumping to the present, but still antecedent to the composition of Bill Newell's paper, I think of E. O. Wilson's *Consilience: The Unity of Knowledge* (1998).

I offer this very short list in order to suggest that Newell misrepresents the problem inherent in interdisciplinarians' need of theory, insofar as he implies that there hasn't been any. The works I've mentioned are a small sample of many that offer plain enough rationales for interdisciplinary studies, clear notions of integration, and solidly accomplished synthetic works—enough to support the development of interdisciplinary theories and theories of interdisciplinarity, which in fact they did. Accordingly, I suspect that the problem on Newell's mind stems less from a lack of candidates or exemplars than from a lack of interest: As a collectivity, interdisciplinarians don't want ***A*** theory—not any of the ones available and, unless I miss my guess, not the one Newell is proposing—if having a theory means agreeing to organize one's work in its terms.

This leads to my second disagreement.

II

Throughout his paper, Newell speaks of interdisciplinarians as a collectivity whose members share "some widely accepted principles for the conduct of interdisciplinary inquiry" and whose "apparently divergent approaches to interdisciplinary study of the humanities and sciences" are unified, whether they know it or not, by "complex systems" (p. 1). As well, he argues in his discussion of motivations that the unifying effects of "complex systems" extend across seven different kinds of interdisciplinary activities or practices.

In all of this, Newell seems to me to be presenting interdisciplinary studies in terms that come very close to Thomas Kuhn's account of scientific development—as emerging from a pre-scientific stage, marked by competing schools of thought, and becoming a paradigm-based community rooted in unifying agreements about such fundamental issues as what basic things make up the universe of study, how these things interact with each other and with our senses, what questions should be asked about these things, what methods should be used to seek answers, and even what counts as a good answer at all.

Since so many of Newell's references are to the membership of the Association for Integrative Studies (AIS), I think it is proper for me to say that I saw nothing of the kind in my years (1987-2000) of editing the association's journal, *Issues in Integrative Studies* (*IIS*). In the many papers I listened to while seeking manuscripts at AIS conferences and elsewhere, and in the many fewer papers that were submitted for review, I saw people operating rather more pluralistically. As if to say that all theoretical formulations raise and pursue questions from some point of view or other, and no one point of view is a substitute for all the other points of view that people who consider themselves to be interdisciplinarians might have in mind.

It is no secret that I wish things were more as Newell claims they are. But that very wish makes me mindful of something deeply perverse in his argument: He would have interdisciplinarians agree to ideas embodied in complexity theory that are capable of unifying scholarly efforts across domains of knowledge. Yet he also points out in his discussion of "forms of complexity" (p. 6) that complexity theory is itself very far from unified. And while he does make clear what forms of complexity theory he accepts, he doesn't provide his readers with at least two things that they surely would need if they were going to make the extraordinary effort he proposes: 1) a rigorously explicated formalism (e.g., graphic, formulaic, or axiomatic) that shows the structure and dynamics of a complex system to which interdisciplinarians

ought to liken the phenomena that interest them; and 2) some thoroughly described exemplars that concretely show the pair of benefits he claims for modeling different phenomena in complex system terms—that *both* the phenomena *and* the process of interdisciplinary scholarship will be better understood. Given the lack of settled unifying agreements about complexity among professional complexity theorists, it is hard to see how complexity theory is going to unify or otherwise organize interdisciplinarians who do not seem to have unification in mind. At the very least, Newell needs to show us how this can occur with something more than lists of claims.

Let me turn briefly from grand unifications across scholarly domains to the more specific activities or practices that Newell calls motivations, I suppose because they can be seen as motives for doing interdisciplinary work. He mentions seven: general and liberal education; professional training; social, economic, and technological problem solving; social, political, and epistemological critique; faculty development; financial exigency (downsizing); and production of new knowledge. Acknowledging that this lot may seem too diverse to be pursued and/or understood in any one way, he argues that the differences among them "reflect different consequences of studying complex systems, not different kinds of interdisciplinarity" (p. 5). Then, to justify his claim, he lists some benefits of interdisciplinarity. Ostensibly based on complexity, the benefits listed are pretty much the same benefits of interdisciplinarity that have been described for something like 50 years at least—long before complexity theory came on the scene.

