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Academic Labor
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The Visible College

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Authorship may have died at the hands of a French philosopher drunk on Balzac, but it returned a few months later, by accident, when an American social psychologist turned people's attention skyward.

The philosopher in question was Roland Barthes (1977), who, in the autumn of 1967, published a provocative little essay (since widely reprinted) entitled "The Death of the Author." It opens with an effusive line from Honoré de Balzac's novella *Sarrasine,* which, according to Barthes, embodied the complexity of voice immanent to all writing (p. 142). More to the point, Barthes argued, the quotation underscored the extent to which Western literary criticism had grown reductive, owing to its tendency to attribute the meaning and value of all books to the styles, personalities, or biographies of their authors (p. 145). Such a view neglected the degree to which human beings are social creatures who create not *de novo,* but instead by marshaling the expressive forms, turns-of-phrase, and other collective resources of the groups to which we belong. Hence, for Barthes, writing wasn't evidence of individual genius, nor was it an indicator of the integrity of a particular creative work. It was, instead, nothing more and nothing less than "a tissue of quotations drawn from the innumerable centres of culture" (p. 146). The end result was a new textual cosmology, one in which the false idols of authorship would be sacrificed in the name of a new *dramatis personae* consisting of humble scribes and plucky readers.

Meanwhile, the trickster social psychologist Stanley Milgram was at it again. He'd made a name for himself in 1963, when he published the results of his infamous "obedience" experiments. Born of the atrocities of the Second World War, Milgram's early research attempted to document the conditions under which one would comply with an authority figure's admonitions to do harm to a fellow human being. The subsequent research took a more farcical turn, however, as in his 1967 investigation into the number of people who would mail a letter, evidently lost, addressed to the "Friends of the Communist Party" of New Haven, Connecticut (Milgram, 1969, 2010). So too, with the study he organized for the winter of 1968. Milgram gathered a team of researchers, whom he dressed in plain clothes and deployed on a stretch of busy New York City sidewalk over the course of two afternoons. At designated time intervals the researchers would stop walking and, without uttering a word, begin staring intently at the sixth floor of a nondescript building nearby. The goal? To see how many passers-by would direct their attention similarly, based on nothing more than some strangers' nonverbal cues. The greater the number of researchers who gathered on the sidewalk, Milgram found, the greater the percentage of passers-by who would look up or join the burgeoning crowd (Milgram, Bickman, & Berkowitz, 1969, p. 81). The experiment has since become a touchstone for the idea of “social proof,” which refers to the tendency among people to think and conduct themselves in accordance with the behavior of others (Surowiecki, 2004, p. 43).

Authorship is neither living nor dead, I believe, but a function of social proof. It exists as long as a sufficient number of people look skyward, as it were, much like the pedestrians did in Milgram’s study. Now, I say this not to doubt Barthes’ understanding of the nature of writing specifically, or of creative process more broadly, but instead because of the cognitive dissonance I experience every time that I read, read about, or discuss “The Death of the Author.” However much I may agree with Barthes in principle, the nature of academic labor is such that, in practice, I constantly appeal to the very same figure whom he purportedly killed off more than 40 years ago. I’ve done this, and continue to do this, in virtually all of my published work; in my classes and seminars (even those where I assign “The Death of the Author”); in the reference letters I write for my students; in my reviews of essays and book manuscripts; in my tenure and promotion reports; and more. And I’m only able to do this because I’m surrounded by communities of people who, with rare exception, reciprocate in kind.

Authors endure in academic writing, as they do elsewhere, for a complex mix of reasons. Some are institutional, as in the practice of assessing professional competence based on individual cases (e.g., promotion and tenure). Others are legal and economic, as in intellectual property, a system grounded in the rights that private individuals maintain over their estates. Still others are more banal, as in the custom of citing work composed by three or more people using the surname of the lead contributor followed by the abbreviation “et al.”[1] The cumulative effect of these and other factors is not only to reinforce authorship as social proof, but also to constrain our ability to represent the breadth and depth of academic labor.

