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## Introduction

### ACCOUNTING FOR COAUTHORSHIP

What happens when writers compose together? This book tackles that question by introducing an interactionist theory of collaboration, one that focuses as much on the objects collaborators share as it does on the relations with one another they must negotiate. More specifically, it introduces an approach for asking questions about the work of collaborative writing by drawing attention to the complex discursive assemblages within and out of which collaborative writers compose. I call this theory *interactionist* because most scholarship positions collaboration as a dialectical engagement between individual human subjects—that is, it assumes collaboration is at root two or more people engaged in talk. But the approach developed here complicates such dialectical models to highlight the necessary role objects play in collaboration. What collaborators do is interact with and manipulate shared objects of attention, objects both material and abstract, and these objects—all the things that populate the rhetorical ecologies of a collaboration, not the least of which is an emerging text—can and do resist the “talk” collaborators invent as they write together. In other words, the genesis of collaborative composition “does not reside in a set of objective rhetorical abilities of a rhetor [or set of rhetors]”; it instead “exists at the intersection of a network of semiotic, material, and yes, intentional elements and relational practices,” to echo Carl Herndl and Adela Licona’s postmodern theory of rhetorical agency (2007, 137). Accordingly, successful collaborative writers must learn to recognize and adapt to the ways collaboration can fundamentally challenge the habits and expectations we associate with and bring to the practice of writing.

Indeed, I am never more aware of how I write as I am when in the midst of coauthorship. I can clearly recall an experience from several years ago, for example, when I coauthored a book chapter with two colleagues with whom I had never written. All of us had experience with coauthorship, just not with one another. After our first meeting to discuss the project, I was confident our collective experience as collaborative

authors would make the process easier than it otherwise would be had we not had such experience. But as we moved from the brainstorming to inscription stages—we were constructing the draft as a shared Google Doc—I developed an anxiety about my usual method of using chunks of freewriting to serve as placeholders for ideas not yet fully conceptualized. The three of us had agreed to start composing separate parts of the draft we would then take over from one another, round-robin style, until the draft was fully fleshed out. When I write, however, I like to bounce around from one section of text to another as inspiration hits, so I often write messy placeholder paragraphs when I'm unsure how best to articulate this or that idea but nevertheless want to get something on the page. But as I read the sections of text my coauthors were writing, they looked more cohesive and polished than my own scattershot musings, devoid as mine were of a clear developmental structure. I soon grew anxious with self-doubt. Was my method going to slow down the progress of getting to a full draft? Should I send my coauthors an apology even though I had already told them this messiness is just part of my process? Moreover, had I grown too reliant on this method of drafting? Was it inhibiting me from expanding my scholarly repertoire and learning how to compose using a wider array of strategies?

The next time we met as a group to discuss the draft, one of my coauthors apologized for what she perceived as the inefficiency of her own drafting method. Indeed, all three of us confessed feeling self-conscious in one way or another about our individual habits of composing, habits that seem to get amplified and become more discernible when we know they are on display to others. As we continued to work on the draft, we also continued to discuss how each of us writes, a kind of side conversation that allowed us to step back and hold in check the anxieties all writers, especially collaborative writers, inevitably face from time to time.

By default, then, we might say collaboration invites uncertainty because writing with others often requires us to reorient ourselves to the labor of writing itself. This uncertainty is one of the reasons collaborative writing can be so difficult, and why many people shy away from it. Writing is an intimate, private activity for most of us. In this way, dialectical models of collaboration make sense insofar as they focus on practices of interpersonal exchange for managing the uncertainties that are part and parcel of writing with others, which is implied in Charlotte Robidoux and Beth Hewett's definition of collaboration, for example, which emphasizes such a dialectical focus: "Collaboration can be understood as a strategic and generative interactivity among individuals seeking to achieve a common goal" (2009: 4). But collaborative composition,

like all discourse production, is materially situated in ecologies we can only ever partially distinguish by observing the various relations between and among the many objects populating these environments, including the collaborators themselves. Accordingly, I believe the discipline of writing studies, including rhetoric, technical communication, and other fields invested in the theory and practice of writing, like communication and education, can benefit from a theory of collaboration that goes beyond conversation as the conceptual locus for understanding what collaborators do when they write together.