Why do this? Why invoke benefits claimed for interdisciplinarity practiced along many different theoretical lines in order to justify limiting interdisciplinarity to practices based only on complexity theory? For that matter, why seek to rationalize, organize, and guide interdisciplinary studies on the basis of a theoretical discourse that is itself not particularly unified? And why, after all, insist that a pluralistic collectivity of scholars must learn to do the unification-through-complexity dance?

An answer is given in the first sentence of Newell's abstract: "Interdisciplinarity is necessitated by complexity, specifically by the structure and behavior of complex systems." From his point of view, there is no better way to go.

This leads to my third disagreement.

III

Newell's central claim is that his rendering of complexity theory "for the first time… sets forth a comprehensive and long overdue rationale" for inter-

disciplinary studies (p. 6). The historicity of his claim is, in my view, mistaken, as is the implication of "long overdue" insofar as it implies the existence of a professional community cognizant of its need for what he is proposing. But neither of these objections affects the two key ideas on which his argument ultimately rests—that complexity makes interdisciplinarity necessary and that interdisciplinarity is not required in the absence of complexity. But if complexity can be shown to be adequately handled within a single discipline, the first idea is in trouble. And if noncomplex phenomena can be shown to require interdisciplinary study, the second idea is in trouble. I think both ideas are in trouble.

Complexity, according to Newell's account, inheres in three attributes of a system—multifacetedness, coherence, and nonlinearity of relations among components. Since he allows that the first two attributes can appear in noncomplex systems, nonlinearity lies at the definitional heart of the matter. And it is complexity so defined that necessitates interdisciplinarity. But this last, it seems to me, is just not so. Specialized disciplines do identify and explain nonlinearity in the behavior of systems they examine.

Consider, for example, the work of experimental psychologist James Diggory (Diggory and Magaziner, 1959; Diggory and Ostroff, n.d.) on self-esteem. Surely, the *systems* under study, human beings attempting to arrive at a judgment of themselves, are multifaceted—composed of an array of subsystems ranging from the purely physiological and neurological through the perceptual, motivational, affective, and cognitive, to the multiple forms of intelligence. Surely, as well, these multiple subsystems manifest a high order of coherence—the sort of thing we have in mind when we use global terms like personality or make more specific references to types of personalities. What Diggory identified and explained was a fascinating U-shaped curve—certainly an instance of nonlinearity—produced by the behavior of his subjects over time.

More specifically, studying his subjects' estimations of the likelihood that they would succeed at a task, he found that such estimates could be manipulated by varying three kinds of conditions: 1) how close the subjects thought they were to success at the outset of the task; 2) whether subjects received positive, negative, or ambivalent information about their performance on successive trials; and 3) whether deadlines for completion of the task were present or absent, and if present whether they were clear or vague. A U-shaped curve for subjects' estimations appeared under the conditions of a highly promising start, a succession of ambivalent performances—sometimes successful and sometimes not—and distant or vague deadlines. In essence,

subjects faced with feedback indicating ambivalent performance following early indications of success lowered their estimates over trials and then, at some low point, reversed their estimations, raising them to produce, overall, a U shape. The same task situation, containing a clear deadline, produced only the downward portion of the curve. The same task situation with the clear deadline changed in mid-process to a vague deadline produced the U shape, just as in the first case. In this sense, Diggory had identified a complex effect in the process of self-evaluation and offered a perfectly clear explanation of it in terms of feedback and deadlines—all within the conceptual and methodological confines of his chosen discipline.