This is also the case where our publications are concerned, which, judging by their capacity to “show the work” (as my math teachers used to say), are nothing if not crude instruments. To put it less figuratively, the privileged forms of academic publication—peer-reviewed books and journal articles—do a reasonably good job of making the products of our labor manifest. The same cannot be said about the process, however, which is where the bulk of the work actually resides. Indeed, however much we may treat them as gold standards, books and journal articles, traditionally conceived, promote a restricted view of scholarly communication and what it’s capable of. They’re what James Scott (1992) calls “public transcripts”: a type of discourse whose propensity is to obscure the intricate web of social interactions that sustains it, interactions that exist mostly “‘offstage,’ beyond [the] direct observation” of audience members and authority figures (p. 4).

There are, of course, good reasons for wanting to keep the “hidden transcripts” of academic production well-hidden (Scott, 1992). I often tell my students that writing is not unlike a laboratory

[1] As Robert K. Merton (1968) has argued, a consequence related to this form of attribution is the “Matthew effect”: the tendency for already well-known academic authors to receive exclusive credit from their peers for collaborative work (p. 57; see also Merton, 1988).

experiment; it may succeed, eventually, but it's almost as likely to blow up in their faces at some point. No one likes to fail or be perceived that she or he has done so. Hence our publications, as public transcripts, serve an important function beyond their capacity to advance arguments or present findings. They help us to *save* face, both personally and professionally, by showcasing only the most polished—which is to say, epiphenomenal—aspects of our work. A significant downside, however, is that much of the backstory disappears, thus denying those who might one day follow in our wake important context about the development—and in many cases, the substantial revision—of key insights, concepts, positions, paradigms, or what have you.[2]

The backstory doesn't disappear entirely from public view, of course. A trace almost always remains: that slim sliver of extraneous matter called the acknowledgments, which is basically a labor history in condensed form. Expanded, it would consist of an inventory of comments and leads contributed by one's colleagues; a detailed account of the work carried out by one's research assistants, if any; editorial feedback—in the case of journals, likely collated and possibly drafted by one or more graduate students, whose labor deserves to be recognized well beyond the masthead; a chronicle of the time and goodwill expended by the referees in responding to as many as two or three different iterations of the material; administrative, familial, and other types of non- or extra-academic support; and surely more. Academic books do an all right job of providing space in which to relay this history, or at least the rudiments of it. Journals, by comparison, fail miserably. Here the backstory of scholarly production is apt to be distilled down to just a few sentences. In either case, it's not unusual for academic acknowledgments to end with a disclaimer: "Errors are, of course, my own." While I appreciate the magnanimity of the gesture, it seems to me another way in which to disavow the sociality of scholarly production as a matter of routine.

The issue, particularly with journals, isn't just about saving face. It's also about a form of publication that developed roughly three centuries ago in response to the material constraints of print media. Printed publications suffer (arguably) from a finite amount of space. Saddled with too much content, they become unwieldy, cost-prohibitive, or both. Digital publishing technologies have helped to throw this facet of print media into sharp relief, but lest we forget, the space issue beset printed publications long before the so-called digital age. Indeed, one of the recurrent themes in the history of print media is that of miniaturization, or the desire among readers, librarians, printers, and others to try to maximize content by reducing the size of printed artifacts (Baker, 2001; Stewart, 1993, pp. 37–69).

Still, it's not enough simply to lay blame on print, as if technological conditions alone dictated the form and content of scholarly communication. The first academic periodicals developed in Western Europe at a time when paper, ink, glue, and other commodities essential to their production weren't only difficult to manufacture in bulk but also subject to the vagaries of unreliable distribution networks. The few roadways that existed, for example, were often impassable for months on end because of inclement weather, political disputes, and more (Darnton, 1990, pp. 119–122; see also Febvre & Martin, 1976, p.