This is not to say conversation, or the talk collaborators produce, shouldn't be accounted for as a critical component in the work of collaborative composition.<sup>1</sup> Far from it. Despite the many ways collaboration might be defined, most uses of this concept imply some level of focused, deliberate engagement among individuals, engagement that no doubt depends on effective teamwork, including the ability to effectively communicate with one another. Accordingly, when teachers, researchers, and other literacy professionals discuss collaboration or otherwise try to explain it as a particular kind of practice distinct from the work of solo writing, they often focus on the interpersonal dynamics collaborators must negotiate and how these dynamics relate to and inform the procedural work of coauthorship, like developing a work plan, sticking to a schedule, delegating tasks, and so forth. Consequently, after wading through advice and best practices focused on how to *set up* a collaboration—the “task design” of a collaborative project (Bremner et al. 2014)—there is usually little said in these discussions about what the actual *writing* in collaborative writing entails. To put this as a question that highlights one of the practical concerns I consider in this book, How do collaborators negotiate the labor of rhetorical invention?<sup>2</sup>

But this question isn't new. In fact, composition theorists began asking different versions of this question starting in the early 1980s as the field's social turn was getting underway alongside the rise of what James Berlin labeled “epistemic rhetoric,” a pedagogical approach that recognizes language as “a social—not a private—phenomenon, and as such embodies a multitude of historically specific conceptions that shape experience, especially ideological conceptions about economic, political, and social arrangements,” arrangements that affect “the dialectical process involved in the rhetorical act” (1987, 166). It is through such a social-epistemic lens that in her book *Invention as a Social Act*, Karen Burke LeFevre, for example, proposes that rhetorical invention “is best understood as occurring when individuals interact dialectically with socioculture in a distinctive way to generate something” (1987,

33). LeFevre goes on to outline a continuum of social perspectives that account for rhetorical invention, one of which she labels “collaborative.” To explain this collaborative perspective, she draws on the sociological theory of George Herbert Mead to model collaboration as a progressive kind of dialogue: “One person acts, and in the act of making the gesture, calls out for a response in the other. Something new is created here. . . . New meanings are thus brought about into existence by means of a social interaction involving a symbolic gesture and response” (62).<sup>3</sup> While LeFevre challenges a “view of invention as the act of an atomistic individual producing a discrete text,” the idea of collaboration in her theory ultimately gets subsumed in her social view of invention as “one in which individuals interact with society and culture” (121). In social-epistemic terms, then, collaboration is not a strategic activity as much as it is a discursive mechanism always at work in both the localized talk of specific collaborative teams and the more abstract talk that constitutes the stuff of “society and culture” writ large.

While I appreciate that social-epistemic approaches align collaboration with the rhetorical canon of invention, thus positioning it as a concept with which to understand the ways our discursive interactions function in relation to the creation of knowledge, these approaches end up falling back on generalities with little pragmatic uptake. Consider, for example, what Gregory Clark writes at the beginning of *Dialogue, Dialectic, and Conversation*, a book published just three years after *Invention as Social Act*. Clark explicitly positions *Dialogue, Dialectic, and Conversation* as an extension of LeFevre’s work: “In communicating we collaborate with others in constructing and continually reconstructing from our commonality the community that enables us, both individually and collectively, to survive and progress, a community comprising people engaged in an ongoing process of renegotiating the beliefs and values—and consequently, the action—they can share” (1990, 1). Clark explains such a philosophy of communication relies on two assumptions: first, our communication doesn’t merely represent but actually constitutes reality; and second, we communicate with, not just to, one another. “When communication is understood on the basis of these two complementary assumptions,” he goes on, “it becomes, above all, a collaborative process through which a community of people construct a shared understanding of their common experience that provides the foundation for their continued cooperation” (2).

Mindful of the risk of reducing the complexity Clark no doubt aims to capture in this theoretical articulation of how communication and knowledge are intertwined, I didn’t use this philosophical explanation

to better navigate the discussions I shared with my two coauthors mentioned above as we came to terms with our own writerly insecurities we believed were impeding the progress of our collaborative work. To the extent that disclosing our insecurities with one another helped establish some “common experience” that promoted “continued cooperation,” Clark is correct, but what are we to do, and indeed what *can* we do with this understanding? Insofar as Clark is not making a propositional argument but a declarative one that outlines “a social perspective on the function of writing,” which is the subtitle of his book, such a theory leaves agency in the wind since such collaborative processes are what, according to Clark, comprise the foundation of communication in the first place.

