

CAREER STAGE AND PUBLICATION IN AMERICAN ACADEMIA

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Abstract This paper first outlines a processual approach to social life and then applies that approach to the changing social structures of American academia. Substantively, it argues that transient demographic patterns in the early and mid twentieth century shaped our ideal picture of academia: small disciplines, expanding rapidly in the face of seemingly infinite student demand, meant young disciplines, a seller's market for PhDs, and a lack of publication pressure. The end of demand expansion meant rapid aging, a buyer's market for PhDs, publication pressure, excessive publication, decline of reading, and consequent disciplinary confusion.

Keywords: sociology, disciplinary demography, scholarly careers, publication.

Resumo Este artigo apresenta primeiro os contornos de uma abordagem processual da vida social e, em seguida, aplica essa abordagem às estruturas sociais em mudança da academia americana. Argumenta-se que padrões demográficos transitórios no início e em meados do século XX moldaram a nossa imagem ideal da academia: pequenas disciplinas, expandindo-se rapidamente face à aparente infinita procura por parte dos estudantes, significava disciplinas jovens, um mercado de vendedores de doutoramentos, e uma falta de pressão para a publicação. O fim da expansão dessa procura significou um envelhecimento rápido, um mercado de compradores de doutoramentos, a pressão para a publicação, a publicação excessiva, o declínio da leitura, e a consequente confusão disciplinar.

Palavras-chave: sociologia, demografia da disciplina, carreiras académicas, publicação.

Résumé Cette article commence par présenter les contours d'une approche méthodologique de la vie sociale, puis il applique cette approche aux structures sociales en changement de l'université américaine. Les évolutions démographiques transitoires du début et du milieu du XXe siècle auraient façonné notre image idéale de l'université: petites disciplines, qui connaissent une expansion rapide face à l'apparente demande infinie de la part des étudiants, signifiait disciplines jeunes, un marché de vendeurs de doctorats et un manque de pression autour de la publication. La fin de l'expansion de cette demande a entraîné un vieillissement rapide, un marché d'acheteurs de doctorats, la pression autour de la publication, la publication excessive, le déclin de la lecture et la conséquente confusion disciplinaire.

Mots-clés: sociologie, démographie de la discipline, carrières universitaires, publication.

Resumen En primer lugar, este artículo presenta, los rasgos de un abordaje procesual de la vida social y, en seguida, aplica ese abordaje a las estructuras sociales cambiantes de la academia americana. Se argumenta que a inicio y a mediados del siglo XX patrones demográficos transitorios moldearon nuestra imagen ideal de la academia: pequeñas disciplinas, expandiéndose rápidamente frente a la aparente infinita demanda por parte de los estudiantes, reflejaba disciplinas jóvenes, un mercado de vendedores de doctorados, y una falta de presión para la publicación. El fin de la expansión de esa búsqueda significó un envejecimiento rápido, un mercado de compradores de doctorados, la presión para la publicación, la publicación excesiva, el declive de la lectura, y la subsecuente confusión disciplinar.

Palabras-clave: sociología, demografía de la disciplina, carreras académicas, publicación.

Introduction

Young social scientists today face an intense pressure for publication. This new pressure arises from essential changes at the heart of academic life. The problem of career and publication thus cannot be addressed without a more general theoretical account of the relation between careers and academic knowledge.¹

This question of careers and types of knowledge is a particularly enticing one for me, because it is located at the intersection of two lines of my work. One line of my work has concerned social ontology (see, e.g., Abbott, 2016). I have long been studying the nature of the entities and structures that make up the social process. The other relevant line of my work concerns disciplines and the theory of knowledge. I have recently been preoccupied with the problem of what is “good” knowledge and what are the social processes that are necessary to produce it (Abbott, 2011b). The problem of career stages and publication provides a useful site to combine these two lines of inquiry in order to address a particular practical problem.

I begin with our common sense idea of career stages and the knowledge appropriate to them. Then I will discuss some stylized facts that seem to challenge that common sense idea. The contradictions among these stylized facts will then lead into a more detailed theoretical analysis. That analysis begins by sketching a new social ontology. I then apply this processual ontology to the case of the social sciences, leading into a demographic analysis. This demographic discussion in turn leads to a conclusion that briefly discusses possible ways forward from our present difficult position.

The common sense idea and some stylized facts

In our common sense conception of a discipline or a form of knowledge, careers and knowledge run together. Each scholar learns some basics in graduate school, develops those basics into a research program as a young professor, and then pushes the research front a little further throughout the subsequent part of his career. As a senior scholar he or she may produce some culminating work, drawing together a lifetime of thought and reflection. That is the modern scholarly career as we understand it today.

1 This paper was written for a talk at ISCTE-IUL in Lisbon, which was given 6 May 2014. My hosts kindly solicited the paper for publication. But with one thing and another, publication has been delayed to 2019. Although I have written a number of new papers in this area since that time, I see no reason to modify the central substantive argument of this piece: that transient demographic patterns in the early and mid twentieth century shaped our ideal picture of academia, with the result that our image of the basic structures of academic life — careers, publication, advancement — is at variance with the disciplinary structures that are possible under the demographic realities of the present. Therefore, in editing for publication I have only corrected a few matters of detail (e.g., I am no longer the editor of the *American Journal of Sociology*.) I have also left the paper in the tone of an oral presentation, rather than modifying it into the tone and detail of a research article.

In this conception, the research front of the discipline moves forward as the individual scholars push it, and so the advance of the discipline and the advance of individual careers are more or less the same thing. Eventually, of course, scholars retire or die, but we usually imagine that they stay current with the latest research throughout their careers. Knowledge is cumulative, in this sense, both within individuals and within the discipline. Of course, there are some individuals who drift away into this or that unorthodox position, and there are some who fail to keep abreast of the major developments of the field. But for the majority, advance of discipline and advance of career are identical. This identity of discipline and career is implicit in the fact that graduate students often seek the most senior professors to be their advisors.

But four facts raise some questions about this common sense view, at least with regard to the academia of today, as we see it in the United States.