What about the obverse case? Is interdisciplinarity ever required when complexity is absent? The question arises for me because of two of Newell's claims:

> [C]omplex systems and phenomena are a necessary condition for interdisciplinary studies. An interdisciplinary approach is justified and required only by a complex system. So if a behavior is not produced by a system or the system is not complex, interdisciplinary study is not required. (p. 1)

and

> The phenomena modeled by most complex systems are multi-faceted . . . [and like] the phenomena modeled by all systems, their overall pattern of behavior is self-organizing, thus different from the sum of its parts and not fully predictable from them. Because the various facets are connected by nonlinear relationships, the overall pattern of behavior of the phenomenon (and thus the system) is not only self-organizing but also complex. (p. 2)

What about phenomena that can be regarded as the patterned behaviors of a system that is multifaceted and self-organizing or coherent but not nonlinear? Newell's claims seem to put such matters beyond the pale of interdisciplinary studies. I just don't buy this. To show why, let me offer another example.

Discussing "personality development in a homogeneous, slowly changing culture" Margaret Mead noted:

> [e]very individual in the human environment will carry the same cultural assumption; both he who observes the social forms gladly, and he who flouts and ignores them; the man who is admitted to the ceremony, and the

> woman who is excluded; the chief who sets his foot on the slave's neck, and the slave who kneels to receive the stepping foot…. Adults and older children, within whose personality the culturally distinctive learning sequence has been integrated, are able to impart simultaneously the place of the present bit of learning in a longer sequence, that which the child has already experienced and the part which is to come….This *simultaneity of impact* is carried not only by the behavior of each individual with whom the child comes into contact, but is also mediated by ritual, drama, and the arts. The shape of a pot, the design on the temple door, the pattern of the courtyard, the form of the bed, the grave post or the funeral urn, the dancer's headdress and the clown's mask, are again reinforcements and whole statements of the same pattern which the child himself is experiencing serially. (1959, pp. 516-517)

Here, Mead is offering a generalized response to one of the most important of all the questions we ever ask about human ways of life: How do they replicate themselves? Why do they persist across many generations? Her account makes it clear that any proper answer to such a question must be sought through a wide range of methods and data sets developed under the general influence of a wide variety of conceptual schemes. This is so not because we are looking at a clear case of complexity—on the contrary, we are facing the absence of nonlinearity or, at least, the kinds of contradictions and bifurcations and changes of developmental direction that are the usual analogs and/or manifestations of nonlinearity. In Mead's case, we are facing a stability of form that must be pursued through the multiple facets of human existence, almost certainly by taking advantage of what several kinds of specialized inquiry can show us, and then discerning within their multiplicity a basis for the coherency of the things they reveal. I just don't see that effort as anything but interdisciplinary in character.

I want to be specific about my last comment. Newell observes that "[i]n order to justify the interdisciplinary approach, its object of study must be multifaceted yet its facets must cohere. If it is not multi-faceted, then a single disciplinary approach will do (since it can be studied adequately from one reductionist perspective)" (p. 2). It seems to me to follow from this that investigating the multiple facets of a system *requires* a scholar to become familiar with the elements of the relevant disciplines—their conceptions, methodologies, domains of observation, and empirical claims. As well, to use what is learned, a scholar would have to formulate ways of regarding relations among facets that have, after all, been studied separately and differ-

ently in the disciplines. Neither of these activities depends on the relations being nonlinear. And both are basic pieces of what most of us, including Newell throughout his paper, think of as interdisciplinary study. Complexity, as Newell defines it, is just too narrow a cut into this universe of discourse. Nonlinear relations among parts are not the only emergent properties of whole systems that require interdisciplinarity. This together with his remarks about disciplines and reductionism suggest to me that his cut could put two of the most powerful synthesizing/integrating devices known—reductionism and holism—outside the domain of interdisciplinarity studies.[2]

IV

Even if complexity doesn't necessitate interdisciplinarity, and interdisciplinarity can contribute to the study of noncomplex phenomena, there remains an important thrust in Newell's account:

> Since the [interdisciplinary] process is a response to the nature of the reality being studied, it should reflect what we know about the characteristics of complex systems. Each step in the interdisciplinary process should have some analog in complex systems theory. (pp. 15-16)

By likening what we don't understand, the interdisciplinary process itself, to what we do understand at least theoretically—the structure and dynamics of a complex system—we gain a rationale for our activities which can also guide them. Such, as far as I can understand it, is Newell's justification for proposing complexity theory as a best basis for organizing ourselves in pursuit of interdisciplinary work.