Herein we find one of the primary shortcomings of social constructionist epistemology as a philosophical foundation for understanding collaboration, namely that it directed scholars to focus on “the social” and its constituent abstractions like “the cultural,” “the political,” “the economic,” and so forth as the primary sources of struggle that collaborators must negotiate if their work together is to be successful. At the same time, these articulations of a social-epistemic rhetoric made it possible to invoke the idea of collaboration itself to underscore in tautological form the import of social turn epistemology more generally, just as Clark goes on to do when he theoretically parses the concepts of dialogue, dialectic, and conversation.<sup>4</sup> Yet this reflects the larger social turn logic with which Kenneth Bruffee explicitly argues for the value of collaboration when he suggests how it “provides the kind of social context, the kind of community, in which normal discourse occurs: a community of knowledgeable peers” (1984, 644). By “normal discourse” Bruffee means consensus, an idea I trace the history of as it relates to the mechanics of collaboration in chapter 3, but here my point is simply that social constructionism limits how we might understand collaboration as a deliberate practice writers pursue to invent novelty, emergent ideas and articulations writers can anticipate but never predict as they enter into and pursue the discursive work of collaboration.<sup>5</sup>

To return to my earlier example, the three of us decided to collaborate on that particular project because we recognized each of us could bring unique disciplinary perspectives that, when combined, would allow us to engage our research subject from a truly interdisciplinary lens, one that had been expressly cultivated for that specific purpose. I knew what ideas I wanted to bring to the table, as did my coauthors, but until we sat down and started to plan what we wanted to write, the best each of us could do was speculate about how our respective ideas would

and would not fit together, and what, as a result of this engagement, we would end up writing. But in large part that was the point. That is, we didn't pursue this collaboration because we figured it would be an easy way to churn out another line on our CVs. We pursued it because we knew doing so would allow us to discover and articulate ideas and observations we wouldn't have invented had we not collaborated in the first place. Indeed, I'd wager that many academic collaborators coauthor for similar reasons.

After all, consider those occasions when collaborative writers might argue the finished product of their collaboration is greater than the sum of its parts. Such a claim prompts me to think about the phenomenon of emergence. "When two separate causes simply add or mix themselves in their joint effect, so that we can see their agency in action in that effect, the result is a mere 'resultant' but if there is novelty or heterogeneity in the effect then we may speak of an 'emergent'" (DeLanda 2011, 382). Here Manuel DeLanda is summarizing George Henry Lewes, the nineteenth-century English philosopher who coined the term "emergence," but DeLanda also likens emergence to the difference between physical and chemical reactions and demonstrates how with the latter the result is not a mere combination of its original parts but something new, something different. When I begin collaborative writing projects, it is this emergent discourse I most anticipate, those articulations that result not despite but because of the interactions between each of our "separate causes" as collaborators.

But social-epistemic theories can't account for emergent discourse because collaboration as such is overdetermined in those conceptual schemes. Consider how social-epistemic rhetoric offers little relief to scholars who, for instance, recognize they might be forced to account for their respective contributions to a collaboration; Naomi Miller laments that academics "are encouraged to dissect any collaboration into absurdly artificial allocations of 'responsibility.' Surely, excessive attention to 'which half is yours' can cause us to lose sight of the whole" (2003). I agree with Miller—such concerns do prompt us to lose sight of what coauthors can accomplish. Is it useful, really, to enter such critical discussions holding up a theoretical argument that shows "how texts, whether individually or jointly authored, should be considered collaborative"? (Thralls 1992, 64). But this simplification is what a social-epistemic approach to collaboration offers; it ultimately boils down in practice to the particular strategies for arguing, in theory, that all writing is social in one way or another. And as for the place of collaborative writing in the courses we teach? Following Stephen Yarrbrough's criticism of

social-epistemic rhetoric, “If the doctrines of social constructionism are true then it logically follows that everyone who teaches composition is already doing what can be done to help students improve their writing” (1999, 221). If collaboration *just is*, if at the end of the day collaboration is something we can’t *not* do when we write or otherwise communicate, then logically speaking there is no pragmatic difference between asking students to write alone or together. At best, collaboration simply becomes a means to divide labor, delegate responsibility, and play at what is already at work shaping our discourse.

But we know better. That is, we know collaboration can yield pragmatic differences in the writing collaborators undertake together. And we also know (the *we* here specifically means those of us who were trained and work in humanities fields) we probably aren’t doing enough to engage our students in the skills and dispositions needed to be effective collaborative writers. Perhaps this is why Cathy Davidson, renowned cultural historian of technology, suggests a different approach in her *Chronicle of Higher Education* column “What If Scholars in the Humanities Worked Together, in a Lab?” In this piece Davidson discusses humanities centers and the different models they assume—some provide course releases for faculty to pursue their individual scholarship, some shape various programming around a particular theme and hold events—but she also speculates about what a humanities center would look like if it was structured more like a scientific lab. As Davidson sees it, “The ideal of individual authorship and genius, so prized in the humanities, often contributes to ineffective models of intellectual innovation and creates poor departmental and university citizens,” so she suggests humanities disciplines might imagine the creation of spaces that “could borrow from the collaborative aspects of the lab, where even the most senior and junior members count on one another, and where joint publication and grant applications acknowledge and formalize a structure of mutual dependency” (1999, B4).<sup>6</sup> Davidson draws attention to the fact that when academics engage a common project from start to finish, like a piece of collaborative writing, the potential for discovery—what I describe above as *emergence*—becomes all the more possible. But this is a commonsense claim, right? Don’t most academics believe research collaborations enhance the potential for discovery?