First fact: Forty years ago, almost no graduate students published in the *American Journal of Sociology* (AJS). In 1974, of 63 total authors, 3 were graduate students. As late as 1990, only 4 out of 64 AJS authors in that year were graduate students. But in 2013, 10 of 64 authors were graduate students, and another 19 had gotten their PhDs in the last five years. Indeed, 10% of all published papers are sole-authored by graduate students, and another 15% have graduate student co-authors, evenly divided between lead authors and trailing authors. Thus, my first fact is that very junior scholars are becoming a dominant presence in the oldest and most prestigious sociological journal in the US. Given our common sense conception of career, this is very strange. No one actually thinks that such young scholars are intellectually ahead of the hundreds of prestigious older scholars who did not publish in the AJS in 2013.²

Second fact: Eminent middle-aged scholars in American sociology have recently founded an online, open-access journal (*Sociological Science*) that shrinks the normal peer review process by guaranteeing an up-or-down decision on any paper in one month (as opposed to the four months typical of the leading journals). There is no commentary on this decision — no reviews and no editorial justification. Indeed, the decision is, in effect, the simple judgement of the deciding editor to whom the paper is assigned. It is thus not clear what exactly is meant by “peer review” in this journal — the decisions could be completely arbitrary. But the move reflects the frustration of these middle-aged scholars with what they call “hidden co-authors,” meaning by that phrase the peer reviewers who have held them to “external” disciplinary standards. Interestingly, this new journal returns sociological publishing to its state in the 1950s, before the advent of modern peer review. But more important, it suggests that these scholars feel that their right to report their own results in the ways they choose is more important than their submitting to the normal process of peer review.

Third fact: Most senior American sociologists are not dominant and generative figures at the top of cumulative pyramids of knowledge. Rather, they have the

2 These facts come from the files of the *American Journal of Sociology*, of which I was Editor 2000-2016.

routine experience of reading in the published work of younger scholars ideas that they themselves published many years ago, even though the younger people claim that these ideas are completely new, typically by assigning new names to them. Senior scholars also watch their methodologies and their theories wither in the literature, not because of any decisive rejection, but in a seemingly arbitrary fashion. The rapid disappearance of log-linear analysis is an excellent example. So also is the relegation of the 1970s generation of historical sociologists to the so-called “second wave” and the relegation of the postwar feminist tradition to a similarly passé “second wave.”³

To summarize my first three facts, the common sense concept of career is not the one we observe today. Careers are not logical trajectories from training, to program of research, to dominance and summary. Third-year graduate students publish in major journals; scholars who feel insufficiently published start new journals for themselves; senior scholars are not usually heads of dominant schools of sociology.

But now let me present a fourth — and somewhat contrasting — fact: Despite these diverse and somewhat jarring experiences of individual scholars, the American discipline of sociology as a whole seems to have a logical and continuous history, which can be recited as a move from reformist beginnings in the 1890s through the Chicago School in the 1920s to the postwar epoch of empiricism and division into subdisciplines, and then to the new general theories (Marxism and conflict theory, for example) and new methodologies of the 1960s, and since then a relatively steady theoretical *and* methodological improvement, with a strong rhetoric of cumulation in both quantitative and qualitative realms, despite the retention of Marx, Weber, and Durkheim as an ancestral canon. Overall, it seems quite possible to tell a logical and continuous history of the American discipline, decade by decade, from its beginnings in the 1890s to the present, with some ebb and flow around the edges, to be sure, but revealing an evolving if sometimes contentious mainstream that is dominant at the center of the discipline. Thus, in quantitative work we had the survey work of the 1950s and 1960s, the more causal analysis of the 1970s and 1980s, the move to two-level models and network methods in the 1990s and 2000s, and the current fascination with counterfactuals. In substantive subfields, there has been a steady succession of small-scale paradigms: labeling theory, status attainment theory, the new sociology of science, Weberian historical sociology, and so on.

This last of my four facts seems in contradiction to the first three. The first three show that individuals do not today seem to experience careers of a continuous and directed growth (in contrast to the common sense view). The last shows

3 On the “third wave” of historical sociology see Adams, Clemens and Orloff (2005). The similar concept of “third wave feminism” is usually attributed to Rebecca Walker. Senior scholars may also have the depressing realization that much of their own work as younger people was of this kind, even though it seemed to them at the time to be novel and revolutionary. Indeed, the middle-aged hotheads with their new journal are simply repeating — with newer and more effective technology — the same pattern the seniors themselves had followed in their own middle years.

that we can nonetheless narrate the discipline itself as experiencing such a cumulating, improving career.

Now it might seem that this paradox can best be understood through elementary demographic concepts. The first three facts seem to concern age effects while the last concerns a period effect. On the one hand, we can see that young scholars are eager to scale the heights of publication, that middle-aged scholars are determined to state their views without interference from others, and that senior scholars find their work replaced by “new” work that isn’t really new. This is a pattern that might derive from a change in the professional life course, a divergence from the common sense pattern with which I started. There seems to be a new pattern of “aging” in the professional life course, in what demographers would call a change in “age-specific behavioral expectations.”

On the other hand, we have not age but period effects. Disciplinary events like the emergence of statistical software were transformative for all ages of scholars at once, in that case during the 1970s, when software packages like SPSS emerged from the middle-aged group of that time, outdating the statistical expertise of the senior generation and providing the youngest generation with simple and automatic approaches to things that had been difficult and complex. In demographic terms, this is clearly a period event.⁴

But further reflection qualifies this picture. To think of the new statistical software as a period effect considers only the careers of individuals while ignoring that of the discipline. For while to be sure statistical software had decisive effects on the lives of all scholars (at different stages in their careers, depending on their age), it also had a decisive effect on the discipline, in part because it outmoded previous quantitative research not by building on it but rather by defining it as irrelevant, by asserting that it was based on weak data or untenable statistical assumptions. The new software thus prevented one kind of simple-minded cumulation — the piling up of new analyses — but clearly was thought to be a general improvement nonetheless. In that sense it made a Kuhnian paradigm shift. But it also had another much more important — and completely unintended — effect on the discipline. By making calculation easier by orders of magnitude, statistical software more or less automatically replaced rigorous hypothesis testing with searches for the best results. Hypothesis testing remained, but only as a rhetorical form. No one in fact practiced it. Scholars searched for the best results, doing hundreds of regression models in the process, and then published whichever of those models worked best or, worse yet, best confirmed the findings desired by the scholars themselves. In a pressured environment, scientific ideals could not stand up against what Edward Leamer (1983) called “specification search.” Of course, this radically reduced the validity of quantitative research, taken as a whole. Hypothesis testing is meaningless when one searches for the best or most desired result.

