
Reconceptualizing Knowledge Accumulation in Sociology

ANDREW ABBOTT

Let me begin with a simple paradox. On the one hand, we have today descriptive data on social life at a level undreamed of a hundred years ago. Sociological data-gathering provides only the tiniest fraction of this, of course; most of it is gathered passively in the course of activities like paying wages, selling goods and services, collecting taxes, and monitoring compliance with laws and regulations. But wherever it comes from, the sheer amount of data is utterly astounding.

On the other hand, it is equally clear that our foundational approaches to social life are little different from what they were a century ago. There are four approaches: the utilitarian individual framework, the social forces framework, the dialectical and other conflict frameworks, and the process/ecologies framework. To these over the past century has been added perhaps one new general approach, appropriate enough for the “information century”: the view that social life is basically about the working out of symbolic systems.

Between these two extremes—the vast amount of data on the one hand and the five basic theoretical schemes on the other—are myriad levels of theories and generalizations and hypotheses and stylized facts and so on. The question of cumulation in sociology is basically concerned with these layers in between, with levels of sociological work that change at a medium rate, something between the grand stasis of our theoretical paradigms and the Brownian churning of micro data.

We can approach this cumulation question in several ways. We can take it as a question about exactly how the work of the past is used, a question about knowledge and citation of past work. Or we can take it as a question about the fate of particular theories, a question of what happened to labeling theory or neoinstitutionalism or status attainment. Both these questions—about citation and about research programs—are more or less *empirical* versions of the underlying question of cumulativeness.

We can, second, take the question of cumulativeness as basically a theoretical question, turning ourselves towards the philosophy of science and inquiry. What is real cumulativeness as opposed to mere repetition? Is cumulativeness a matter of piling up facts? of developing theories? of paradigm shifts? What are the alternatives to cumulativeness as models of scientific life? These are the theoretical versions of the cumulativeness question.

Andrew Abbott is Gustavus F. and Ann M. Swift Distinguished Professor at the University of Chicago. He can be reached at aabbott@uchicago.edu.

We can, finally think about cumulativeness itself as a particular cultural belief and investigate its history and functions as we would those of egalitarianism as a part of liberal politics or efficiency as a part of bureaucratic ideology. Is cumulativeness a real phenomenon in the life world or is it a symbol with which we try to understand the social process of our professional lives? Might not, for example, the real purpose of a norm of cumulativeness be to let us off the hook, to let us read the last ten years of work instead of the last hundred? How many students of immigration, one wonders, have read *The Polish Peasant in Europe and America*? Not many—but that’s because a belief in cumulation lets them assume that all the crucial nuggets of Thomas and Znaniecki’s masterwork have been preserved by the careful sifting of criticism and “cumulation.”

I shall here touch briefly on all three of these versions of the cumulativeness issue. My aim is not to answer questions, but to raise them. I have a feeling that most of us have some level of faith in cumulation as an ideal, but that we’ve never really thought about it much. So my goal here is simply to suggest some puzzles that thinking about cumulativeness raises.

Cumulativeness as an Empirical Issue

To begin, then, with cumulativeness as an empirical issue. I have here two kinds of facts to note, as I suggested earlier. They are at two different levels: one is about articles, the other is about literatures.

So first, about articles. Many of us think that the literature of the past is forgotten. Students seem unaware of monumental studies well known to the faculty generations. Students cite long-familiar ideas to recent writers as if the latter invented them; Bourdieu receives the most such homage these days, replacing the egregious Giddens. And we can indeed watch even major scholars rediscovering the wheel—John Goldthorpe’s recent rediscovery of prestige is a good example, as was Evans, Rueschemeyer and Skocpol’s (1985) rediscovery of the state twenty years ago. Overall, we just seem to lose sight of earlier work. Let me, by the way, avoid making this fault myself by pointing out that Herbert Gans (1992) made the same argument in writing about “sociological amnesia” in 1992—using data on reference lists in 24 books—and that he, in turn, cited Sorokin’s 1956 book on *Fads and Foibles in Modern Sociology and Related Sciences* as an important predecessor. If Gans and Sorokin are right, then cumulation does not exist (except in the case of the judgment that cumulation does not exist), and the discipline is, in the words of Shakespeare (1606) “a tale told by an idiot, full of sound and fury, signifying nothing.”