Well, yes and no.

Popular trade books like *Where Good Ideas Come From* (Johnson 2010), *The Silo Effect* (Tett 2016), and *Group Genius* (Sawyer 2017), to name just a few in what is a very long and growing list, explain and in most cases advocate for the benefits of collaboration. They do so, however,

in general terms and by drawing on examples from business, medicine, technology, and the entertainment industry—these are trade books, after all, so the examples must have popular appeal. The market for books geared toward academics about the power of collaboration is, not surprisingly, much smaller—for a very good reason. Academic collaborators must navigate disciplinary and institutional economies that define and determine how collaborative work is valued and assessed, so while it's one thing to advocate for the value of collaboration in the abstract, it's another thing to account for and promote this value in terms that have significance in the ritualized routines of academic disciplines, college and university administrative structures, and department-level policies and procedures. Abstract defenses of collaboration are thus difficult to translate in ways that carry currency across academia's myriad institutional landscapes, including what are sometimes competing authorial economies.

Most academics probably know or at least recognize the value of collaboration, in other words, but we learn to parse this value in different ways. As a result, and when it comes to collaborative writing in particular, even though coauthorship in most academic disciplines is more common now than it was in the past, undertaking a collaboration can be difficult and even a bit risky if one is unfamiliar with what counts, both literally and figuratively, in these various economies.

\* \* \*

Navigating coauthorship in academia's authorial economies requires knowledge of three different but obviously related concerns. First, who gets to count as an author? This question is simple if not obvious, but in the physical sciences, for example, determining what counts as authorial labor can be tricky, especially when dozens or even hundreds of people participate in the design and testing of a research project.

Second, how should coauthorship be attributed? Beyond asking the question of who counts as an author, we must also decide how to ascribe singular texts to plural authors, to echo the title of Lisa Ede and Andrea Lunsford's (1990) important study of collaborative authorship. Should name order imply level or import of contribution? Should footnotes or other textual features be utilized to qualify which contributor did what? These are thorny questions, but important ones given the responsibilities claims of authorship carry.<sup>7</sup>

Third, and finally, how should coauthorship be credited as a form of labor? Does it make sense to assume a text coauthored by two people, for instance, took more work on the part of each of those writers than a comparable text coauthored by three people? And how should that

labor be ascribed value in end-of-year reports, tenure and promotion guidelines, funding decisions, even author-impact algorithms?<sup>8</sup>

No matter the discipline, academics must constantly navigate these authorial demands. In some cases, resources exist to assist with making these determinations. Professional societies and advocacy groups release policy recommendations, organizations like the American Psychological Association and the American Medical Association develop style guides, publishers adopt guidelines, and individual schools and departments establish criteria for deciding how to represent or otherwise “count” coauthorship. With that said, “No standardized policies enable collaborators throughout academe to prevent disputes about authorship,” write Ann Austin and Roger Baldwin in their 1991 report on faculty collaboration for the Association of the Study of Higher Education; moreover, they continue, these decisions are sometimes made unilaterally via top-down university assessments (68). While Austin and Baldwin devote significant attention to the challenge of crediting collaborative endeavors as they review the scholarly literature and report on various data related to authorship practices in higher education, they never question the assumption that the products of collaboration must be conceptualized as the sum total of individual efforts. “In a meritocracy like higher education,” they say, “it is not sufficient to recognize [equally] all contributors to a joint publication. The system demands to know who contributed more and who contributed less to the collaborative endeavor” (67).