[2] The only exception to this might be the rare interview, memoir, or personal reflection, usually in the case of academic superstars.

222). With so much uncertainty surrounding the availability of these and other basic materials, is it any surprise that the earliest academic journal editors and contributors understood that they needed to be frugal with the amount of extraneous matter appearing in their publications?

Furthermore, our present day practices of representing scholarly labor in and through our writings are a legacy of the broader social, economic, and legal conditions present in Europe in the seventeenth and eighteenth centuries. Capitalism was, back then, just an upstart economic system, secondary to both the dominant tides of mercantilism and the receding forces of feudalism (Heilbroner, 1953, 1992, p. 26; Polanyi, 1944, 2001). Copyright and patent laws were only beginning to be established (Rose, 1995), and the foundational concept of private property was, by today's standards, limited in its application. Piracy (to impose an anachronism) and plagiarism (another) flourished under these conditions, along with ample accusations thereof (Johns, 1998). The result was a context in which publicly shared knowledge couldn't be trusted or its source easily confirmed.

Scientific organizations, such as Britain's Royal Society, arose partly in response to this uncertainty. One of their primary aims was to secure professional credibility for their members and for the burgeoning disciplines these groups represented. They did so not only by devising peer review and other practices that would become the mainstays of scholarly publication, but also by certifying priority of discovery. The latter designated, essentially, an informal intellectual property right, or at least an attribution of intellectual propriety (Rose, 1995, p. 30; Zuckerman & Merton, 1971, p. 70). Either way, these organizations seemed to be compensating for the particularities of the sociology of knowledge by codifying in the form of their publications the notion of authors operating, for all intents and purposes, independently. Put differently, the exigencies of the time called for academic journal articles and other types of texts to behave, not simply, but significantly, as "objects of appropriation" (Foucault, 1977, p. 124). For academic knowledge to become the gift it's now widely purported to be, researchers and journal editors felt they needed to suppress the story of the takings on which this gift was premised and then marginalize even the mere traces of those acts—literally—by shunting them off to the hinterlands of their publications.

For better or for worse, these are the habits we've inherited from early modern Europe. Here I'm following Jeffrey Masten (1997) in my use of the word "habit," which, he points out, denotes not only unreflective, repetitive behavior but also clothing specific to some profession (p. 91). Lately I've taken to thinking about scholarly publications in exactly this way—as textual habits, or uniforms we use to dress up our research because they're expected and easily identifiable, not necessarily because they're well suited to the content. This, of course, begs the question: What might a more timely and appropriate fit look like?

There are efforts underway to rethink *what* and *how* our publications communicate. Among the most compelling are those attempting to resituate the social work of academic scholarship, taking it from the realm of extraneous matter, or "paratext," to something approaching text, proper (Genette, 1997). I have in mind Kathleen Fitzpatrick's (2006a; 2006b; 2008) writings about and experiments with what she calls "peer-to-peer review." The term refers to a particular—and particularly innovative—implementation of digital journal publishing, in which the refereeing of scholarly work takes place post hoc. That is, instead of a small cadre of gatekeepers determining which material is suitable for a journal's readers, work is simply posted online in some established venue. The worthiness and value of the work are then adduced by how well it's received by the journal's readers, for whom it's open to response. A system like this could be wedded to versioning software, moreover, which would allow the author or authors to update the piece based on the feedback, if any, they've received. Peer-to-peer review thus is compelling because of its potential to enlarge the scope of peer review, and in the process make the underlying labor more apparent than it is conventionally.

