On Writing The Social Process

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It is an honor to address you today. Germany is the home of the modern research university and Bielefeld is the university of one of sociology's great theorists. So I am indeed pleased and honored to be here.

Among the possibilities I proposed for this lecture, my hosts chose the lecture reflecting on the writing of my current book. But when I sat down to write that reflection, I wondered how to justify so self-regarding an exercise. I have already published a disturbing amount of confessional prose, about a life that is in fact a life of ordinary successes and failures, of high ambitions and less-than-high achievements. I feared lest I wander toward the post-modern self-indulgence that sometimes disfigures the work of my younger colleagues.

I therefore decided to treat this lecture as a small investigation into the habits of scholarship. For the last four years my empirical work has investigated modes of knowing - from the highest forms of archival scholarship to the so-called "research" our undergraduate students conduct on the internet. I have done surveys of student knowledge practices, experiments in modes of reading, investigations of scholars' use of library tools, ethnographies of library behavior, and studies of disciplines. So this lecture shall be one more such inquiry, in this case an inquiry into the life-experience of writing a particular kind of book.

Now of course I need to introduce this topic with an appropriately gracious gesture to my hosts, and that necessitates some kind words about Bielefeld. And now I must tell you a long story about my search for those kind words, a story which, although it may at first appear irrelevant, will eventually provide central pieces of my main argument.

I Chicago and Bielefeld

I began my search for kind words about Bielefeld by idly trolling our library catalogue for books about the city or the University or Professor Luhmann. And since my German is - I am embarrassed to say - quite rudimentary, those books had to be in either English or French. In our catalogue I found a few such things, but only one of them looked mildly interesting: an 1893 book on the subject of Pastor von Bodelschwingh's Bethel colony.

Here, I thought, was a possibility for an opening anecdote. For at the origins of Chicago sociology was precisely the impulse of Christian reformism that had animated Pastor von Bodelschwingh. After all, the second professor to enter my department - Charles Richmond Henderson - was University chaplain as well as Professor of Sociology. Henderson agitated tirelessly on prisons, labor law, unemployment, infant health, and a host of other reform topics. He was such good press that the Chicago newspapers even covered his routine sermons at the University. Thus, we know that he once told students they ought to starve or commit suicide rather than take a bribe in office and that another time he told them that behavior characteristic of the Old Testament would land people in jail today. We know that when the University held a giant reception for the opening of the massive Harper Memorial Library, Henderson urged that the reception be opened to the workers who had built the building "in order to show our appreciation for the dignity of labor." That was in 1911.

So reformism provided a first connection between Chicago and Bielefeld. But there might, I thought, be more. Having published an empirical paper on Henderson, I knew also that he had taken several trips in Germany to visit charitable institutions and prisons, and I knew that he had written extensively about German charities in his book *Methods of Modern Charity* in 1904. Perhaps he had stopped in Bielefeld on one of his four trips through Germany. So I logged into the online archives of the *Chicago Tribune* - the local newspaper, which covered Henderson very extensively - and searched for "Bielefeld" and "Henderson." But I found no evidence. I tried several different forms of his name and - since I know that optical character recognition systems are often unstable - tried several different spellings of Bielefeld. Still I had no luck.

But one of the luxuries of working at the University of Chicago is that I - along with 200 of my colleagues - have a study in the main library, with its 4.5 million items. So a short walk across the second floor reading room brought me into the enormous stacks, where, along with three books on Niklas Luhmann, I was able to pull Henderson's 1904 *Charities* book off the shelf. Sure enough, there were two lengthy discussions of Pastor von Bodelschwingh's colony. So here was clear evidence that Henderson knew all about Bielefeld, or at least that he knew all about what was then one of its best-known institutions.

But there the trail went cold. There were no further references in Henderson's discussion of German charities, so I returned to my library study and searched the *Tribune* a few more times, trying words like "Epilepsy," "Bethel," and so on. There were no new results.

At this point I decided perhaps I should go find the 1893 book on Bethel, which was entitled *A Colony of Mercy: Social Christianity at Work*. It was written by one Julie Sutter, of whom I could discover nothing other than that she was English, that as of 1891 she had translated 7 books from German or Norwegian, and that she had published at least one other book, on urban social problems in Great Britain, in 1901. Unfortunately, the catalogers had put Miss Sutter's Bethel colony book under the Library of Congress heading for epilepsy, RC 395, which meant it was in the science library a block away, where another 2 million items are located, assiduously ignored by the scientists, who now get their ideas from the internet.

It was a lovely spring afternoon as I walked over to the science library. Up and into its stacks I went. I soon found myself with Miss Sutter's book in my hand. There were two copies - one without covers and held together by carefully tied string, the other still effectively bound, but with its cover falling off at the front endpaper. I opened the bound copy. It was filled with underlinings and marginal comments. In the back pages were notes in a curiously cramped but elegant handwriting. That writing looked familiar. A wild thought crossed my mind. I opened the book carefully to its front endpaper. And there indeed was the signature, C. R. Henderson, The University of Chicago, 1894. And the bookplate opposite carried the legend "The Charles Richmond Henderson Library, presented by Mrs. Charles Richmond Henderson." In my hand I held Henderson's own copy of the book on the Bethel Colony, with his own notes, donated along with thousands of its fellow books by his widow on his death in 1915. Incidentally, Mrs. Henderson left also her estate, which provides a fund that annually supports two sociology students for a year of PhD writing.