Like Gans, I also thought it might be interesting to look at citations, but unlike him, I have chosen to look at articles, where there is much more—and more stable—data. Of course, citation is often purely decorative and so on, but it is still the case that if there really is a steady pattern of forgetting the past, over time we should see the typical age of a journal citation holding constant or perhaps even getting younger and younger, as the sheer mass of recent work increasingly prevents our digging into the past.

As it happens, ISI has not kept subject area data on such things for very long, so we are forced to look within particular journals. This however enables us to rule out ISI coverage changes as a confounding factor, although at the same time it exposes us to the problem of local variations in journal practices. I have looked at five journals: *American Journal of Sociology* (AJS), *American Sociological Review* (ASR), *Social Forces* (SF), *Journal of Marriage and the Family* (JMF), and *Social Problems* (SP). For each I have looked at both of the ISI measures—the “cited” measure of the average age of articles in

those particular journals that are cited by any ISI journal in a given year, unweighted by the numbers of citations to particular articles, and the “citing” measure of the average age of citations (so far as I can tell, these can be citations of anything, including books) that appear in these journals themselves. Of course there are various problems here—whether references to particular pages of a given title count as part of its total, and so on and on. But these problems should not substantially vary between journals at any given time and will only vary over time if there have been major changes in ISI practice, which there do not seem to have been. (Most ISI changes are in coverage.)

I have looked at four years of data—1977, 1985, 1995, and 2004—covering about 30 years of sociology. And the data are almost completely incompatible with the idea that the old stuff gets forgotten. For each of the five journals, for both citations to the literature (citing) and citations from the literature (being cited), the cumulative distribution of citation ages shows that citations are getting older and older. The proportion of cited *AJS* articles published less than ten years before the citing articles fell from 46% to 25% between 1977 and 2005. Put simply, in 1977 46% of the *AJS* articles cited somewhere or other had been published less than ten years before the articles that cited them. In 2005, only 25% of the cited *AJS* articles were published less than ten years before the citing articles. At *ASR*, the fall was from 44% to 31%, at *SF* from 46% to 43%, at *JMF* from 83% to 46%, at *SP* from 54% to 35%. With the exception of *SF*, these are dramatic falls. Taken together they mean that cited papers in these five journals were casting a longer and longer shadow forward.

If we look at the citation lists of these five journals themselves, we find the same pattern. The proportion of all citations in *AJS* published less than ten years before the publication date of the article in which those citations appeared fell from 60% to 47% in the period 1977 to 2005. The *ASR* fall was from 61% to 44%, the *SF* one from 61% to 48%, the *JMF* one from 63% to 56%, and the *SP* one from 66% to 54%. Indeed, such a fall is observed for all these journals even if we lower the cutoff point from ten years to three years pre-publication (and for all points in between). I might note that the ending averages are in the ballpark—but a little below—Gans’ number of 55% of citations being to work published in the last ten years.

These facts seem to me inconsistent with the idea that contemporary scholars pay less attention to earlier work than did their predecessors. That is, unlike Gans, I think the glass is half full, not half empty, and that it is getting fuller.

Now there may be, to be sure, some artifactual explanations of this pattern. Perhaps the sheer number of citations in given journals grew, and as they expanded their bibliographies, authors added earlier works on a somewhat decorative basis. But while the *JMF* more than doubled its total citing level, *ASR* and *AJS* stayed roughly constant; yet all three cited earlier material as time went on. And indeed, if anything, *JMF*’s vast increase went to more recent material, for its citations aged the *least* of all five of these journals. So expansion of reference lists seems ruled out. Another artifactual possibility is that the extremely rapid expansion of scientific publication in the 1950s and 1960s (the current expansion is from a larger base and so has a much lower rate) may have meant that the pool of available things to cite was much younger in 1977. But this would only explain the aging of citing levels, not the aging of articles cited. Still another possibility is that the level of measurement is so fine that single articles may be swaying the data. This is hard to tell without the detailed data, but to take one example I know well, the three most heavily cited *AJS* articles in modern times, so far as I can tell, are Mark Granovetter’s on “weak ties” and “embeddedness” and Jim Coleman’s on “social capital,” and these three articles—of course over ten years old in 2004—account for only 11% of the 5,600 cita-

tions to the *AJS* in that year. That's not enough to dominate the data. Still another possibility is that increasingly effective access to past work—a process culminating in *JSTOR*—may simply have made older work more visible and hence more cited. This would lead us to expect a very big recent leap—between 1995 and 2005—but the data don't show that.