Even though Austin and Baldwin’s report was published thirty years ago, the logic it reflects concerning the nature of authorship in higher education still persists. Specifically, and despite famous assertions by the likes of Roland Barthes (1977) and Michel Foucault (1979) that the author is dead—at least the one understood as singular and autonomous—coauthorship still gets positioned as a zero-sum undertaking. As Martha Woodmansee reminds us, “By ‘author’ we mean an individual who is the sole creator of unique works the originality of which warrants their protection under laws of intellectual property known as ‘copyright’ or ‘authors’ rights’” (1994, 15). With that said, she notes, “The notion that the writer is a special participant in the production process—the only one worthy of attention—is of recent provenience. It is a by-product of the Romantic notion that significant writers break altogether with tradition to create something utterly new, unique, in a word, ‘original’” (16).<sup>9</sup> T. S. Eliot, that paragon of literary modernism, illustrates the tenuousness of such conceptions of authorship when he stipulates the writer’s job is to impersonally channel the tradition (i.e.,

defining works of a particular literary epoch) into their work as part of a “continual surrender of [them]self” so the writer’s own personality remains separated from the work itself, thus positioning authorship as a kind of tapping into the collective sensibilities of a particular school of thought; yet Eliot also stipulates that true artists are masters of concentration who through acts of creation produce “a new thing resulting from the concentration” (2007, 539, 541). Even starkly anti-Romantic critics, that is, rely on conceptions of the individual author as chief controller of the creative process. As a result, the Enlightenment philosophy that positions knowledge as something humans discover, combined with Romantic conceptions of inspiration individuals conjure through focus and will, has resulted in a set of sensibilities that supports the economic rationalism that continues to influence how collaborators are conditioned to understand their work.

Even though she’s not discussing coauthorship specifically, Kathleen Fitzpatrick points out how we are disposed to “draw boundaries around our texts” and to consider them distinct objects “separate from those of other authors” (2011, 72). This limited view is certainly one of the reasons coauthorship continues to be undervalued in the humanities disciplines, despite the fact that in literary studies, for example, teachers and scholars continually ask “fundamental questions about the very institution of authorship, how it came about, the aporias that underlie it, the economy that supports it, and the legal constructs invoked to justify it” (Biagioli and Galison 2003, 3). This irony hasn’t gone unrecognized. In the Modern Language Association’s most recent report on evaluating tenure and promotion, which is now more than a decade old, its authors acknowledge that collaboration is often treated with suspicion and assert the need “to devise a system of evaluation for collaborative work that is appropriate to research in the humanities and that resolves questions of credit in our discipline as in others” (2007, 57). The report even suggests humanities fields could learn from the “rigorous systems” devised by “academic disciplines in the sciences and social sciences” for assigning authorship credit (56–57). But the suggestion that in the humanities we should be shaping policies for collaboration modeled around authorship practices in these other disciplines implies these latter fields have figured out how such policies should work. It doesn’t take much looking, however, to locate ongoing debates in these other fields that mirror those in the humanities, especially when it comes to the three criteria I mention above.

Consider the question of how to credit collaborative work. This is a challenge the natural and physical sciences have struggled to address.

For instance, consider how author-impact metrics have become ubiquitous in these disciplines, so much so that researchers are increasingly expected to quantify the impact of their scholarship as a performance measure. One of the most common of these metrics is the h-index, which is intended to assess the relative impact of a researcher based on the number and reach of their publications. Developed by the physicist Jorge Hirsch (thus the name of the metric, the Hirsch index) in 2005, the h-index works by calculating the number of citations of a scholar's work against the total number of that scholar's publications. If someone has a score of  $h$ , that means  $h$  of their publications must have been cited at least  $h$  times. For any readers who are like me and get apprehensive around what amounts to some pretty basic math, the h-index is not that difficult to understand once you see an example or two of how a score is derived from this metric. Let's say Eric has published four articles and each of these articles has been cited four times; these numbers mean his h-index score is 4. His score will remain the same even if one of Eric's publications has been cited twenty-eight times because the highest possible value of  $h$  is the total number of one's publications. Now let's say one of Eric's articles has been cited six times, one four times, and one three times, and his last publication has been cited only once; his h-index score is 3 because that is the highest shared number of citations his work has relative to the total number of his publications. What is important to note for the present discussion, however, is that the h-index doesn't take into account whether a publication has multiple authors.<sup>10</sup>

So, if two scholars each have an h-index score of 15, but three-fourths of the first scholar's publications are coauthored compared to one-fourth of the second scholar's, does that mean the second scholar is more productive? This hypothetical question only scratches the surface of the problems collaboration poses when it comes to author-impact metrics. "The more researchers involved in a project, the more complicated it is to quantify their individual contributions," something established metrics like the h-index can't do because they "ignore collaboration or assume equal contribution of each coauthor" (Stallings et al. 2013, 9680). So says a team of researchers who in 2013 proposed a new metric—in the same journal Hirsch's metric first appeared—that assesses "true credit shares" of coauthored publications, thus providing an avenue for "fairer evaluations of a researcher's individual scientific impact" (9680, 9685). I won't pretend to understand the mathematics at work in this proposed metric (they call it the "A-index"), but it uses three axioms to determine a set of scores that can be used to identify a so-called credit vector, which in turn is used to determine a researcher's

relative level of productivity as a collaborator. What is worth noting about this proposed collaborative productivity metric is that it prevents coauthors from claiming credit shares the sum of which exceeds the total percentage of the publication. That is, two coauthors can't each claim 100 percent of the publication credit; the most they could each claim is 50 percent.