So I can in fact tell you a bit about Professor Henderson's views of the Bethel colony, although only a bit because his handwriting is actually less legible even than my own. Professor Henderson was deeply interested in the communitarian aspect of Bethel, having written the word "communism" at several points in the text and as a major heading at the back of the book. He paid particular attention to the remarks on unemployment and the German tramping system with its *Natural-verpflegungs-stationen*. Yet not all his judgments of the book were positive. He disputed some of Miss Sutter's medical references and some of her understandings of German social welfare policies. He sometimes disliked the sugary tone of the text, although to be sure at others he seemed to approve passages that were to me horrifyingly sanctimonious, stories of little consumptives giving away their last pennies to children in Africa. To me, these pages evoked the spirit of Dickensian parody, but then I remembered that Henderson's only daughter had died in her twenties. Perhaps these stories evoked noble memories for him.

Henderson's most important question about Bethel concerned adequacy. He saw clearly that such a private system reached only a tiny fraction of the welfare needs of its society. Even if expanded throughout the Empire, it would not suffice. His concluding notes focus on the unserved needy and the necessity for "a union of the complete care of the state and the tender and loving care of religious people as shown in this book."

So that is as much as I can tell you about this particular Chicago Bielefeld connection. My discussion of it has three lessons.

The first concerns the small world. One has only to scratch history the least little bit, and one finds a quite solid and relevant connection between two cities one imagined to be far apart.

The second lesson concerns Professor Henderson's view of his own subject. As his reaction to Miss Sutter's book makes clear, and indeed as I have shown with much more extensive evidence elsewhere, Henderson did not distinguish between "is" and "ought," between, if you will, pure and practical reason. Doing social science and doing reform were not different things for him, as they are for us. For him, to understand why things happened was at the same time to understand how they ought to be changed. This union of theory and practice was of course something Henderson shared with Marx and a host of other nineteenth century thinkers. But such a union is hard for us to imagine today, for we are used to first figuring out what is happening and then figuring out what kinds of policies might change what is happening. We are indeed so used to it that we can only imagine Henderson as "putting is and ought together" when in fact he simply didn't believe there were two separate things called "is" and "ought." This attitude of Henderson's matters to my present topic because - as I shall tell you - I have found myself in the last analysis unable to maintain this distinction, and this reunification is one of several forces that have recast my project of writing about the socal process.

My third lesson moves me even closer to my main topic. It has to do with habits of scholarship as these are shown not so much by Professor Henderson's notes as by my having discovered them in the way that I did. Although I have a reputation as a theorist, I have also done a large amount of empirical work. I did about two years of full-time ethnography in a mental hospital. I have done many historical and library-based projects, have worked in dozens of archives, and am indeed writing a manual on library research. And I have also brought a whole family of computational methods into sociology, sequence-pattern methods now familiar to us all from dozens of search engines, but rather less familiar in 1981 when I first took them up.

In general, it is an important quality of my own habits of thought that I use empirical projects to think through theoretical puzzles. It is thus no surprise at all that when I found myself a little blocked in the middle of Chapter Five of my *Social Process* book, I shifted forward to work on Chapter Six, and then, when after some days I found myself blocked again, I decided to take a break by writing this essay and then, when I started looking for a way to start the paper, got quickly caught up in a very simple empirical research question - what did Chicago sociologists know about Bielefeld? - which led quickly to an even more focused empirical question - how much did Professor Henderson know about the Bethel Colony and what did he think of it? Thus, this little story illustrates a much broader quality of my thinking, which is that I puzzle through theoretical issues by undertaking quite particular empirical investigations.

From what I understand of Professor Luhmann's methods of writing, the move from one text to another text because of blockage was quite typical of him. He would have made the same move from Chapter Five to Chapter Six. But the ending of the chain in an empirical problem was not something he would have done, at least as far as I am able to tell. By contrast, this was a very typical slide for me. Theoretical difficulty always pushes me into empirical investigation, even though my empirical work is often found to be unnecessarily mathematical in its habits of argument and even is thought to be - if I can quote one commentator - "excessively clever." Indeed, perhaps that's why my empirical work is thought somewhat peculiar. It always has a slightly too explicit theoretical agenda.

Thus, the third point I take from this opening story concerns the habits of work that unearthed it. I would imagine that all productive social scientists work on several projects at once. The theoretical question of interest is how those are organized and harnessed into a career of work. I recognize that by Luhmannian standards this is not really a theoretical question at all. But you will have to bear with my American concreteness.

The three lessons of the Henderson/Bodelschwingh story leave us then with three questions. First, the connections between Chicago and Bielefeld: if Henderson knew something about Bodelschwingh, what do I know about Luhmann and how has that influenced my current writing? Second, if Henderson combined social investigation and moral activity, how have I envisioned that combination in the time I have been writing about the social process? Third, what are my habits of thinking and how might they too influence my writing. Having talked a bit about these three questions, I shall close my lecture with a consideration of what is probably Bielefeld's most famous "habit of thinking" - the celebrated *Zettelkasten*.

II Some Notes on Not-reading Theory.

The first of my questions is relatively easily answered. I am largely ignorant of Luhmann's work. The reason for this will take us immediately into my third quetion of habits of work, and, more broadly, of how habits of work fit into the academic life course.