These explanations all feel a bit like clutching at straws. A more likely explanation of the aging phenomenon—and one that brings us closer to thinking about cumulation—is that the 1977 data are backed up against a real sea change in methodology. The late 1960s saw the firm establishment of regression methods and, far more important, their commodification in canned software. The change in methodology made many earlier articles methodologically incommensurable with current work and perhaps therefore drove them into obscurity. Because no subsequent methodological change has been so drastic, past work now lives longer in the literature. This seems a more likely explanation to me.

So my first bit of evidence is that perhaps the sound and fury view is not all it is cracked up to be and maybe we do have some cumulation after all.

What then if we turn to the half-life not of articles but of research programs? What evidence can we find there? Here it is harder to undertake a positivist analysis. It is not really clear how to identify the birth and death of research programs.

If we look at methodological enthusiasms, these seem to me to betray a clear pattern of sudden vogue followed by settling back into steady production. The vogues seem to succeed each other pretty quickly, as we on the editorial staff of the *AJS* have good reason to know. Today, negative binomial regression is the hot thing—if I had a five-dollar bill for every *AJS* submission last year that used negative binomial regression I could take you all out to dinner (and I wouldn't want to go bail for how many of those people could motivate the negative binomial distribution probabilistically). The negative binomial replaced HLM as the hot item, which replaced logistic regression, which replaced event history methods, which replaced pooled time series methods, which replaced path analysis, and so on. Somewhere in there LISREL and tobit rushed across the stage and once in a while instrumental variables and multidimensional scaling make forlorn appearances, as if they're looking for the party and came to the wrong house.

In all this it is hard to see cumulation or growth amid what seems simply like faddism and replacement. But perhaps the reason you can't publish a multiple regression with only two predictors or a simple crosstab in *AJS* today is that the really important things in social life—things that are pretty obvious in such tables—got said a long time ago, and so now we have to look at fainter—and probably less important and less believable—things because that's what our fancy current methods are better at. In that sense, too, perhaps we are cumulative—we nailed down the big stuff a while ago and are now cleaning up the details. But I would not want to push that opinion in the midwinter methodology section meeting.

As for research programs themselves, it seems to me—on the basis of case study evidence—that many literatures in sociology have about a 25-year-life cycle. Labeling theory is a classic example, beginning in the early 1960s in *Social Problems*, flowering in the 1960s and 1970s, then falling victim to quantitative attack and paradigm exhaustion in the late 1970s and early 1980s. Historical sociology had its precursors in the 1950s and 1960s, burst out fully formed in the late 1970s, churned out epigones and specialized studies in the 1980s and 1990s, but now there is talk of a third wave and a linguistic turn, sure signs that the original paradigm is exhausted and that something new is replacing it. There are dozens more examples. And on this argument one might

predict, for example, that neoinstitutionalism, population ecology, and the anti-Mertonian sociology of science are all nearing the ends of their scholarly life cycles, and we can expect major transformations in them soon.

This steady change is as characteristic of quantitative literatures as of qualitative ones, by the way. The status attainment paradigm had about a 20-year run after Sewell et al. got it going; with a few conspicuous exceptions, we were all pretty tired of it by the end of the 1980s. And I have no need to list the current quantitative excitements for you—social capital in the inequality racket, political opportunities in the social movements racket, and so on. These are the coming things, paradigms with about 15 years left in them.

In summary, it seems that sociology is littered with research programs that are exciting for a couple of decades, then peter out into routinism and time-serving. The same is probably true of methodologies. There are a number of possible mechanisms predicting this cycle. The most obvious is career structure. Twenty-five years is about the length of time it takes a single group of individuals to make up some new ideas, seize the soap-boxes, train a generation or two of students, and finally settle into late career exhaustion. Their students may keep things going, but their students' students tend to be fairly mechanical appliers of the original insights. The really creative people don't make their careers by hitching themselves to other people's wagons. As for methodologies, it is not surprising that people with heavy investments in mastering particular methods try to keep extracting rents from their working assets instead of retooling. It is always the youngsters, with no sunk costs and no assets to provide rents, who make their careers on new methods.

An alternative possible mechanism is a version of the one just employed—the idea of exhaustion—but applied not at the level of individual careers, but at that of the research program. Perhaps we say most of the things we can say within such a program in a rush, at the beginning. The first generation issues the manifestos, their students nail down the major details, and the grandchildren mop up tiny issues and take the paradigm to new datasets and empirical settings. By then there is not much left to do. Similarly, when we get a new methodology, we take everything we can lay our hands on, run it all through the new meat grinder, and serve up our results as a dandy new meatloaf. Pretty soon, all the old scraps are ground up and there's nothing left to cook with.