4 Because of the increasing dominance of economic terminology, “period events” are now often called “exogenous shocks,” although the two concepts have important differences.

But there is an important distinction to be made here. Insofar as they affect the careers of individuals, such “disciplinary” effects were *period* effects in the demographic sense; they fell on individuals at all ages at the same historical moment. But with respect to the discipline itself, they were simply age effects, the story of the discipline’s own life course, as one among many disciplines. Disciplines suffer sudden transformations of their knowledge contents at regular stages in their life courses: at the beginning, at first maturity, and, if we trust Kuhn, at relatively regular periods afterwards when enough “anomalies” have been discovered that the total of them overwhelms the reigning paradigm. Thus what were period effects for individual scholars were simply age effects for the discipline.

But the reverse is true also. A discipline, living a typical disciplinary life course, can itself suffer *period* effects: arbitrary interruptions from outside its own aging process. But these are often provided by individuals and groups of individuals, just as the aging of the discipline falls reciprocally on those individuals as period effects. Thus, the 1960s flooded American sociology with a whole generation of radical students aiming to realize their political views through scholarship. Whatever earlier sociologists may have thought of this phenomenon, the flood of in-migrants transformed the discipline “from the outside,” as it were. It was an enormous period effect in the life of the discipline, which was aging in a normal way, as disciplines do, enduring the usual fractal splits that are common in disciplines (Abbott, 2001). In fact, American sociology was in the mid 1960s arguing about a new split between high theory and dust-bowl empiricism, which had replaced the old split between East Coast “science” and Midwest (= Chicago) “reformism.” But while it was having this almost traditional debate in a sober fashion, American sociology was suddenly forced to ingest a large and somewhat alien group of young people, who immediately pushed the discipline towards Marxism, historical sociology, political sociology, and the like. Many were women, who pushed the discipline towards studies of women, a topic previously ignored almost completely. As those young people aged and came to positions of power they did their best to transform the discipline in their own image. (Thus, for example, in the ASA election of 2013, eighteen posts were contested, and in none of them was a man elected, even though the organization was 47% male.) Note too that other disciplines than sociology might have been of a different “age” than sociology when the new generation of the 1970s arrived. Thus, a young discipline like linguistics would have experienced this period event quite differently than an old and stable one like economics or a middle-aged one like sociology.

Thus the distinction between age and period effects is completely relative. The discipline’s history acts as period effects on the lives of aging scholars, while the scholars’ personal histories act as period effects on the life of the aging discipline. There is no real difference in kind between the life narratives of the individual scholars and the life narrative of their discipline. We must therefore theorize the life course of the discipline at the same time as we theorize the life courses of its members; the two co-constitute one another. Moreover, because, as we have just seen, both the scholars and their discipline endure in time, we have to recognize that arbitrary qualities associated with the one shape the other *at some later*

time period, and vice versa. Taken together, these two facts-co-constitution and lagged effects — mean that no theoretical analysis of the relation between the scholars and the discipline can begin with only the one or the other. We cannot derive the properties of the discipline purely from the interactions of the scholars, as the economists would have us do, nor the properties of the scholars purely from those of the discipline, as the Durkheimian sociologists would have us do. We must theorize them both at once.

This is the first result of a serious reflection on the evolution of academic life in the United States in the last fifty years. Any serious social ontology must theorize groups and individuals at one and the same time, rather than treating one as the outcome of the other. Before we can move on to theorize the publication process, then, we need to reflect about a model of social ontology that can sustain such a duality.

An outline of social ontology

I have just stated the first crucial fact about such an ontology — the necessity of jointly theorizing the individual and the group. We must recognize from the outset that any action that I undertake as a professional — writing this article, for example — is simultaneously an action of both myself and of the discipline. Action is always multiple. Because of this multiplicity, neither the sociologist nor the discipline can be seen as the primary unit of society, but rather both of us are produced by the putting together, over time, of extended chains of such multiply meaningful actions. Therefore, the social world, in the first instance, is a world of events, which have multiple connections to past and future. The micro constituents of the social world are not individuals nor are they social groups. They are events. All that exists at any given point in time are the social events taking place at that point in time, into which are brought vast numbers of determinations and expectations from the immediate past, which are then knitted together in the present by action. In this knitting together, all these old determinations and expectations become new arrangements in the present, which in turn become the determinations and expectations that will shape the immediate future, which becomes, in that very process of knitting, the new present of the social process. Because the social world is such a world of perpetual change, in which stability must be explained, we must speak of the ontology of the *social process* rather than the ontology of the *social world*.

To summarize my argument so far, any social ontology must theorize both groups and individuals at once. The proper units of social ontology are thus events which are multiply connected to preceding and following events by ongoing social processes. It is these links and lineages of events that create the individuals and social entities that appear to be the microstructures of the social process in our usual ontologies. In short, social life is processual and multiple.

Now of course, some of the “past” endures independent of the moment-to-moment social reproduction I have just mentioned. Some of it is written down as words, and other parts of it are written in aspects of the physical environment like

farms, buildings, and cities. Still other parts are engraved in the memories of the billions of human beings whose common activity is the social process. We might speak of these embodied aspects of the past as “encoded,” because they are more or less automatically reproduced over time. This constant reproduction happens because that they are “remembered” by non-social things like physical realities or biological organisms. But all the same, the future meaning of these encoded aspects of the past is always determined by social activity in the present. Everything in the social process, from the “largest” social structure to the individual, is at risk in every moment. “Large” social structures are simply those most likely, for processual reasons, to be reproduced from instant to instant, possibly because of the encoding I have just noted. Encoding is thus a third basic premise of social ontology, in addition to the first two — processualism and multiplicity.

Within the social process, stability appears as what I shall call lineage, taking a term from studies of kinship. A social entity is a lineage of events. The metaphor of lineage captures several important ideas. First of all, it captures the idea of the continuous processual reproduction of the social world. Second, it captures the fact that each social event has many ancestors and many descendants. Third, through that complex web of kinship relations, it captures the notion that social entities have complex and permeable borders. It is often difficult to say where one social entity ends and another begins, and moreover it is — as I noted earlier — meaningless to attempt to place any given event in one and only one lineage, just as we as family members are both the descendants and the progenitors of many different lineages, not just of our patrilineage, as we so often think. Like our selves, actions and events are multiple. Fourth, the metaphor of lineage captures the ongoing contingency of social persistence, the role of chance and recombination in the creation and destruction of ongoing social things.