I am ignorant of Luhmann's work not because no one has ever suggested that I read it; several of my Chicago students have in fact done so. But rather I am ignorannt because those suggestions to read Luhmann came after what I shall call "my farewell to theory." The fact is ... that I gave up reading sociological theory for good in 1984. I was a busy man at the time - developing computational methods on the one hand and writing about professions on the other. For the one task I was deeply involved in programming and measurement, mastering methods and in some cases mathematics that I did not know. For the other, I was reading dozens of case studies of professions. With so much to do, I had little time. So I stopped reading theory, although I did not by any means stop thinking theoretically.

My reasoning was fairly simple but underscores an important rule I follow in my work: I judge material by its payoff. When I was a graduate student, I read theory in great detail, like many of my peers. Theory was important. Theory was high status. One scored points in seminars by casually mentioning subtle differences between the Durkheim of the *Division du travail* and the Durkheim of *Le suicide*. But once I started teaching theory to Rutgers undergraduates who didn't care about it, I soon realized that most new social theory goes over the same old turf. So it is wasteful to learn each theory completely, on its own terms and in its own language. You have to translate new theory into the single coherent system you are building up in your head. Then you can tell if anything in it is new to you. But if that is true, then there are two tasks in reading theory; first to translate the theorist's language into your own labels for commonly understood concepts and second to identify what is actually new and add it to your conceptual armamentarium.

The first, translation part of the exercise is wasted time. You don't actually learn anything about norms or habitus or whatever; you just learn how Parsons or Bourdieu or whomever uses those new and unfamiliar names to label things you have long known. This is a waste of time unless you are doing the history of theory or trying to keep up with the young and the intellectually fashionable. Therefore, the real payoff in reading theory lies in the second phase, learning the new ideas that are left over after translation of the relabelings. I gave up reading sociological theory when I began to find that this second phase payoff was not worth the effort of going through the phase of translation. Put more brutally, I gave up reading theory when I found out that most of the time I could think up new ideas faster on my own than by struggling across a long desert of relabeling to find a tiny oasis of novelty.

For me that date came in 1984, when I forced myself actually to teach Anthony Giddens's most recent book and found it so total an exercise in regurgitation that I swore off theory for good. At that point, the only major Luhmann work translated was *Soziologische Aufklarung*, which had been out only two years in English. I hadn't even heard of it. But I had stopped reading theory, so it, like many other works, got ignored.

Thus, I don't know any Luhmann because of strategies I applied at a certain time in my career in order to lower the amount of work I had to do. As I understand it, this is a somewhat Luhmannian argument - we apply order to tame chaos. In this case, for me, the incessant rewriting and relabeling of core theoretical concepts is chaos, and I deal with it by ignoring it.

I should mention that I have another such strategy, embodied in another simple rule. If I make a good faith effort to read a book by an author and I either can't figure it out or don't find it rewarding, I don't bother to read the author's other works. Thus, although I liked Foucault's *Folie et deraison a l'age classique* when I read it in college, I found *Les mots et les choses* impossible and spared myself further Foucault. I recently had to teach *Surveillir et punir* and found that my decision had clearly been the right one. The same thing happened to me with Bourdieu - after trying to read *Esquisse d'une theorie de la pratique* three times, I just gave up on him. As far as I can tell, the word "habitus" is actually a name for a problem, not the solution to it, just like Giddens's egregious term "structuration."

So I am agree with Luhmann that superabundance leads to chaos, and find that rule clearly embodied in my own practices of scholarship. My research on usage of libraries also shows that the chaotic overproduction of scholarship has led to a battle between universal and particular algorithms for knowledge that goes back to the 1920s. I shall speak more of this later.

Before leaving the topic of my embarrassing ignorance of Luhmann, I should note that to say I am ignorant of Luhmann is not to say that I don't know anything about systems theory. That is, just because we haven't read a particular text doesn't mean that we don't already know some of the ideas in it. (As I noted, this premise lies behind the calculus of my "I haven't read theory since 1984" decision.) For example, I read von Bertalanffy in college. I read Wiener and talked communication theory with my father, an electrical engineer. I knew Heinz von Foerster's son intimately and was aware of his father's work. Having once dated Jay Forrester's daughter I knew all about the DYNAMO simulation system, and having married a physical chemist, I knew all about Ilya Prigogine. I had worked on recursive programming structures and elementary graph theory at odd moments in graduate school and would learn the mathematics of compression mappings when I wrote *Chaos of Disciplines*. Sequence analysis would eventually introduce me to the extraordinary self-organizing power of optimization through Gibbs samplers. So systems and self-organizing systems and so on were and are pretty familiar to me.

That I knew all this stuff about systems does not, however, mean that I had a focal idea of systems. Indeed all this knowledge of systems had nothing to do with the title or the concepts of my first book, *The System of Professions*. The book's title does not refer to "system" in any Bertalanffian sense. My very first paper on "the system of professions" defines that system simply as the ensemble of mutually constraining professions and semiprofessions. What made that ensemble "systematic" was not any self-organizing property, much less any set of rules governing the professions as an ensemble, and least of all any functional differentiation. It was simply the fact that the histories of professions were not mutually independent, but rather parts of an ecology of mutual constraints. So there was no formal connection with system theory at all, and I often think that I would retitle the book if I were publishing it today, because many people - including some who claimed to have read it - found in it an autotelic character that is simply not there.