Another part of this corporate self-limitation of paradigms is the fact that every such literature—theoretical or methodological—is based on some highly conventional assumptions—about theories, indicators, methods, concepts, types of data, and so on. As time passes, these become more and more fixed inside literatures, since it is easier not to start from scratch every time. But this makes literatures more and more sealed against outsiders, more and more idiosyncratic, for most of our assumptions of this kind are pretty extravagant, indeed they are sometimes downright peculiar. But this increasing idiosyncrasy of literatures means that while their work appears to its authors to cumulate steadily, to outsiders it often seems more and more insular as time goes by.

This is my own reaction, for example, to Michael Hannan's very admirable population ecology paradigm. I admire indeed the attempt to specify terms, to develop and formalize theory, and so on. But the assumptions required to do this are quite strong, the kinds of evidence scholars in this paradigm use are standardized and limited, and the central methodological tool of event history analysis has vital assumptions violated in every single application. So we can allow that there is cumulation in population ecology, but only within a certain limited area and given acceptance of a fairly strong set of assumptions. None of us truly believes that this is the last word on organizations, even

though it is a focused and cumulating research program on that topic. It just has natural limits, imposed by its radical assumptions. Perhaps most of our literatures are, at their best, like this quite admirable population ecology literature. They make a set of assumptions, then try to work out the details of what can be found given them. But eventually, those of us in other assumption communities are going to rebel and judge the evolving paradigm as just too insular, too limited. And at the same time, its increasing fixity means that it will begin to have a hard time attracting truly creative minds.

Thus my second empirical observation is that cumulation in sociology is not a discipline-wide phenomenon. It tends to happen within what I have elsewhere called generational paradigms (Abbott, 2001), invisible colleges that tend to accept a set of conventional assumptions about the social world and ways of studying it, and that tend to work out all the various permutations and combinations of social knowledge possible within those assumptions. That cumulation happens mostly at the level of these cohort-based communities was not at all the belief of our predecessors, however. The mainstream generation of the 1950s, for example, believed devoutly in a broad, fully disciplinary cumulativeness. They thought of sociology as a matter of building a house, stone by stone, room by room. It is instructive indeed to look at the astounding book of Berelson and Steiner entitled *Human Behavior: An Inventory of the Scientific Findings* (1964?). Today it seems like a kind of Ozymandias statue in the lone and level sands, summarizing a set of things many of whose very premises we no longer accept. But one can now see that this book was a kind of summary of the social psychology and survey paradigm that rose during the 1930s and that dominated the discipline for about 20 years. Of course that paradigm did not disappear in the 1960s. But it was no longer the focus, the center, the cutting edge of quantitative sociology. That focus became mobility studies and inequality in the 1960s, and social psychology settled back into being a routine production system. Perhaps, indeed, generational paradigms do not so much die after 25 years as they do settle into a kind of stable, middle-aged existence.

Cumulativeness as a Theoretical Issue

Before we try to develop our empirical sense of cumulativeness further, however, we should reflect for a few moments about what cumulativeness is. The answer to this—the theoretical question of cumulativeness—is not at all obvious.

There is a primitive sense of cumulativeness; it consists of simply piling things up. We can all agree that this is “accumulation,” but I think we intend some more specific qualities by the term cumulativeness, which is after all not just a formal concept, but also a normatively positive term for most of us. Most of us think that cumulativeness involves consistency in underlying approach. Cumulation involves commensurability, a building directly on things before, which implies in turn the mutual translatability—if not, indeed, the identity—of concepts, measures, and so on. But I think most of us actually think cumulation entails something beyond the piling up of consistent and mutually translatable things. For most of us cumulation involves also the idea of abstraction. We expect of cumulation that ultimately we will find empirical regularities that allow us to “know” large numbers of particular facts in an efficient way. Force equals mass times acceleration is the classic example of this. It is an empirical regularity (it is not a causal statement except in the limited Humean sense) that saves our having to know billions of particular data points. For most of us, I think the concept of cumulation involves developing precisely these kinds of abstractions and resolving the minor contradictions that they produce, and so on.