A possible drawback of such an otherwise rich system is the functional separation it still maintains between text and paratext, where comments typically appear, much as they do in blog posts, as an appendix. Text over here, influence over there: Is it possible to embed our social work even more deeply and deliberately in our academic objects? The Commentpress writing platform, which is an offshoot of the popular open source blogging software WordPress, goes a long way toward accomplishing this. It enables users to "annotate, gloss, workshop, [and] debate" within the margins of a text published online, essentially "turning a document into a conversation" (Institute for the Future of the Book, 2011). Powerful, yes, especially since it simultaneously allows users to leaf through layers of revision as the text evolves. One downside, though, is that however extensive a conversation may ensue, it's still represented as peripheral rather than proper to the text. Wikis, on the other hand, may go too far in the opposite direction, dissolving one's contributions too much into the stew of the collective, at least for purposes of university record keeping (Striphas, 2010).

I don't mean to deny the value of these kinds of systems. Each holds significant promise for helping people come to terms with academic labor, and more specifically that of publishing. It may well be that some combination of their features could, in fact, yield a form of publication capable of communicating a richer picture of the work embodied in our textual objects. Lately, though, I'm beginning to wonder if a document-centric approach, which subordinates social to textual relations—much like print publications and the online systems I've just mentioned—is the best way to go about things. What might happen, for example, if an academic publication were architected not first and foremost around the principle of content management, but instead, say, social networking? The latter is significantly content driven, yet the content always branches out from a clearly defined backbone of social relations—relations that develop more or less out in the open and in tandem with the material that's shared.[3] I should add that there are plenty of free, open-source tools to begin experimenting in this vein;

[3] "The Faculty of 1000" (http://f1000.com/), billed by *The Chronicle of Higher Education* as the "Facebook of science," is one experiment along these lines (Macilwain, 2011). On the humanities side of things, HASTAC's (http://hastac.org/) recent addition of a "groups" feature to its website holds significant promise, as does CSeARCH, the Cultural Studies E-Archive (http://scm-rime.tees.ac.uk/VLE/DOMAIN/CSeArch/TABS/About.asp)—although the latter is not specifically architected around the principle of social networking.

among them is Elgg, a flexible platform for social networking and content sharing with which I plan to begin tinkering over the next couple of months.

The benefits of turning the social work of academic publishing inside out could exceed matters of openness. In fact, we might end up broadening the definition of higher education along the way. One of the things I most enjoy about being a professor is the ongoing interaction I get to share with colleagues—smart people with whom I'm in dialogue and from whom I'm constantly learning. However important the traditional student-teacher relationship may be, it's not the sum total of the education that occurs in university settings. We professors also learn from our peers, teaching and mentoring one another about the specifics of some subject matter and about the broader ambit of the profession. Scholarly communication is no exception to this. Sure, it's about gatekeeping—often frustratingly so—but it fulfills an indispensable phatic function as well. The so-called "invisible college" of academic life is real, manifesting itself in a citation someone might casually share with you in an office hallway, a trusted friend's written feedback on a draft essay, readers' reports, an editor's feedback, and any number of other—usually private—exchanges. To call this work "service" is to do it a disservice, for it's what makes higher education dynamic, relevant, and about so much more than the pursuit of degrees.

What I'm proposing, I suppose, is something along the lines of a *visible* college, one that would realize itself in publications better equipped to convey the sociality of academic labor in general, and of academic scholarship in particular. I should be clear, however, that I'm not calling for absolute or compulsory transparency. There still are good reasons to limit access to the backstory of scholarly production. Beyond the face-saving issues I noted above, there are the sensitivities involved in working with human subjects, not to mention some very real concerns about how systems like the ones I've been talking about could not only augment but intensify academic labor. All that to say, approach, but approach with caution; know that, alone, new forms of publication aren't enough. Much like our counterparts in the 16th and 17th centuries, we'll also need to feel out the norms of social interaction most appropriate to our scholarly needs.

But for the time being, we academicians continue gazing skyward. And as we do so, we send mixed messages about our labor and its value. Authors and outputs loom large, like city skyscrapers, casting long shadows over the hustle and bustle happening below—which is to say, all around us, every single day. Doesn't this welter of activity deserve its place in the sun?

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http://www.diffandrep.org/wiki

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