Moreover, the book illustrates, in a fractal manner, the same strategy of "ignorance as defense against chaos" that I just mentioned as a much more general habit of my mind. I did not at any time while writing that book read about the professions more than one or two theoretical books or *any* theoretical articles. I read only historical case studies. Because I was attending a weekly seminar on the history of professions, I simply tried to build a "theory" of professions that gave me something interesting to say no matter what the occupation we were discussing and no matter what the time period involved. If I had read theoretical writing about professions in sociology, I would have thought that everything interesting in that "theory" had already been said, and would therefore not have bothered to write the book. Only while the book manuscript was being reviewed did I read the theoretical literature and sprinkle it into footnotes. The references to others' theories are therefore just dutiful Christmas decorations, not references to influential sources.

This little story however captures another aspect of my habits of work, or perhaps of my mind. I have a mind which tends to see the similarities in things rather than the differences between them. That is why, if I had read the professions literature, I would have thought that everything had already been said: I would have seen prefigurings of my own arguments in places where others did not see them. This habit of seeing similarity is also why I can think I know something about systems theory when what I actually know is the grab bag of things I listed for you earlier.

The deep premise of such a strategy is that it doesn't really matter much what particular material in a general area you know; you have to know only enough about that area to be able to regenerate, with your own reflection in a relatively finite amount of time, the major theoretical concepts and insights of that area. That is, you conserve on memory not by offshoring and indexing, but by populating your mind with relatively general algorithms for reimagining theories given a few random pieces of them. It is thus little surprise that I like the Gibbs sampling algorithm, which follows precisely this logic.

Thus, in addressing my first question - my ignorance of Luhmann - we have uncovered partial answers to the third, the question about habits of work. We have discovered several alternative stategies for dealing with the chaos and complexity of knowledge: ignorance, regeneration from random bits, and satisficing. We have also uncovered a habit of mind that may shape one's choice among such strategies: the tendency to see similarity or difference. I think we can also see in these stories a certain emotional quality to the mind, a desire to be advancing at a certain rate, an impatience. Most of my decision strategies for reading and non-reading involve exasperation or boredom as their major trigger; I have to be getting new ideas at a certain rate or I am bored; this too flows from the habit of seeing things as similar: boredom comes all too easily.

III Writing *The Social Process*

Let me now turn to the particular issue of writing *The Social Process*, my current book (actually there are four current books, for reasons you will soon hear.) That will bring us yet closer to my two remaining questions - the relation of moralizing and theorizing in sociology and the various strategies for writing and developing books.

Since I finished *System of Professions* in late 1986, I have always had it in mind to write a major work on the place of time in social science. Temporality and its place in sociological methodology had become a focus of my empirical work in the 1980s, growing out of polemical arguments about time that in turn grew from my attempt to defend historical sociology against quantitative sociologists who thought it nonsense. By 1988, when *System* appeared, I had articles in print on the fundamental temporal assumptions of variables-based sociology, I had adapted computer sequencing algorithms to the study of social data on careers and histories, and I had written a good deal about the philosophy of time and history. I had also given - at least in my own view - a formal theory of contingency, along with empirical analyses of three contingent work arenas, in *System of Professions*. Obviously the next step was to write a book about time.

The history of this obvious book has now taken the 21 years from 1988 to the present. Yet I am at present only halfway through the current outline - only six chapters are in full draft. I am presenting these chapters here at Bielefeld, and indeed I have to confess that part of my reason for accepting the invitation was, quite literally, to force myself to write the last two of these chapters. But why is this taking so long? After all, since I began this book, I have published four other books and about sixty other articles and chapters. What, then, can possibly be the nature of this book that I keep in the background? What is a book that takes so long? Am I like Penelope in the Odyssey, unwinding each night what I have supposedly done during the day?

Let me be frank about some initial facts. I know as well as the next man that being a person who is in the process of writing a major book is in many ways better than being a person who has written a book that has fallen well short of its ambition to be a major book. Like sexual partners and tourist attractions, a book in anticipation is often better than a book in reality. So one force slowing me down has been the generic fear of failure. But I am also well aware that having already published a work that both I and others could see was magisterial and important, I had raised the bar on myself very seriously. Writing something to come after *System of Professions* was in some ways an impossible task, and this more specific fear of failure had also its effect on me.

In fact, the earliest design of my time book was quite straightforward. There were to be two major sections: a first section on theoretical issues involving time in social science and a second section looking at the ways time was in fact handled in empirical social science. The first section would have an introduction, a chapter looking at models for time and their philosophical assumptions, a chapter on sequences and formal models for them, and a chapter on examples of historical analysis of causality. The second section would have a chapter on stage theories, a chapter on career theories, and a chapter on interactionist models. There would be a methodological conclusion.

Such a book was in fact the logical summation of my methodological and theoretical work of the 1980s. It argued that time mattered in social life, that our theorists' notions of time were impoverished because they were clock-like rather than truly historical, and that our methodologists' approaches to temporality were little less than absurd. The book would think the issues through carefully, examine the riches of existing work that *did* take time seriously, and come to some kind of judicious conclusion.

Of this book were written the introduction, a second chapter analyzing the philosophical assumptions of four articles on labor force mobility, a gracious metaphorical account of the statistical problems raised by temporality, and most of the chapter on causality. Even as I wrote these chapters, however, I gave up on the book. For I was drifting into a general analysis of disciplinary differences, from which was coming a whole new theory of the philosophical assumptions of social science. Moreover, at the same time, my methodological work was ignoring causality altogether, pursuing pure pattern-recognition. And my reading in the philosophy of history was demolishing my earlier faith in the narrative past. Thus, in my new papers of those years I pillaged these early chapter drafts for useful text, probably knowing that the book would never be written as then planned.