But the project of abstraction clearly has intrinsic limits, because large parts of social life concern things that are specifically set up not to be law governed; the game of human life is after all to assign new meanings to old things. That's how we differ from ants. As a result, each time we social scientists think we have found a law of human behavior, we can suddenly see all the ways in which people create meaning by violating it. It is the same as when a bunch of people have cut a shortcut across the university quad, and once it becomes normative and we cave in and pave it, people begin to take shortcuts from the middle of it across another green space and so on. The limit is simple; you pave the quad. The green space—which in my analogy is the interesting stuff about human behavior—simply disappears. Similarly, the project of finding the ultimate law-like basis to social behavior—the project pursued by many scholars, (my friend the late Roger Gould, for example)—is a logical impossibility. It is a dream that leads us on and on and that eventually will simply lead us back to where we started and then run off laughing.

It is also important to notice that cumulation is not the same thing as improvement. Indeed, improvement and cumulation are often opposed. For we can know more about something while forgetting particular details we used to know, and we can create vastly improved methodologies that despite their overall preferability cannot do particular things our older methodologies could and that therefore negate all the knowledge our older methodologies produced.

For example, suppose I want to divide up American incomes into a set of n categories each of which has p members. If my first cut at this has 11 categories and my second has 200, I know that the second is much more detailed than the first and contains much more information. But it does not contain *any* of the information of the first cut, because 11 and 200 are relatively prime. I have improved my current knowledge at the cost of losing all of my old knowledge. This process—redoing data categories without preserving mutual translation—is, of course, endemic in social science data-gathering and is one of the principal mechanisms by which simple cumulation is to all intents and purposes prevented. But if we try to control this incommensurability by producing mutual translation systems—take for example the various attempts to create stable overtime codings of occupations—we end up ignoring historical change in the categories and hence misinterpret it as change in the lives of people flowing through the categories. So you are damned if you do as well as damned if you don't. This model of successively incommensurable bodies of knowledge is of course the Kuhn model. The question is whether the word cumulation means to us Kuhnian normal science—the piling up of commensurable studies—or Kuhnian paradigm shift in reaction to this piling up and the problems it raises.

It is also helpful to extend our theoretical discussion by considering the alternatives to cumulation or even opposites to cumulation. (It is often useful—in conceptualizing a term—to imagine what we think its opposite is.) Now, a first alternative to cumulation is rediscovering the wheel, simply rediscovering exactly the same thing over again. Up to a point, we could think of this as replication—something we don't do much in sociology, but something all of us believe is a part of good science.

But there is the question, here, of what we mean by “exactly the same.” To say that it is *not* cumulation to rediscover the wheel makes sense, but what if we in effect rediscover the wheel, but mistake it slightly and so give it a new name? In this case we have relabeling, in most cases with some kind of drift at the same time. This is a second alternative to our “ideal” of cumulation. And although in some ways relabeling seems a waste of time—I've made snide remarks about it earlier, after all—there are lots of optimization algorithms in which slight and even random rearrangements and recastings of

things are essential to speeding ultimate convergence. So maybe it is better than we usually think.

Moreover, we can rediscover the wheel in a new empirical or theoretical context. Take the literal example of the wheel. The most important part of the wheel is in fact not the piece that goes round and round but the journal bearing—the place where (in nearly all forms of wheels—the Conestoga wagon was an exception) the axle rotates against a fixed, curved, and lubricated surface attached to the moving object, through which the axle receives the weight of that object. The ball bearing replaced the whole concept of a journal (the balls are in effect a moving journal.). That was the literal reinvention of the wheel, but obviously *not* in our pejorative sense. This argument suggests that yet another alternative to cumulation is what we might call metamorphosis. Again, this kind of thing might easily be part of a broader notion of cumulation. So we have as alternatives, so far, cumulation, replication or rediscovering the wheel, relabeling with drift, and metamorphosis. All of these are interesting possibilities for further study.

To close my theoretical remarks about cumulation, let me propose that we think about science as a kind of computational system. For my present argument, it does not matter whether we think of the aim of that computation as finding out the truth or thinking up interesting things to say. In both cases, the computational aim is to optimize the filling of some space subject to resource constraints. There are really two general ways to think about such an optimization, and they imply quite different ways of thinking about cumulation. In the von Neumann model, computation is a sequential set of instructions yoked into a structure by a hierarchical system of rules and laws, with various subparts connected by constant definitions, consistent variables, and so on. This fits very well with the “cumulation as abstraction” model; we imagine ourselves as running a set of programs, which we are ever improving, aiming to find a set of hierarchical rules that will enable us to predict the future. Indeed, our science is in this sense a simulation of how we imagine the world itself to work, for we will ultimately view the scientific laws of society as themselves constituting an algorithm which can be run to predict the future because the elements of it determine that future.