To follow this metric, in fact, requires a team of researchers to first group themselves into relative credit categories. A five-person research team, for example, might identify two credit levels, the first one worth 60 percent of the total publication credit and the second level worth 40 percent. After they split themselves into these two groups, credit share can be weighted according to each category and then distributed. One's A-index score is determined by the sum of these various credit-share weights in conjunction with another metric, like citation counts or journal-impact scores.

The point, however, is that these researchers believe quantifying collaboration is not only necessary but also possible. Biologist Cagan Sekercioglu also insists this type of calculation is necessary, but his proposed formula is much simpler: "The  $k$ th ranked coauthor can be considered to contribute  $1/k$  as much as the first author" (2008, 371). If three coauthors are listed on a publication, the second author can be assumed to have contributed 50 percent of the first author's contribution, and the third author 50 percent of the second author's contribution, and so forth. In the end, what ultimately matters for proposals like these is, to use Sekercioglu's words, that "coauthors' contributions can be standardized to sum to one" (371).

There are two obvious shortcomings with proposals like these. First, despite their quantitative sheen, these formulas require collaborators to first determine a set of qualitative values. When it comes to the A-index, coauthors must invent credit-level categories and then decide which collaborators should be slotted into which categories. But these values will always be relative from one collaboration to the next, so the only way to "accurately" represent the different contributions to a coauthored publication is to specifically qualify who did what, something that, ironically, the researchers behind the A-index do in a note that indicates who designed, performed, and analyzed (three separate tasks) the research and then who actually wrote the article (Stallings et al. 2013, 9680). But there is another problem with these proposals, and that is what happens when instead of a handful of contributors to a publication, that number explodes into the tens, hundreds, or even thousands. Sekercioglu notes, for instance, that in 2006 the journal *Science* published at least one

hundred papers with over five hundred coauthors, a fact that leads him to speculate, suspiciously I might add, about the legitimacy of collaboration in such scenarios (2008, 371).

But why would it be so hard to imagine that a group of 475 physicists associated with the CERN Large Electron Positron (LEP) can all be credited for the discoveries that have resulted from the collective efforts of their research? This isn't a hypothetical example. Peter Galison (2003) looks at multiple examples of collective authorship in the field of high-energy physics in his discussion of why the concept of authorship itself can be problematic when it gets treated as a zero-sum activity. After reviewing the authorship protocols of at least five large-scale "monster" collaborations, as he calls them, Galison notes that contrary to sentiments like those of Sekercioglu, who thinks too much collaboration diminishes credibility, the opposite is the case when physicists publish apart from the collective authorship of a research team.

Disunity of authorship appeared to many if not most participants in these large collaborations as tantamount to epistemic subversion. All of these gestures of control served to create both an internal and external social-epistemic unity: they aimed at making the knowledge embodied in physics claims come from the group as such, not from its component parts. . . . The whole of these massive authorship protocols aims to form the "self" of a monster collaboration so that the "we" of the collaboration can produce defensible, authored science. (345).

The collective identity of a collaboration, in other words, holds more weight and is thus more important in these cases than individual claims of credit. Indeed, here the A-index would not only be impossible (How many different credit levels would have to be invented?), it would fundamentally undermine the epistemic authority claimed by the collective not despite, but because of, their collaboration. More to the point, and as Sarah Scripps, Soumitra Ghoshroy, Lana Burgess, and Allison Moss observe, "The vast increase in cost, scale, and complexity of scientific ventures over the course of the twentieth century" means "collaboration has become less of an option and more of a requirement" (2013, 48). In fact, single authorship in the sciences has all but disappeared, while in the social sciences it has become the exception rather than the norm (Endersby 1996; O'Brien 2012). But the fact that coauthorship has become more normalized in these disciplines doesn't necessarily mean the policies they use to account for it are unambiguous.

In the social sciences, for example, it is well established that authorial labor is designated according to the relative importance of each individual collaborator's unique contributions. But how does the APA instruct

practitioners to make these determinations? Section 8.12 of the APA's guide *Ethical Principles for Psychologists and Code of Conduct* indicates that authorship attributions can only be given to individuals for work they "have actually performed or to which they have substantially contributed." Moreover, it reads, "principal authorship and other publication credits [should] accurately reflect the relative scientific or professional contributions of the individuals involved, regardless of their relative status" (American Psychological Association 2017). The double use of "relative" to qualify what is an otherwise direct statement opens up for debate the question of whether standardized practices of *attribution* can and should themselves imply standardized practices of *authorship*. That is, to what extent can a policy for representing the labor of collaborative authorship actually capture in any meaningful way what such labor consists of in any given case?