Not bad, not bad at all! At least not if you buy two underlying assumptions: The first is that the steps Newell describes as constituting the interdisciplinary process are in fact the steps practitioners take, rather than steps that are said or imagined to have been taken at some time after the actual stepping was done. In *The Conduct of Inquiry,* the philosopher Abraham Kaplan (1964, pp. 3-11, 162) taught us to be wary of such claims lest we mislead ourselves and our students into not taking available steps because they do not resemble some idealistic reconstruction of steps that no one—not even our successful predecessors—ever actually took. Following Kaplan, I have my doubts about Newell's analogs, not least of all because both the steps and the theory that is supposed to rationalize them are so very general. In this connection, I have wondered for quite some time about why Newell settles for such omnibus terms as "insights" and "assumptions" in the steps that tell us what we are

trying to find and integrate as we examine and gain command of each of the disciplines that we have somehow determined to be relevant to a problem. Why not focus more specifically on the elements of disciplines—their conceptions, their methodologies, their findings—that so plainly affect their claims? That we may benefit from effecting integrations at this eminently describable level is obvious enough from the many instances of it, e.g., the conceptual integration of sociology, psychology, and anthropology presented in G. C. Homans' *Social Behavior: Its Elementary Forms* (1961); the integration of anthropological and psychological methods pursued in M. J. Herskovits, D. T. Campbell, and M. H. Segall, *A Cross-Cultural Study of Perception* (1969); the conceptual and empirical integrations of psychology (gestalt, learning), sociology (community structure, socialization), political science (revolution), and history (establishing chronology from present remains of the behaviors of past scientists) developed in T. S. Kuhn's *Structure of Scientific Revolutions* (1962). These *oldies* are offered in keeping with my disagreement with the historicity of Newell's claims about theory. But for the sake of having a couple of more current references, let me mention the brilliant substantive and theoretical synthesis of materials from something like a dozen different disciplines in John Reader's *Africa: A Biography of the Continent* (1997), and the methodological integrations across multiple fields involved in the process of meta-analysis, summarized in Charles C. Mann's "Can Meta-Analysis Make Policy" (1994). Perhaps a more developed explication using a completely worked out case of an interdisciplinary study cast in terms of the complexity analog would help here—something that I simply didn't find in Newell's road map and acid rain examples.

The second basic assumption is that "we interdisciplinarians" do in fact *both* take reality to be complex in Newell's sense of the term, *and* take complexity in that sense to be what we want to understand about reality. Here again, I have my doubts. Newell's argument depends on the idea that the character of a subject matter (complexity) determines the kind of theory that will work on it. Maybe so, but maybe not, as I will soon suggest by way of conclusion. Here let me suggest that he may be omitting a crucial consideration: What one *wants* to know about any subject matter deeply influences which aspects of its character will be allowed to determine the kinds of theory one employs. On this account, I don't think the analogs he identifies, even if they exist, are all that likely to find ready acceptance of their implications for *doing* interdisciplinarity. Even if *we* agree that bringing ideas and information from several sources is a good thing to do, it does not follow that what we want to know about the things we treat that way are the same.

Speaking personally on this last point, I must say that there are times when the complexity of a phenomenon is not what interests me about it. As in the earlier example from Mead, there are times when the multifaceted nature of a phenomenon invites me to pursue it using the elements of several disciplines, and when the pursuit reveals an underlying pattern to the facets, that is interesting precisely for its *lack* of Newell's sense of complexity. And there are times when what interests me about the elements of different disciplines per se is not their differences, but their agreements—when differences in terminology are only that and the same things are being called by different names, or when different things are indeed being treated differently but in ways that foster their use together. And there are times when disciplines and their constructions of reality don't interest me at all—when a topic or problem or theme takes my interest and all I want to do is pursue it, wherever it happens to be taken up, without paying much attention at all to the disciplines that may be affecting the discussions I find. In these three kinds of instances, I am not dealing with complexity and I may not even be dealing directly with disciplines as such—but I am exercising my own skills and familiarities as an interdisciplinary scholar bent on some kind of integration, albeit not the specific kind that involves complexity per se—not in the phenomena I'm studying and maybe not even in relations among the disciplines I'm using.