In the alternative, neural net model (I use the Hopfield formulation), computation proceeds by having a bunch of calculators who like neurons take a variety of inputs which they multiply by a set of idiosyncratic weights to determine an output which then goes on to become an input to various other calculators. This is, as should be obvious, not only an empirical model of the brain, it is also a pretty good empirical model of social science; we all read varieties of stuff, take in varieties of data, process it all with our particular versions of methodologies (the weights) and in turn emit our own little pulses into the literature. In this model, convergence means the finding of a local standard, and indeed one of the classic problems of neural computing is finding the pattern of weight assignments that allows the system to have attractors, that is, to have regions of weights in which the system will converge to—in a word, to discover—various different patterns. This does indeed seem like the local cumulation of my generational paradigms.

I think this second computational model is preferable as an empirical model of what actually goes on in social science. In it cumulation is a quite local, noisy, and iterative affair. It is not at all the steady process that I think is implicit for many of us in the term cumulation and that was for my generation the true heart of science. But I think we had better get used to it. Cumulation in sociology is a pretty noisy business, happening at different rates in different areas, and always undermined—system wide—by precisely the problem that local cumulations at all levels are perpetually emerging that are incom-

patible with “cumulation” at the next higher level. It is not at all clear, for example, that there can possibly be a master set of laws under such a model, whereas the von Neumann approach guarantees that we will find whatever master laws there are, even though these might in fact determine only a quite minimal part of the totality of what is going on.

Cumulativity as a Belief

In closing, let me try to redefine cumulation as being not so much a property of scientific systems as it is of the people who inhabit them. I noted at the outset that our data churns and grows at an exponential rate while our general frameworks grow not at all. Cumulation, I noted, was something that happened in between. It is in that sense a kind of clutch mechanism that disengages the rapid motion of the data from the turgid motion of theory. Note that this means that cumulation happens at a pace that corresponds, at least loosely, to the pace of a human life and in particular to the pace of a career. We often feel that we can see cumulation in sociology. It is on “our scale.” We can remember with some self-satisfaction what people used to think about professions or mobility or trust or gender in the bad old days. And we think with a curious combination of satisfaction and dismay of the mixture of discovery and relabeling that we have seen over our own careers. We look around the discipline and see the citations reaching more and more deeply. We watch the generational paradigms flame up with excitement, suddenly enlightening us, showing us new things and casting a new light on old things. Surely all this, we think, is the stuff of cumulation, growth.

But perhaps to say that is to put the matter backwards. Perhaps, rather, cumulation is our name for that motion which we descry in the discipline on the scale of our lives. That is, cumulation is the name we give both to a personal experience and to our normative, somewhat desperate desire that that personal experience be part of a larger whole. In this sense, one person’s cumulation need not be another’s. To the dynamic leader of a generational paradigm, cumulation means the two-decade triumph of population ecology or resource mobilization or affect control theory or whatever. To the acolyte of the great theorists, cumulativeness means the much slower manifestation to all the world of the truly foundational nature of Max Weber or Georg Simmel or W.E.B. DuBois or whomever. Cumulation, that is, becomes our name for that particular type and level of directionality that we hitch ourselves up to in the discipline, that rising conjuncture that we manage to identify with.

From the point of view of my computational models of scholarship, of course, what we really are is *indeed* little pieces of something much larger, something that may not be going anywhere at all. In the von Neumann view, we are little cogs in a giant calculating machine of which we have little idea and whose optimization criteria get changed often enough (by paradigm and methodological change) to preclude convergence. In the neural net view, we are somewhat random processors taking in an idiosyncratic array of inputs and doing a fairly idiosyncratic array of things with them, with only a vague idea of nature of the attractor that defines our part of the social scientific inquiry space and a sneaking suspicion that it, too, changes more rapidly than we think. Either way, to take ourselves for what we really are is pretty alienating, in terms of the traditional ideologies of science with which we grew up; we are not heroes of discovery, but random explorers in Borges’ endless, labyrinthine library. So perhaps an ideology of cumulation is how we make sense of this to ourselves, finding a level of the system with whose direction and pace we feel ourselves comfortable. We can then feel that at least for a while we are part of something that is going somewhere. Maybe that’s the best we can hope for.

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