Thus, what we usually call a social entity or group — a discipline, for example — is actually a lineage of events, bound into an ongoing thing by certain processes that maintain the lineage’s distinctiveness from the rest of the social process. I do not have the space here to discuss these bounding processes in detail, although I have already mentioned some of them under the idea of encoding. But I would like to mention what we might call “implicit encoding,” the fact that the mere pattern of events at any given moment contains cycles of connection and resonance between events — mutual effects over time — that make that reproduction very likely — a kind of encoding implicit within the instantaneous social process itself.

I have so far established four key concepts to use in our analysis of careers and publication: processualism, multiplicity, encoding, and lineage. These are general qualities of any reasonable social ontology. Let me quickly sketch what these four qualities mean about the personality or individual, since that is the case in which their implications are most at variance with our everyday thinking. As the idea of encoding suggests, the concept of memory is crucial to the social process, and so one extremely likely form of lineage is that associated with an individual human organism, since individual human beings are repositories of immense collections of memories, concentrated on a single item — that particular biological organism. To be sure, not all the memories involving Andrew Abbott, for example, are in my

head. Many of them are in the heads of my former teachers, lost friends, old students, family, and thousands of others. But a very large proportion of them are in my head, and so although I don't command all the memories and expectations that make up my personality, I am at the least a majority stockholder in that personality. So personalities, although social in nature as George Herbert Mead taught us, are tied relatively closely to particular human organisms.

But a social entity like a discipline does not have such a unified memory. Its memory is scattered in thousands of people. And indeed large regions of it involve conflicting memories as well as conflicting ideas. But since the discipline persists, we can infer that these conflicts don't cause disruption to the lineage, either because they are not often invoked simultaneously or because the cycles of interrelation between discipline members that are implicit in the moment-to-moment existence of the discipline do not permit such conflicts to propagate within the lineage that is the discipline, perhaps dissipating them to the outside or perhaps locking them up in more or less independent internal groups. Examples of this defusing of disruptions are common. We don't tear sociology apart — we just complain about it to each other. Or again, a discipline may appear to have a momentary consensus, but even though that consensus may be mere appearance, we will typically ignore much of this dissensus in our written histories of the discipline; it the narrative to weak.

This large quantity of internal contradiction, which is concealed by the gossamer, networked quality of any social entity, has a centrally important implication. In our usual thinking, disciplines appear to be "larger" social structures than are individuals. We take disciplines seriously; we believe they are strong and constraining. Yet in fact they are much more changeable than are individuals. A discipline can change its "mainstream" approach to social reality quite a bit faster than can most individual scholars. That is because it may already include the new mainstream concealed within it and because it can remodel itself quickly by internal rearrangement or by binding new people into itself and loosening its relation to others. A discipline may be "large," but it is surprisingly contingent. This fact is evident in the massive literature on "weak ties," which are important precisely because they have large structural consequences, far out of proportion to their own immediate "strength" (Granovetter, 1973).

I want to take five basic themes from this brief ontological exposition. First, that the social world is one of action and events, in short that we should speak not of a social world but of a social process. Second, that action is always multiple, or, to put it another way, that lineages always overlap — the fact that when I write this article, *I* do something but so also does the discipline of sociology, and men, and Americans and so on. Third, there is encoding; certain kinds of continuity are in some sense etched into the social process, either because they are etched into the physical and biological embodiments of that process (the built environment and the bodies of biological individuals) or because they are implicit in cyclical resonance structures among social arrangements themselves. Fourth, constancy and stability in the social process are not *given* phenomena but are rather *created*, taking the form of constantly rewoven lineages of events. Finally, from all these premises

flows the crucial axiom that individuals and groups are not hierarchical levels, but simply different kinds of connections between social events. Individuals and groups are coplanar, as one says in mathematics.

My central ontological ideas are thus processualism, multiplicity, lineage, encoding, and coplanarity. Each has a role to play in my analysis of scholarly publishing. As you can guess, process will be important because the development of modern scholarly publishing is inevitably a process, not a fixed system. As for multiplicity, it will be important because the individuals and their discipline consist simply of events — the giving of talks, editing of journals, doing of research, and teaching of classes — and each of these events is part of both the making of an individual and of the history of the discipline. As for encoding, it will be important because sociological skills and techniques are etched into the lives of individual scholars very strongly, while they are not so strongly etched into what we might call the mainstream of the discipline; the discipline forgets faster than do its members. As for lineage, it will be important because the making of both the intellectual personalities of sociologists and of the ongoing history of their discipline take place as the production of coplanar lineages of events. And coplanarity is important because, as I have noted throughout this little list, the making of lineages is at once the making of individual lives and the making of a discipline.

Let me again emphasize that the discipline is not bigger, in any sense, than are the individual scholars. Scholars are not included within disciplines, as the common metaphor of levels would have it. Rather, disciplines on the one hand and scholars on the other are simply different ways of looking at the social process of knowledge as a whole. They are different lineages that are defined on the same collection of events — the teaching of classes, the doing of research, the writing of articles such as this one.

The process of knowledge

Our detour through the problems of social ontology helps us address the curious paradox of my opening section: that individuals in the discipline seem to be having less and less coherent careers at a time when the history of the discipline itself seems to be moving in its usual pattern of continuous and directed growth. As my ontological argument shows, although the making of the careers and the making of the discipline proceed simultaneously, the relation between them is something that is itself continually remade through time. However, in order to put the two into this continual reciprocally determining relation, we must first develop a general theory of disciplinary evolution to complement the theories of careers that we have already developed.

The best place to begin is with the concept of cumulation. In the natural sciences, knowledge works in some sense through a process of cumulation. Cumulation does not mean the simple piling up of little bricks of truth into larger walls and pyramids of knowledge. Nor does it mean that science does not sometimes experience paradigmatic change. Rather it means that natural science tends

to grow by creating theories that subsume more and more facts — and indeed that subsume more and more smaller theories. That is, subsumption is an essential process in the evolution of natural science: the explanation of things by taking them as instances or special cases of some larger, more general regularity. Because it is based on subsumption, natural science can be taught using sequences of textbooks that take years to master and that teach hierarchically organized structures of uniformly accepted truths.