Most of us are familiar with the system of numbered authorship practiced in the social sciences and devised by the American Psychological Association: "The general rule is that the name of the principal contributor should appear first, with subsequent names in order of decreasing contribution" (2001, 350). But this system of numbered authorship doesn't address the question of what counts as authorship itself, especially when individuals might contribute to a collaboration without necessarily being responsible for generating its products, such as being directly involved in the writing of an article or report. In this case, the APA's guidance aligns with the standards outlined by the Office of Research Integrity, which is part of the US Department of Health and Human Services, in its online handbook *Avoiding Plagiarism, Self-Plagiarism, and Other Questionable Writing Practices: A Guide for Ethical Writing*. Here is the advice it offers for establishing authorship:

Generally, examples of substantive contributions include, but are not limited to, aiding in the conceptualization of the hypothesis, designing the methodology of the investigation and significantly contributing to the writing of the manuscript. "Menial" activities, such as entering information in a database or merely collecting actual data (e.g., running subjects, collecting specimens, distributing and collecting questionnaires) are not, by themselves, sufficient grounds for authorship, but should be acknowledged in a footnote. (Roig 2015)

Even though it isn't explicit, authorship according to these guidelines implies some type of *invention*, the generation of ideas, methodologies, or text that directs or otherwise informs the overall scope of a research project. But even if we can draw a line that separates such activities from those considered "menial," it is still unclear how authorship becomes

something distinct from contributorship, say, or editorial assistantship. Language should matter, obviously, so to say that “aiding” in the conceptualization of a hypothesis, for example, counts as a “substantive contribution” is to utilize a set of terms that on its face hardly clarifies the nature of authorship as a kind of labor.

Speaking of language, what significations inform distinctions academics make between the terms *research* and *scholarship*? What about the conceptual metaphor of *discovery* versus that of *creation*? To what extent does the work of writing in a particular field imply the *doing* of research as opposed to its *transcription* into a text? And where does the term *author* fit in these discussions, if at all? As Mario Biagioli (2003) points out, for example, after years of complaints about the challenges of attributing authorship, leading medical journals like the *Journal of the American Medical Association (JAMA)*, *Lancet*, the *Annals of Internal Medicine*, and the *American Journal of Public Health* dropped the *author* designation altogether in favor of the term *contributor*. Biagioli explains the new policy adopted by *JAMA*: “Each name should be attached to a verbal description of that person’s contribution, and the contributors list should be published on the article’s first page” (2003, 265).<sup>11</sup>

But even when contributor roles are clearly qualified and delineated in a standard fashion, how should those things requiring attribution—data collection, research design, claims, written manuscript, and so forth—be understood? Are they *content*, *creations*, *constructions*? In the social sciences, the metaphors available to talk about these products often converge in ways that trouble easy distinctions between research and the inscription of research. Yet these distinctions persist. Referencing the work of Bernard Berelson’s (1960) study of the first one hundred years of graduate education in the United States, Austin and Baldwin say that to understand the nature of academic coauthorship, we should distinguish between the “word disciplines” and the “data disciplines” (1991, 25). While James Endersby proposes that “modern science and the humanities can be distinguished by the likelihood of collaborative writing and research”—a comment that on its surface seems generalizable enough—he goes on to say humanities scholars “may continue to write alone as their subject matter is less likely to require the talents of others for the completion of their works” (1996, 380–81, 384). Even though this is speculation on Endersby’s part, such an observation reinforces negative stereotypes about research in the humanities as being somehow less rigorous, or more basic, than research in other areas.

In humanities disciplines, where single authorship does remain the standard—one recent study concluded 90 percent of humanities

scholarship is written by single authors (Scripps et al. 2013, 48)—there has been no shortage of attempts to reorient and restructure, respectively, the expectations ascribed to notions of scholarly authorship and the systems of evaluation that encourage these expectations in the first place. In her 2000 presidential address for the annual meeting of the Modern Language Association, for example, Linda Hutcheon bemoans that the organization still holds tight to “a model of the humanities researcher as, to cite Jonathan Arac [1977], ‘the figure of the creator, treated as a distinctive, single isolated individual,’ not unlike the Romantic genius” (2001, 524). In her own MLA presidential address two years later, Mary Louise Pratt echoes this sentiment: “Humanists’ isolation or disengagement is greatly of our own doing. Humanists need to get beyond what Goldberg and Davidson [2004] call the romance of the scholar-prophet.” Pratt insists on the need for “a culture of collaboration” (2004, 426). And in yet another MLA presidential address, this one more recent, Russell Berman criticizes the “garden variety illusion that scholars, and we humanists in particular, properly work in sublime isolation, when, in fact, we are always engaged in teamwork and networking,” which he follows up by asserting the need for more “vibrantly collaborative scholarship” (2012, 451).