Moreover, one new paper destroyed the entire plan. My 1992 ASA paper finally confronted the contradiction between my methodological work with its implicit assumption of binding narrative trajectories and my theoretical work with its insistence that nothing exists outside the present. With a concept of "encoding" the paper explained how an instantaneous social process could nonetheless appear to sustain action-at-a-temporal-distance. Never published, this paper required that my "time" book address time and social structure simultaneously, for social structure was simply the memory of the past of the social process, and encoding the means by which that memory was perpetually replenished. Meanwhile, my work about disciplines evolved steadily, producing a series of empirical analyses (in 1990, 1991, and 1993) as well as a flashy theory lecture in 1993. The disciplines work was organized around the concept of fractals, which seemed to handle most of our *Methodenstreiten* with a minimum of unnecessary verbiage. In parallel, I was reading and writing in the philosophy of temporality for social science. I had written a large paper based on a careful reading of Bergson, Mead, and Whitehead and considering in detail an empirical case - the invention of a certain whaling device. Thus, in the mid 1990s I was, first, again using empirical work to think about theory, and, second, working in three slighly different areas - temporality, disciplines, and social theory. (I won't mention the book on education I drafted in that period - it too is unfinished.)

Thus, the next time I sat down seriously to work on my "big time book" - now metamorphosed into a big "time and social structure" book - I was deeply confused. It was 1997. At the behest of my colleagues, I had just finished an immensely detailed history of the *American Journal of Sociology*, so my empirical mind was full of the problems of reconstructing the past as it had been as a present. I had three months in Oxford on a prize fellowship. I felt that I simply had to work on the "big book." So I sat in front of a computer and rapidly typed out four chapters of a wholly new book. By moving social ontology to the level of pure events, I could obliterate the structure and agency problem, resolve the problem of causes with differing time scales, and address many of the major sticking points of social theory. There was just one difficulty: to come up with an account of stability in a world made of nothing but events.

Because the argument was revolutionary, I could not figure out how to order it. Each chapter simply went over the preceding material in further detail or from a different angle. I wrote an introduction plus chapters on events, action, and structure. I had decided to finish off the book with a second half on applications, somewhat like the review of existing literatures in my preceding version, but now organized around the major issues of epistemology rather than ontology: discourse, causality, methods, explanation, and programmatics. Of these chapters, only causality was partly written, and only causality and methods were survivors from the earlier design. While thinking about explanation later in the year, I wrote up a quick draft of a paper for a speaking obligation. I also firmed up the causality chapter into a published paper. These too showed a characteristic and quite bad habit, particularly in those overworked, unscheduled years. Ideas and writing that belonged in major and substantial designs were ruthlessly pillaged for immediate speaking needs and then inevitably turned into smaller published papers.

On my return to Chicago in the summer of 1997, I took stock. My life was now full of half done projects. It was clear that I should finish them before turning once again to the book on time and social structure. Because the book-length history of the *American Journal of Sociology* was the most complete text, I combined it with two existing papers on the Chicago School and wrote a bibliographic and historiographical introduction in order to make it a coherent book. All the same, I could not resist using crucial concepts from the torso texts of the time book. So the ideas of lineage, eventness, braiding, and stability were used in that book, which thus turned into a kind of meditation on the nature of social stability in time, using the journal's history as an exemplary narrative. Once again, empirical work was used as a vehicle for theory. This book - *Department and Discipline* appeared in 1999.

Next on the agenda was the fractal material. I knew that if I did not make this into a separate book it would overwhelm and unbalance the time and social structure book. I already had three empirical studies of fractal processes on hand plus a theoretical introduction. I wrote a conclusion to the existing four chapters on fractal cultural patterns, expanded my early unpublished theoretical paper on self-similar social structures into a sixth, theoretical chapter, and wrote a new, concluding chapter on fractal moral systems. I knew that the latter two chapters, although among the most original things I have written, would get lost in this design, overshadowed by the fractal cultural structure material, but I did not have the ideas or the time to expand them as they deserved. Unlike *Department and Discipline*, this new book - entitled *Chaos of Disciplines* - was not influenced by the ideas in the time and social structure book. Rather, it removed unnecessary detail from that book. The fractal mechanisms could merely be one part of my eventual theory of stability. Note also, however, that the final chapter of this book was my first real venture into normative thinking in print.

It was now 2001. I had now been thinking about a time book for more than a decade. I was panicked about being scooped. Unable yet to complete the monograph, I decided to publish a collection of already published papers on temporality and theory to claim the turf. Like *Chaos of Disciplines*, this book (*Time Matters*) appeared in 2001. It concluded with a short sketch of the argument of the time and social structure book as I then imagined it. This I hoped would serve as a placeholder until I could write the book.

Meanwhile, while finishing *Chaos of Disciplines*, I took what had been planned as the explanation chapter of the 1997 version of the time and social structure book and made it the foundation of a little book about heuristics. I was teaching the entry course for graduate students at the time and was trying to make them see how to generate new ideas by simple heuristic tricks. This book - *Methods of Discovery*, which appeared in 2004 - was easy to conceive, but laborious and even boring to write. It required dozens of examples, each one to fit a particular type of heuristic. But it helped the time book by removing yet another piece of the irrelevant material. I had now removed explanation and causality from the second half of the book, turning them into a freestanding book and a published article. I had given up on the topic of discourse as a waste of time, and had decided that *Time Matters* contained all the methodological polemics necessary in one life. So the entire second half of the 1997 plan for of the book was now gone.