None of these things is true about what we might call the human sciences. As I showed in the book *Chaos of Disciplines* (Abbott, 2001), much social scientific knowledge is just rediscovery, as we see from the many articles whose titles take the form of “bringing something back in.” In sociology itself, investigation quickly reveals that there is little case for real cumulation. Unlike the scientists, we do not teach from texts whose fundamental principles and simple techniques embody a cumulated, systematized, and consensual state of the art. Nor are we denizens of a “cutting edge.” In an article, our average reference is to work more than ten years old, not to the latest results. Our classic theoretical works are a century in the past; no new, subsuming theories have replaced them. Even our quantitative subfields do not build on past work, but forget it altogether. They routinely reject all work more than twenty years old as methodologically suspect, but then go ahead and have the same old debates with newfangled mathematics. In short, taking sociology as an example, we find that our discipline seems at one and the same time to be oriented to classics and to novelty, to enduring ideas and to cutting edge techniques. All the same, it is obvious that within subfields and over limited periods, there is much cumulation in sociology. The discipline thus seems both cumulative and non-cumulative. (See Abbott, 2006).

There is a relatively simple way to theorize the apparent contradiction that knowledge in the human sciences has cumulating, progressing subparts yet does not itself cumulate overall. Such a knowledge system has three levels, one of which simply piles up — what I call Rankean facts, invoking the name of the famous German historian who preached the importance of such facts. Such things are obviously true or not, and many of them are piled up continuously in routinely gathered data like censuses, financial records, organizational data, and so on. Rankean facts obviously cumulate.

Above this level of Rankean facts, there seem to be cumulating things that I have elsewhere called “generational paradigms” (Abbott, 2001: 23-25): things like labeling theory, new social movement theory, population ecology, and queer theory. These schools of thought do cumulate for a while. But they all eventually weaken, for a variety of reasons. (The most evident reasons are, first, attempts to move the paradigm beyond its founding, preferential zone of application and, second, the handling of anomalies through adding variables and scope conditions, a process that ultimately points the paradigm towards mere eclecticism.)

Above the level of such cumulating local paradigms is a third level, of very general ideas like methodological individualism, historical materialism, pragmatism, and other general frameworks for thinking about social life. These are the

potential sources for the conventional assumptions typically made by the (lower level) generational paradigms. These general frameworks are loosely orthogonal. One can potentially combine any two of them but almost never a third. Moreover, they do not change to any serious degree over time. And none of them is ever going to be decisively proven or disproven: they can be restated and they can be made more simple or complex, but they cannot change in their fundamentals. I shall call this third level the “categorical” level.

In short, each of the human sciences functions at three levels, at least at present. The first of these — the level of Rankean facts — cumulates trivially, like bricks in a pile. The second — the level of generational paradigms — cumulates much more substantively, but inevitably reaches internally-imposed limits. The third — the level of social categories — does not cumulate at all.

Demography

We have then a set of ontological terms and a model of knowledge. The core terms from my ontological argument are processualism, multiplicity, encoding, lineage, and coplanarity. The core of my model of sociological knowledge is its three level structure. How can these two analyses be combined to understand the relation between publication and careers today?

The theoretical combination seems straightforward. The “events” of the process are things like the readings of books, the writings of articles, and the givings of conference papers. Any given reading of a book or writing of an article or giving of a conference paper is at least two things at once: first, it is an event in the life of a personality; second, it is an event in the life of the discipline. As we have already seen, one way of “connecting the dots” of these events gives rise to what we can call the lineage of the *discipline*, embodied in the narrative of a “disciplinary mainstream” or an overall disciplinary process like specialization. Another way of connecting the dots gives rise to the lineages that are the personal *careers* we know and experience ourselves. Because of these dual lineages, we can give a paper that is very important in our personal trajectory but unimportant in the disciplinary narrative. But we can also give a paper of little importance to us personally, but that becomes important in the discipline for some reason unrelated to us. And, finally, we can also have the experience of writing something that we think means one thing while the discipline takes it to mean another. That is, a given article may have different meanings for us and for the profession.

More particularly, as I have already noted, the discipline imposes period effects on the trajectories of our careers. The three-level model makes visible this intersection between individual careers and disciplinary history. If we begin our career within a generational paradigm that is almost at the end of its life cycle, our career is going to be seriously damaged. Those who took up labeling theory during the 1970s, for example, took their degrees just in time for that particular generational paradigm to die. They essentially had to start over. It is much better, of course, to join a generational paradigm when it is just beginning. It can then carry

us through nearly an entire career. The lucky person who makes this choice will feel that career and discipline have moved in tandem throughout his or her career, as we all feel they should, but as so seldom happens in fact.

Thus we see the importance of encoding. For this potential for misalignment is caused by the encoding of a set of paradigmatic ideas and methods into a given personality in its years of training. That encoded expertise can leave this personality suddenly out of fashion if his or her generational paradigm passes away, but with strong incentives to continue the old ways because they are familiar and were achieved at considerable cost.

If we generalize this analysis across the whole discipline we can see how encoding viewed more generally accounts for much of the publication problem as we know it today. But to do so we must turn from the discussion of the differing lineages of individual scholars (and of disciplines and generational paradigms) to an analysis of professional demography. For demography is our general word for the process by which individuals carry their encoded memories, experiences, and skills into the future. Demography gives us a vivid sense of the coplanar relation of individuals and discipline, of the fact that disciplines and careers are made out of the same events, at the same time. For there have been two very different demographies of American academia in the last 120 years, and they have resulted in two very different kinds of academic systems and career structures. Publication in particular is the crucial site that allows us to see the effects of this changing demography, whose pressure has completely transformed the nature, meaning, and timing of publication for individual scholars.

The first demography of American sociology was one of rapid expansion. This period lasted from the beginning in the 1890s until about 1970, with a brief downturn in the 1930s and early 1940s. Until the Second World War, the expansion of the universities themselves was fairly slow, but disciplinary ranks expanded steadily through the replacement of the non-academics then doing university teaching (Abbott, 2011a: 44-49). After the Second World War, disciplinary ranks again expanded, but because of expansion of higher education itself. The GI Bill, the increasing demand for college education of women, and, ultimately, the creation of the community college system created vast new opportunities for hiring.