V

In an earlier version of his paper, Newell wrote, "the [interdisciplinary] process is a response to our perception of the nature of reality being studied (namely, its complexity)" (1999, p. 10). In the present version, as we have seen, he writes that the process is "a response to the nature of the reality being studied" (p. 15). The change in language is, I think, a change toward what he has had in mind all along.

To be sure, he does speak often enough in terms that could make it seem that he is on a *perception* kick—when, for example, he tells us that the proper objects of interdisciplinary study are phenomena that can be "modeled by complex systems" (p. 5) or when he proposes that we interdisciplinarians are or should be in the business of constructing complex systems to represent the phenomena which interest us. At these moments he sounds for all the world like an instrumentalist, one who asserts that our knowledge claims about reality are not descriptions of what is actually there, but metaphorical statements in which we tacitly liken what we experience and don't understand to conceptions—mental images of the structure and dynamics underlying the

observable world—that we use to direct our inquiries and make sense of our results.

But there are many more statements that are entirely in keeping with the one he finally has settled on—not that complex systems are merely a way of regarding reality, but that reality is by its nature, complex—constituted by nonlinear relations among the multiple factors of which it is composed.

To one who holds this view, the disagreements I have raised must amount to mere quibbles:

- It doesn't matter whether there has been a long history of integrations of knowledge proposed and used. Insofar as they were not addressed to complexity, they were simply wrong.
- It doesn't matter if there is no community of interdisciplinarians ready to agree to the idea that their only justifiable subjects are subjects that can be modeled by complexity. Ready or not, they have no real choice because complexity is how it is.
- It doesn't matter that specialists sometimes address complexity, nor does it matter that interdisciplinarians sometimes don't. Such facts merely show that specialists are sometimes right about reality and interdisciplinarians are sometimes wrong about it.
- It doesn't matter whether interdisciplinarians don't, in fact, do the steps of their process as prescribed by the dance master of complexity, but when they don't, they are out of step with reality that calls the ultimate tune.

But I am an instrumentalist. I do not think interdisciplinarity is or can be a response to the nature of reality per se because I don't think we poor humans can know what the nature of reality is. Rather I think we come to the task of trying to understand reality obliged to rely on what Jerome Bruner (1986) has called cultural prosthetics—devices that extend the powers of our perceptual and cognitive equipment vastly, but only at the price of shaping what we know. Our knowledge is constructed through the imposition of concepts and methods and received information rather than a direct response to reality.

On this view, interdisciplinarity is a response not to reality as it is, but to the condition of our knowledge about reality. That *condition* is, I think, understood in terms of three widely held intuitions: 1) that knowledge is almost always sought by means of specialized inquiry—because, as I think it was Bertrand Russell who said, we can't know everything before knowing some-

thing; 2) that knowledge produced by specialized inquiry is necessarily partial, focused, as Newell argues, on particular facets of the observable world; and 3) that our knowledge, being partial, is dangerous when applied to wholes—when we treat wholes as if they were the same as the parts we *know*.

From this point of view, the interdisciplinarian's task is to figure out how to make synthetic use of the partial knowledges that specialized scholarship is bound to generate. As I have argued, there is no lack of examples of this having been done. At the same time, I am bound to grant that there is blessed little evidence of a common theoretic informing the synthetic or integrative games that interdisciplinarians play. In part, this reflects variations in the types of knowledge that any particular integration brings together. And those variations are apt to reflect the particular problems, topics, themes, applications, etc. that particular scholars happen to find interesting. For in the end, I suspect that very few people are drawn to interdisciplinarity for its own sake. Rather, they are drawn to it for the sake of some matter of substance that they find interesting and important and inadequately or, as I am inclined to put it, dangerously handled by any single discipline.

The pluralism my view implies is by no means intended to deny the importance of theory to interdisciplinary practice. Let me be a bit more specific about this.

I take theories to be very generalized statements about relationships among objects and processes that constitute our experiences of the world and ourselves. By associating a theory's generalized statements with very particularized statements about specific objects, processes, and relationships, theories are used to predict and/or explain our experiences. Theories that do explain and predict large ranges of our experiences are deemed adequate. Theories that lead to many successful applications in the world of our experience are deemed powerful.