Therefore, in 2003 I designed the book once again. I had in the interim recognized that while I had a complete social ontology, I had very little to say about the actual links between events. More specifically, although I was loosely using the Whiteheadian distinction of prehensions and determinations - meanings and actions, one might say - I in fact had no real discussion of meanings and actions. So I left the ontology at the beginning and sketched in a second section on emotion, action, and meaning. More important, I taught reading seminars on each of those topics in the subsequent years and started to take detailed notes on my thoughts about them. The reading courses largely focused on classical materials I hadn't read for many years or hadn't read at all.

Thus the book now had a new and more realistic design. But in 2003 and 2004 what I did in terms of writing was to rewrite the existing process ontology sections, finally getting them right. Like Penelope, I rewove the cloth once again. Merging time and space into the single concept of locality anchored chapter 3 and specifying the concept of lineage now anchored chapter 4. Moreover, the merging of time and space in Chapter 3 meant that "time and social structure" was no longer an appropriate title, so I renamed the book *The Social Process*. By 2005 I had made an even more radical move. For a 2001 conference I had written a paper on concepts of outcome that challenged most of social scientific conceptions of "the good." Three years later, I decided to take a similar process-oriented approach to social order and found a conference venue for that analysis. I now decided to finish the book not with methods or applications; my views on those matters are well known already. Rather I would follow the lead of *Chaos of Disciplines* and conclude with the explicit analysis of values.

Here Charles Richmond Henderson becomes relevant again. One of the things I most admire about Henderson was his lack of a certain kind of alienation. Like all of his pre-war peers, Henderson lived his social science: there was not one social science that he wrote and another one by which he lived. By contrast, we are quite different. Most of us write about the social world as if it were determined, but we live in it as if we were free Kantian individuals. I decided to renounce that comfort. I would end the book with normative conceptions of outcome and order. These would of course be processual; both of the existing manuscripts took that approach to social life

two normative chapters two more - chapters on power and difference. They too have become seminar reading courses and occupy pages and pages of my notebooks. The book design at last has the coherence and balance. Now I just have to write it.

I have also lowered my ambitions for the book. I no longer care about the hundreds of books I haven't read about time - Heidegger's among them, by the way. I simply want to write some things that have mattered to me about social life and that I tell my students when I talk to them about my views. If the book is useful to others so be it. But I write not for the ages, but simply because I told myself, many years ago, that I would answer these questions to my own satisfaction before death. Death is a lot closer now - cancer has made that clear - so it is time to sit down and write.

IV Zettelkasten and the Problem of Knowledge.

I have then answered my three basic questions. I have first told you something about what I know about Professor Luhmann and how that has influenced my current writing: that I know little of his work, that my thought emphasizes process more than system, but that we share a certain abstract habit of thinking. I have, second, told you how I have envisioned the combination of social investigation and moral activity. I am coming to think they are indistinguishable and shall not only include normative chapters in my social process book, but shall also undertake a detailed analysis of the essentially normative basis of all action in the core chapter on action. Third, I have told you a bit about my habits of work and how they influence my writing: I have several projects going at once, I pillage ongoing projects for immediate needs, and I use empirical investigation to think through theoretical problems. I have also told you that I tend to see similarities where others see differences and that I have a variety of strategies for dealing with the overwhelming chaos of scholarship: ignorance, satisficing, rigid decision rules for the reading of others' work.

I want to close by reflecting about what is to me the most interesting point of difference between Professor Luhmann and myself, our methods of filing and anotation. I come at last to the Zettelkasten.

I need not elaborate the Zettelkasten system for this audience. But perhaps I do need to translate it into formal librarians' language. The Luhmann system as I understand it combines several classic tools. The first is accession numbering: every card gets a unique number. The second is a generalization of what librarians call faceted classification, first promulgated by the Indian library theorist S. R. Ranganathan: there is no limit to the number of implicit links between one card and another, and links can follow various traits, not simply a hierarchical organization in terms of an antecedent controlled vocabulary. The third is what we might call reference listings: a card can refer directly to a large although limited number of other cards. Fourth, there is an index, although unlike many indexing systems, this emerges after the fact and is empirically rather than theoretically derived.

Up to this point, a Zettelkasten card is essentially analogous to a book in a library, which has a unique bibiliographic identity, which refers to a wide variety of other books and materials, which uses an idiosyncratic vocabulary, and which is located in dozens of partial indexes to a library - from the card catalog to reference works to the books themselves (each of which is, after all, a partial index to the library). The Zettelkasten system, as several others have pointed out, is more or less analogous to a standard library with its books and its ongoing indexing and reindexing.

There do seem to me a number of unique characteristics to the Luhmann system, however. Of these the most important is heritabilty in the sense that term is used in object-oriented computer programming. New cards growing off a particular branch inherit the qualities of that branch. This heritability seems not however to be of great depth. Rather like the lineages of Evans-Pritchard's Nuer, it seems in practice only a few generations deep. But it is not clear - at least to me - to what extent such a system actually forgets its past. Presumably this is a function of the relative density of new references and the time periods chosen for modifying the index.