This expansion was very rapid. American academia in the humanities and social sciences numbered about 150 PhD level professors in 1890, about 1000 PhD level professors around the First World War, and about 10,000 by the time the 1930s and the War brought a temporary end to expansion. That is, expansion was more or less exponential. Precisely the same thing happened after the Second World War, but at a faster rate: American sociology doubled in size every five years from 1945 to 1965.

As demographers know, a population with a high birth rate is a young population. Suppose we create a model academic population with a forty year career for everyone and begin with a flat age distribution across the discipline. And suppose we assign that population the growth parameters actually observed in American academia from 1945 to 1965, doubling every five years — that is, expanding 15% per year. The population will reach a stable median age of only *six years post PhD* within five

years. This is a world of assistant professors — a very young world. If the discipline grows only ten percent per year, it will reach a stable median age of eight years post PhD in seven years. That's still very young. Even at five percent per year, it will reach a stable median of 14 years post PhD in eleven years. That's a little more middle-aged, but five percent is far below the expansion rates actually observed.

Such a rapidly growing population inevitably produces the structure of graduate education and of professional careers to which I referred at the outset — the “common sense model.” It is a model that was evident to any observer alive at the time and was then encoded into memory. In this model, most of the members of the professoriate are very young. Most training is done by the small core of quite senior professors, who are located in very few universities. (In most fields in the US even as late as the early 1950s, less than fourteen departments produced over half of the PhDs.) Each of the few major senior figures or departments has a large group of students, who rapidly spread throughout the expanding academic universe. (Talcott Parsons, for example, had about fifty PhD students.) In this model, too, all PhDs who want academic jobs get academic jobs (because of the rapid expansion), and as a result there is no particular pressure to publish. Indeed, until the 1970s publication was not very common in the human sciences, or in the natural ones, for that matter. As Alfred Lotka pointed out almost a century ago, lifetime publications have historically taken an inverse-square shape. That is, even for those who published anything, the modal value of lifetime publications was one or two; so also was the median. Even the mean in such a system is only between four and five.⁵

Moreover, during the years of expansion, this published work was actually read. For reading too was facilitated by rapid expansion. In part, it was simply a matter of having time to read, since no one was pressured always to be writing things. But it was also a function of the sheer youth of the professoriate. Young professors have more time on their hands to read, because they do less graduate training and administration than their elders. Moreover, older professors have made up their minds about many important sociological issues. They are already upholstered with intellectual furniture, and reading is for them not only less immediately necessary, but also is more work, because it requires “interior re-design” to adjust the new readings to their existing intellectual décor. For all these reasons, both the reality and the idealized picture of the academic profession were full of readers, simply because the profession was on average very young. And indeed there is serious evidence that academic reading in this period was much more careful than today's. For example, the amount of time faculty spent in the library was much higher in the 1950s than it was in the early 1990s (that is, even before the internet). Perhaps more important, we know also that about two thirds of references in the typical social science article in the 1950s referred to a single page or page range — not “Bourdieu, 1988,” but “Bourdieu, 1988: 127-129.” That is, they indicated close engagement with what was read; there were no lists of four and five

5 See Lotka (1926), and also Simon (1957: 160) and Price (1963: 40-51).

citations to “support” some kind of truism, something we see throughout almost every article today.⁶

Finally, I should note that this demography articulated quite well with the three level model noted earlier. Generational paradigms were not “close” to one another in intellectual space; there was lots of open land. The paradigms had coherence because the rapid expansion provided ambitious young leaders with many followers to flesh out details and undertake the case studies with which the middle-aged leaders could consolidate their ideas. And the restatement of the non-cumulating pieties of the categorical level could safely be left to the relatively few senior faculty: Parsons for Durkheimian functionalism, Homans for exchange theory, Coser and Gouldner for conflict theory, and so on.

In summary, in the years of expansion, there emerged from everyday disciplinary experience the ideas (1) that most professors were young, (2) that there were relatively few senior trainers of graduate students but (3) that each had many students, (4) that every student would get a job, (5) that there was no particular reason to publish often, and (6) that anything published would be read by many of one’s colleagues. This is the common sense model of academia with which I began. And obviously, it was etched into the memory of everyone in sociology (actually of everyone in academia) who left graduate school before about 1970. The last of those people, of course, would not retire until 2010, long after these “everyday” ideas were longer realistic. This is what is meant by encoding. The common sense model of academia had had a very long life because the simple facts of physical survival and of the dominance of faculty politics by the oldest non-retired generation meant that even though the “senior faculty” was steadily turning over, it was not until about 2000 that a generation that had grown up with the new demography actually reached the age of power in its departments and universities.

But by 2000, the old demography was indeed long gone. For externally induced reasons, the American academic system entered a zero growth period after 1970, at least in the human sciences. Universities stopped growing their faculties. Moreover, because of pressure from business interests, the final zone of the earlier expansion — the community colleges — was transformed into a vocational sector, further reducing the employment opportunities for young graduates in the human sciences (Brint and Karabel, 1989). Yet the graduate education system, spurred by the military service deferment in time of war, was reaching its all-time pinnacle of production in the early 1960s, and took a long time to downsize, in part because of the new pressure to produce female PhDs to balance the genders of the professoriate. Eventually, graduate numbers were reduced by the roaring economy of the mid 1980s; strong economies have always undercut graduate student numbers and by this time it was agreed that in any case academic job prospects were dismal. (See Bowen and Rudenstine, 1992: 41-55).

This change to zero growth had drastic consequences on the shape of the profession. The preceding rapid expansion had loaded the profession with graduate

6 For sources and further background, see Abbott (2011a and 2016).

students and young professors, but suddenly there were no openings for them. To be sure, a fraction of them got jobs replacing the aging faculty who were leaving. But these were very few relative to the size of the discipline, because of the age distribution produced by the preceding rapid expansion. Therefore, many young PhDs left academia for libraries, not-for-profits, and government agencies. Within disciplines like sociology, however, there remained the large mass of professors produced by the expansion years, which had to work its way up the professional ladder before retirement.