The practice of any scholarly discipline is largely a matter of trying to determine whether its theories are adequate and powerful. Not surprisingly, such efforts are directed and organized by the theories being evaluated, that is, the theories are the basic source of the questions asked, of the data and methods used to pursue answers, of the experiences we actually have during the evaluative process, and, ultimately, of the meaning and significance of what is learned. This last, indicating both orderliness and circularity, is why theories are both necessary and dangerous—necessary because they organize work, dangerous because they do so by deliberately limiting attention, perception, and inquiry.

Interdisciplinary theories express the understanding of some phenomenon, problem, topic, theme, etc. that has been reached by bringing to bear upon it ideas and information that have been separately and differently developed in specialized disciplines. Theories of interdisciplinarity express an understanding of how and why ideas and information drawn from different specialized disciplines have been brought together, integrated. Newell offers complexity theory as an instrument fit for both purposes—for theorizing anything and everything that interdisciplinary scholars ought to take up and, as well, for theorizing the how and why of doing interdisciplinary work. Were it agreed to, it would be dangerous in the same sense as any discipline's theories are dangerous.

Nevertheless, by proposing his theory, Newell calls attention to a very real problem affecting interdisciplinary practices—the mélange of instructional, investigative, and interpretive activities, and their applications, that bring together ideas, information, and sometimes people from different specialized disciplines. Much of what we encounter at AIS conferences and in submissions to *IIS* are reports on such practices. The problem they present is that the reports commonly do not say much about the interdisciplinary theories and/or theories of interdisciplinarity involved in the practices they describe.

This is not surprising insofar as theory can be seen as the besetting sin of disciplinary work—to dwell on it is to run the risk of accepting its limiting effects, hence accepting a contradiction of purpose. No doubt, there is some truth in this view. Nevertheless, there is a difference that is not to be overlooked: Interdisciplinary theories are responsive to the existence and legitimacy of multiple perspectives and their effects. That is what makes the constructions produced by such theories interdisciplinary. Organizing our efforts around such constructions is not likely to lead to the sort of enforced tunnel vision that we associate with the practice of disciplines, for the inference problems associated with interdisciplinary work almost guarantee that there will always be several such constructions generated for any topic. On the other hand, it seems to me obvious that we shall always be in the position of reinventing the wheel if we do not organize ourselves around the task of articulating and evaluating these constructions—that way showing the strengths and weaknesses of alternative interdisciplinary theories and theories of interdisciplinarity.

In this sense, we must indeed make a practice of paying more systematic attention to theories. But if the attention is to be paid on behalf of interdisciplinarity, then it must be paid to the knowledge games we play. To try to focus attention on the absolute nature of reality pursued in the one and

only way that reality itself requires, is to claim an *a priori* knowledge of the real that no one, not even my pal Newell, has or can have.

VI

So Newell's case just doesn't work for me. All the same, I recognize and deeply respect his hard work that has put before us an explicit, insistent case capable of drawing out the issues that surround our enterprise *and* of organizing our several constructions of those issues. This is something that we in AIS sorely need to do. It takes both courage and devotion to put one's own work on the firing line for the sake of effecting this discussion. For both, as for the work itself, he deserves our deepest thanks.

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Notes

1. See R. K. Merton's chapters, "The Bearing of Sociological Theory on Empirical Research" (1957a, pp. 85-101) and "The Bearing of Empirical Research on Sociological Theory" (1957b, pp. 102-117). This pair of essays offers the best discussion of our need for theory that I have ever read. First published, I believe, in the late 1940s, they are a superb interdisciplinary tour de force in the dual sense of both drawing upon and making self-consciously integrative use of ideas and information from no less than seven different disciplines. The date of their publication, like the publication dates of so many of the items mentioned in this essay, are important as regards Newell's claims about what we have lacked until now.
2. For a discussion of emergent properties of systems and both holism and reductionism as integrative forms, see Stanley Bailis (1984/85).

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