My own systems of note-taking and personal memory are quite different. Although I am considered by colleagues and students to be an obsessive filer and record-keeper, I have spent much of my life regretting that I am not more organized. I have rediscovered the same old ideas many times. I have forgotten things and lost them forever. I have been utterly bewildered by my own notes in the margins of old books. Yet there is a fundamental unity to my work, a set of habits and styles and even problems that give it a consistent structure. This originates in the phase of academic work that was not covered in what I found in English about the Zettelkasten - the phase of thinking.

So let me speak about my own note-taking and work practices and then talk about how these relate to my thinking. Early in graduate school, I used notecards organized in a personal, hierarchical classification. I have perhaps four thousand such cards. I haven't looked at them in thirty years. I also took notes in the margins of books, many of them paperbacks whose bindings are now broken. I often buy new copies of old classics, to escape my old notes. Indeed, one look at any of this material tells me that I am no longer the same human being who took these notes: I have new preoccupations, new definitions for basic terms, deeper understandings of things I didn't know as a graduate student, and so on. Most important, when I look at the notes I wrote thirty years ago, I can see that I was a defenseless young intellect who had no internal frameworks with which to judge what I was reading; rather, I was learning a view of the world from the texts, at the same time as I was pretending to analyze them and to figure them out and even to judge "what they said."

By contrast with these reading notes, myPhD thesis on the history of American psychiatry was an immense empirical undertaking and of necessity required extensive paper files. For my dissertation, I evolved a filing system that I continue to the present. Every empirical project has a master folder containing a number of subfolders. These subfolders contain the original ideas, the business and other correspondence, the needed acknowledgements, the initial bibliography, and the drafts of the ultimate paper. From this I gradually separate out the following folders, once they grow too thick to be subfolders. First, a bibliography folder, with a log for bibliographical activity already accomplished as well as printouts of bibliographies and lists of open bibliographical trails that need to be pursued at any given moment. Any specific physical copies - I hate reading things on screen - will be in subfolders here. Second is an ideas folder, into which I throw pieces of scratch paper with ideas written on them. Every now and then, I will troll through the idea folder and reorder it, but generally my principal ideas stay at the front of my research consciousness without needing reminders, and I typically find my ideas folders full of unused thoughts if I look at them after a paper is written. Third are data folders, often broken into subfolders for specific types or sources of data. These can be xeroxes, or hand-copies, or sheets of paper with reading notes or whatever. Fourth are analysis folders, typically one for each little block of analysis done - calculating the average size of university libraries from a collection of published data, developing a time line for occupational therapy as a profession, making a list of all references to Charles Henderson in the *Tribune* ordered by year and another ordered by venue, etc. Typically, these analyses will become parts of papers, and like most empirical workers I write analysis sections long before I write the theoretical ones. My analysis folders are sometimes written by hand and sometimes by wordprocessor.

I should note here, by the way, that I am completely in agreement with Professor Luhmann that it is best to waste little time redoing things that don't need to be redone. I have therefore used the same wordprocessing system for my entire career, Wordstar 3.24 from 1983. Every research text I produce, including this one, is done on this wordprocessor, which occupies a grand total of 80 kilobytes and runs only on DOS machines. As a result, I have never wasted any time learning new wordprocessors or dealing with new editions of Microsoft Word or being unable to read old files or having my research files damaged by viruses. I have probably saved at least two years of my research life by doing this.

In terms of amount, a project like the paper on Henderson that I mentioned at the outset involved about a linear foot of material, probably something like 1500 sheets of paper and copies, sorted into about fifteen main folders subdivided into from two to six subfolders apiece. My PhD thesis, by contrast, comprised about eight linear feet of material, probably 12,000 sheets of paper, organized in about 100 folders, some of which were data folders containing coded abstracts of the lives of a thousand psychiatrists.

So my empirical work produces a lot of highly organized paper. I teach this system to students learning about library research, and they seem to love it, even the most technologically sophisticated of them. They have no skills of project management whatsoever, having grown up with the belief that everything can be done on a just-in-time network basis. A system of filing and control, even if it embodies the principles of hierarchy and linear order that they affect to despise, is very, very welcome to them. For it enables them to manage their research.

So far, I have discussed by system of folders for empirical projects. My theoretical work has quite different kinds of files. In the early days of my career, I developed files for theoretical papers the same way I developed empirical files. In part this was because many of my early theoretical papers took the form of *analyse de texte*; they were in fact "empirical investigations" of prevalent philosophical assumptions about time and methods. By contrast, *System of Professions* was organized around a set of data folders containing hundreds of articles about professions. There were only five or six folders of general theoretical material, but then there were three files each for dozens of individual professions and pseudo-professions, one for the US, one for England, and one for the Continent.

But the book that has become *The Social Process* is based on folders that grew in rank profusion out of the historical sociology course that produced my original focus on narrative and order in human experience. These were idea folders purely, many of them filled with copies of the relevant articles or chapters, often with extensive comments. But also in these folders were pages and pages of handwritten, barely legible meditations on yellow lined paper. By the late 1990s, this material had grown to perhaps ten linear feet, containing everything I had ever read about time, or thought about reading, as well as thousands of pages of reflections and personal ideas. Assuming 25 lines of text or material per page, there are 1.8 Kilobytes of ASCII characters per page, which means that ten linear feet totaled perhaps 25 to 30 megabytes of material. It was in 150 to 200 folders, categorized by a system that had changed drastically over the years since these files saw their beginnings in 1980, recast with each redesign of the book, although never fully refiled from scratch.