To give some numbers, let me return to my earlier model. A discipline with a forty-year-career lifetime, expanding at about ten percent per annum, will, as I said, have a median age of eight years post PhD by year seven in the expansion. Then the newly large cohorts start to pass the median, slowing the decline in average age. The median then begins to creep *upward* in about the 18th year of the expansion, as the larger cohorts enter the mid-career stage. Suppose then that in the 20th year of such growth we shut off the externally induced growth and simply allow exact replacement of those faculty retiring from the top. (This is approximately what happened in 1975.) The big mass must now work its way through the long snake. The median age of the discipline will rise by nearly one year *every year*, reaching 18 years post PhD in the ten years after zero-growth started (that is, in the early 1980s in the US), reaching 27 years post PhD after twenty years of no growth (in the early 1990s) and reaching a full 33 years post PhD — that is, the median age will be within seven years of retirement — after thirty years of zero growth, fifty years after the start of the initial expansion. This was the case in the US in the early 2000s. It is exactly the demography of my department, whose median age as of 2014 was 57.

This astounding demographic change is a simple and wonderful example of encoding, as embodied in the idea of tenure. The reason there are “X many” 48-year old professors in 1970 is that there were “X-plus-a-few many” 47-year old professors in 1969. Human bodies just keep on going. This is so obvious that most of us forget that when humans are viewed as a group or population, their survival — in this case the survival of professors — can have very sharp consequences, in this case the relentless aging of the discipline. But that the bodies survived is not the only encoding. More important, as I noted earlier, these people retained an image of what disciplinary life should look like — how many of which kinds of people there would be, how much scholars should publish, how many graduate students there should be — and they continued that image as an actual practice long past its viability. Even by the 1990s, only a few programs had seriously downsized their incoming cohorts of graduate students, although by the 2000s nearly all private universities in the US had done so. (The state universities could not do so because they had come to rely on graduate student teaching, which was cheaper than professorial teaching. So they continued to overproduce.)

Now there were further consequences of this encoded demography. Eventually, graduate programs shrank in size. But until they reached a level that was sensible in the new demography, many PhDs — probably about one third of all PhDs in the humanities and the humanistic social sciences — never entered academia. Of those who stayed, many moved into applied programs in fields like

criminology and social work. And because graduate programs shrank in size, there were many fewer PhD dissertations to supervise, even as the large mass of faculty recruited in the expansion years moved into the mid- and late-career years in which they might have expected to do significant supervision. So the work experience of mid-level and senior faculty changed sharply, compared to what they expected. There were now many fewer graduate students to teach, so the role of senior faculty changed completely. One suspects that “interdisciplinarity” emerged in part as an alternative career path to absorb these “surplus” senior scholars.

But the consequences for the publication system were the most drastic. The post 1975 demography meant that professional standards could rapidly be raised, particularly in terms of publications. Peripheral universities could demand the very best in this new buyer’s market for academic talent. Of course this change did not happen overnight. Publication was not necessary to get a job even in the late 1970s, but even by 1990 scrutiny of publications had become important at hiring time. Moreover, there were changes in authorship itself. The research assistants who used to be thanked in the acknowledgements of quantitative articles were now promoted to the author line, because it advanced their careers.

General data confirm this view. As reported in surveys, the rate of publication by university scientists doubled between 1977 and 1995 alone (Tenopir and King, 2000: 144). (Interestingly, the ratio of the number of all papers to the number of all scientists did not double, which indicates that some part of this “doubling” was actually simply increasing co-authorship, not an increase in total publication.) Data on the *American Journal of Sociology* show that in the first seventy years of the journal’s history, the average age of contributors fell only about four years, most of it within the first two decades, as the expansion pattern set in. This roughly paralleled a similar but slightly later fall in the age at which contributors took their degrees. The typical author thus wrote his or her article eight years post PhD for most of this period. But today, as I have noted earlier, 10% of papers published in AJS are by graduate students alone and 25% involve at least one graduate student. This is not because today’s graduate students are somehow smarter, more precocious, or better-trained than were earlier generations of students. There is no evidence whatever of that. It is because without publications on graduation, they will not get jobs.⁷

The same rise in publication requirements holds true for later personnel actions like tenure, promotion to full professor, and inter-university mobility. It thus seems almost certain that the large expansion of publication among scholars after 1970 reflected not a sudden increase in academic excellence or ambition on the part of individual scholars, but rather the shift to a buyer’s market, in which publication was the only convertible currency. That is, after 1970, publication became as much or more about personnel actions than it was about scholarly communication. A similar thing had happened earlier in the field of education, where the doctoral degree became in the interwar period a condition of certain kinds of advancement within

7 For the pre-1965 figures on age of authors in the *American Journal of Sociology* at PhD and at publication time, see Abbott (1999: 90, 129). Current data comes from the journal’s current databases.

public school systems. The result was an epidemic of education doctorates — which were 7% of American doctorates in 1920, 15% by 1945, and 25% by 1981 (*Historical Statistics of the United States*, Millennium edition, series Bc600, 606/4).

In summary, after 1975 publication played a much different role in the life of the discipline than it had before 1975. Before 1975, publication was important to the small elites at the top of the discipline — elite professors in 1900 wrote six to ten books and dozens of articles in a career, just as such people do today. But for much of the discipline in the earlier period, even for those at the PhD level, within the fully academic discipline, their publication career took the form of a couple of articles early in professional life and not very much thereafter. Sociologists read a great deal of work — as I noted, in all probability they read much more and much more carefully than they read today. But they did not publish a lot. Moreover, the discipline was young as a whole, and more likely to read for that reason alone.

After 1970, the publication system changed from being a scholarly communication system to being a system whose chief function was simply to provide evidence for personnel decisions. This has resulted in the publication of what are, in effect, papers that forty years ago would simply have been graduate student course papers — mediocre work replicating widely-known results. We function today in a world where there is extensive and well-documented pressure to publish for purely instrumental reasons, in addition to whatever “natural” desire there is to speak to one’s colleagues. (See, e.g., *National Enquiry into Scholarly Communication*, 1979: 43.)

But the hiring consequences are not the only consequences of the new demography. There was also a crisis in reading. The end of expansion meant a rapid proportional *decrease* in the younger ranks of the profession, which, as I noted, contains the people with the most time to read and the most inclination (and indeed need) to read. To be sure, for older faculty, the decrease in need for graduate supervision probably meant there was more time for reading. But there were countervailing forces. Administrative work became more onerous, because the removal of gender ceilings in the professions vastly reduced the availability of good secretaries, and the simultaneous advent of the personal computer meant that professors took back large amounts of their clerical work in any case: typing of scholarly work and recommendations, preparation of teaching materials, filing travel reimbursements, and so on. And even at the old rates of publication, the simple expansion of the discipline by a factor of ten between 1945 and 1970 would have meant that there was ten times as much material to read. And once the job-market-induced publication spree began, another multiplier entered the equation. Moreover, as my analysis of the publication boom suggested, much of this job-market-induced publication was intellectually unnecessary and of dubious quality. Thus even as there was more and more to read, the good work became harder to find. Note, too, that the personal computer, which arrived in 1983, facilitated writing, while there was no equivalent new commodity aid for reading, which remained as hard as ever. And the 1970s were the decade in which statistical software first emerged on a widespread basis, making quantitative sociology far easier than it had even been before. Even technology therefore favored

publishing over reading, on the quantitative as well as the qualitative side. For all these reasons, reading has seen a precipitous decline.⁸

In summary, the demography of American sociology (and indeed of all the human sciences in the US) changed quite radically around 1975. The system that produced our common sense image of careers and disciplinary history existed only before that date. It was a system in rapid expansion. It was filled with young people, who were readers much more than writers. Expansion meant that each older person had many younger clients, but the small size of the system meant that most graduate training was highly centralized. After the 1970s, the system did not grow, standards were raised radically for all personnel actions, and there was a consequent epidemic of publishing. Publication became as much about personnel action as about scholarly communication. At the same time the discipline aged very rapidly, even while the graduate cohorts shrank. The aging professors thus lost their old outlet of graduate teaching, even while the rapidly expanding pool of authors rapidly lost their readers as the professoriate aged into the life-course years where less reading is done. Even worse, the rapid publication meant that good work was harder and harder to find amid the mediocrity. The new demography guaranteed a scholarly crisis, which indeed has not failed to arrive. American academia in the humanities and the social sciences is in desperate intellectual straits.

The institutionalization of rediscovery

We see then that a processual account does well in making sense of publication as a changing zone of experience for American sociologists in the twentieth century. Each of the major concepts in my ontological outline has played a role in this account. Thinking about process is important because it helps us see that actors are continually making decisions about publication in a social moment that they are themselves both making on the one hand and experiencing as constraint on the other. Thinking about multiplicity is important because it shows us *how* the alignment of the individual and disciplinary meanings of publication is mutual in the first period, and orthogonal in the second (during which the individual imperative to publish for personnel reasons, taken discipline wide, has essentially destroyed the communication functions of the system). Thinking about lineage is important because it frees us from fixed concepts of individuals and disciplines, enabling us to see the flexible system of generational paradigms and the ways it articulated but also constrained the relation between individuals and the discipline. Thinking about encoding is important because it points us to the centrality of remembered theories and methodologies and mindsets, but above all to the centrality of encoded images of the disciplinary and individual life cycles, which endured long after they had become irrelevant to the contemporary process. Finally, coplanarity is important because it reminds us

8 I have estimated that on conservative assumptions, there are about one tenth as many reading-hours devoted to a given publication as under the pre 1970 demography. See Abbott (2016).

that individuals are — surprisingly — more enduring than social entities like disciplines, even though the latter appear “larger” for some purposes. And the endurance of individuals and their mindsets about the proper academic career are the central themes — and indeed the central causal factors — of this story.

The analysis here presented leaves us with an obvious task. We must invent an intermediate structure to succeed the generational paradigm, a structure that can provide a sensible articulation of the individual career with disciplinary development. The generational paradigm worked in the expansion period because it took the demographic shape that was viable in an expansionary demography. It had many young people and few old ones. Its danger was that it marooned some of the young people, for those who joined it late in its life were trapped with theories and methods that would be set aside as the paradigm succumbed to old age and was replaced by newer, more fashionable successors. In the old demography, such marooned young people could either continue producing for a smaller and less central audience, or they could join the new paradigms or they could simply drop out of the scholarly game.

In the new demography, dropping out is not possible. External management and a buyer’s market guarantee that one must always publish. Moreover, two other things seem true as well. First, the process by which generational paradigms age will not change. They are driven by clever new ideas, which are invariably first applied to the data most welcoming for them. Initial success always leads them to attempt to broaden their application, and eventually this broad application either relapses into eclecticism (losing the focus and clarity that made the paradigm exciting) or confronts data for which it is much less suitable. Either way, the paradigm collapses. These kinds of cycles will not stop.

Second, the new demography inevitably imposes a flat age structure, which means that it is impossible to continue any age-pyramidal disciplinary substructure — consisting demographically of a few older people, a goodly number of middle-aged people, and a swarm of young people. Most disciplinary substructures will have to be “tubes” of uniform width through the age structure, with equal numbers of scholars at all ages. This means that the entire division of labor between older and younger will have to change. It cannot remain the ideal that, as I stated at the outset, each scholar learns some basics in graduate school, develops those basics into a research program as a young professor, pushes the research front a little further throughout the subsequent part of his career, and then, as a senior scholar, produces some culminating work, drawing together a lifetime of thought and reflection. The scholarly world doesn’t present enough of a market for culminating works.

It is not clear what such a new intermediate structure can be. As the most likely possibility, the demography imposes something like a set of parallel, separated tubes. In some ways, this tube model seems to imply a return to relatively stable subspecialties. Perhaps the pulse and succession of generational paradigms was itself a phenomenon only appropriate to expansionary times. Moreover, it could easily be the case that the external pressures on academia — to produce relentless “innovation” and “creativity” and such like — will actually not permit the

emergence of any such intermediate structure, naturally adapting to the new conditions of the academic world. Oddly, generational paradigms produced much novelty and innovation. Most of it was merely apparent novelty, at least at the categorical level, as I argued in *Chaos of Disciplines* (Abbott, 2001). But the novelty and excitement were always palpable in the experience of scholars as individuals. One recalls the buzz about world systems theory, world polity theory, labeling theory, status attainment theory, and dozens of others. They were all ways of becoming excited once again about the same categorical matters, and in that sense combined an obsession with novelty with profound conservatism. It would be ironic indeed if the new conditions, and the external pressures so intent on novelty, produced a system less able to achieve this combination. For the moment, it is enough to have recognized the nature and dimensions of the crisis we face.

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