I last looked at those files seriously in 1997. For there is nothing more disheartening than looking at 25 Megabytes of material and feeling that you need to refamiliarize yourself with all of it before writing something. Indeed, I am reminded of the famous story told about Harold Laswell, how he used to be an empirical political scientist, and then he decided to leave Chicago for Columbia. He put all his files in a truck and sent the stuff to New York. But somewhere it all disappeared. He became perforce a theorist and enjoyed a far better career than he had had as an empiricist.

The reality is that the only way for me to write a sensible theoretical book is simply to sit down and write what is in my head. For me, at least, it is not having notes that enables my scholarly writing. It is *writing* notes that enables my scholarly writing. The act of writing notes fixes things in your mind. Not that you remember them in particular. But rather you process them through your theoretical approach to the world, the framework that exists in your head. If it doesn't handle them well, you change that framework a bit. Gradually, over the course of reading thousands of things, the framework in your head gets more and more effective. Teaching also improves it, for you must simplify it radically in order effectively to guide young scholars through puzzles and material that are unfamiliar to you.

As time goes on, you come to realize that your system isn't changing much with new readings and challenges. Like a sequence in calculus, it eventually converges. Now is the time to sit down and simply write it out. That's what I did with the theory in *System of Professions*. I simply sat down and wrote the view of professions that I had debugged on hundreds of case studies. The core theoretical chapters were written very, very quickly. I simply sat down and wrote what was in my head. I think this kind of approach has worked for me because I had very strong mathematical and logical training and so tend to be careful about the mutual consistency of my arguments, the clarity of my definitions, the problems raised by unexamined implications, and so on.

This worked for *System of Professions*. But eventually even I felt that I had too many ideas going at once, and that the dangers of mutual inconsistency were high. It's also true that I'm probably losing brainpower, so I just can't maintain the sheer memory frames that I used to maintain. So I moved to a formal notebook system in 2004, at age 56, after a lifetime of happily forgetting things. My notebooks are not Zettelkasten. For I do have a set of basic terms and concepts, and it has been my goal through most of my career to develop a loosely-axiomatic basis for my general theoretical thinking. Such an approach - carefully logical without being rigid - seems to offer me the best combination of logical discipline, comprehensiveness, and productivity. The reader of my books will always find a list of concepts in the indexes, with the page number where the definition is given. I have indexed all my books myself and have made sure that their logical foundations are absolutely clear in their indexes.

Thus, my notebooks are based on a rough controlled vocabulary, most of which is terminology I developed either to teach introductory sociology at Rutgers in the 1980s - a task that requires drastic simplification and clarity - or to further the writing of the book I have been speaking about this afternoon. To the extent I need new terms, I simply coin them. As for a new idea, when I have a new idea, I write it on a new page and assign that page the terms I think are best. And I immediately enter the page numbers under those relevant terms in the ongoing index at the back of the book. My thoughts are thus continuously subject-indexed in a set of terms that I develop as I go along and that have an imposed logical consistency.

I do not by any means write down every idea I have. Only the good ones, the dangerously forgettable ones, the new ones. I fill one of these notebooks about every eight to twelve months. There are presently eight of them, of two hundred barely legible pages apiece. It is thus not at all difficult for me to review the best of my thinking on any topic for the last seven years in one or two afternoons. It's simply a matter of reading the indexed pages. I could of course maintain this index on a computer and avail myself of key-word indexing. But my opinions of keyword indexing are pretty negative. And more to the point. to write an idea down by hand one must cherish it, and fondle it, and come to know its idiosyncracies, in a way that one does not if one simply types it quickly into the machine. An idea will not have done its work of changing what you think unless you give it the time to do so. For me, writing the idea by hand takes that time.

Behind this approach to note-taking and indeed to thinking lie some very profound beliefs about knowledge and knowing as well as some adjustments to my personal idiosyncracies. As I said earlier, I am a man to whom lots of things look alike. Analogy comes easily. Coming up with new relations between things, even bizarre relations between things is not difficult for me. Indeed, one of my books is about techniques for this very task. Thus I don't need a lot of connections betweef things. Quite the reverse, I need a way to control the chaos of all the different skills and random knowledge and endless serendipity that goes with the kind of analogic mind that I have. For me, developing a controlled vocabulary and a mental framework for analysis constitute that control.

In summary, I take notes and develop files in a very different way than did Professor Luhmann. I rely on a hierarchical file structure for my empirical work. I keep my reading notes by project rather than all together. I have an indexing and terminological system that is convergent rather than perpetually emergent. This reflects a mind that is highly analogical, that has been unable to decide between scientific and artistic modes of knowledge, that has very catholic empirical tastes, and that is easily bored. But it also reflects an ideal of knowledge and knowing. I do not conceive of optimal knowing as the subsuming of the largest number of facts under a single framework. I am more interested in things like coherence, generativity, and plenitude. I want my thinking to be strongly consistent withou being narrow-minded. I want it to be productive for other people, especially for the young people I advise. I want it to fill the space of possible social knowledge rather than simply carving up one little area with enormous precision.

But for the moment, I just need to finish the damn book. It's a question off sitting down and organizing the most complex thing I have imagined in my entire life. Wish me luck.