While public addresses such as these serve an epideictic function more than a deliberative one, to its credit the MLA has tried to be more forthcoming in its attempts to encourage greater recognition and reward for collaboration. Consider the opening paragraph of the “Evaluating Collaboration” section in the 2006 “Report of the MLA Task Force on Evaluating Scholarship for Tenure and Promotion”:

Solitary scholarship, the paradigm of one-author–one-work, is deeply embedded in the practices of humanities scholarship, including the processes of evaluation for tenure and promotion. Collaboration, however, offers significant opportunities for enterprising, untenured scholars to tackle problems or interdisciplinary topics too formidable in scale or scope for an individual. Sometimes collaboration simply offers the most satisfying way to approach an issue or problem in an article or a monograph. In fact, recent technological advances have made collaboration with distant colleagues easier, faster, and more efficient. And the special challenges involved in creating digital scholarship have led to new forms of collaboration in that arena as well. (MLA Task Force 2007, 56)

Notice the emphasis on practice in this statement, specifically its invocation of “recent technological advances” as a warrant for embracing a more welcome outlook on collaborative work. To what extent do appeals like this one, which point to changes in the infrastructure available to writers, presume that just because the potential for collaborative

authorship has become more ubiquitous, more scholars will actually pursue these opportunities, especially when such work remains undervalued in their home institutions? That is, there remains an obvious disconnect when in an organization like the MLA there can be repeated calls from diverse voices for more and better recognition of collaborative work, but in terms of policy, the most that is offered are suggestions wrapped up in an if-you-build-it-they-will-come ethic.

But the MLA is light years ahead of other humanities organizations. Not only does the American Historical Association (AHA) have nothing on order in terms of policy statements or task-force initiatives that compare to the MLA's, at the AHA's annual meeting in 2017, the speakers on a panel titled "Is Collaboration Worth It?" collectively agreed that, yes, collaboration is worthwhile and should be pursued if possible, but they also agreed such work will be at best undervalued and at worst completely discounted when it comes to tenure and promotion. According to an *Inside Higher Ed* article about the panel (apparently the panel was enough of an anomaly at the convention to garner media coverage), audience members suggested "that the AHA might strategically or explicitly—perhaps through some document or set of guidelines—encourage institutions to reward collaborative research" (Flaherty 2017). Writing about the session in the AHA organ *Perspectives on History*, Seth Denbo (2017), an AHA staff member, acknowledges that the field has a distorted view of authorship, especially when one considers how much interaction takes place among historians, archivists, curators, editors, peer reviewers, and the like. But as Flaherty (2017) reports, one of the speakers on the AHA panel, Joseph Locke, nevertheless insisted "historians might be surprised by the appetite for collaboration, despite prevailing attitudes, as evidenced by scholars' willingness to contribute to his project," an open-source, collaboratively written history textbook. As other responses to the "Is Collaboration Worth It?" panel attest, Locke seems to be right about the growing desire among historians to pursue collaborative work; what's missing are accessible outlets for exploring different models of coauthorship that can be studied and replicated (Oatsvall and Scribner 2017; Poska and Amussen 2018).

If these responses are representative, the primary challenge to collaborative writing in the humanities has less to do with the material structures of support that make coauthorship possible, or even with the persistence of a narrowly conceived author construct, than it does with its collective inability to make visible the work of collaborative authorship in practice. It's not that there are disciplinary or institutional resistances to collaboration so overwhelming that coauthorship should be completely

avoided, that is, nor is there a dearth of sound arguments for abandoning the myth of the solitary author. When it comes to the latter, humanists have them in spades; with the former, efforts to discourage collaboration increasingly come across as anachronistic, if not draconian. The problem is that humanists by in large have a difficult time imagining the viability of coauthorship as a creative rather than strategic resource.

It's easy, after all, to imagine the strategic benefits of collaboration when cowriters come together because they hope to capitalize on how their different backgrounds or sets of expertise might be mutually beneficial—this is what initially brought the three of us together in the example from my own experience I discussed earlier. It's much harder, however, to anticipate the novelty cowriting has the potential to manifest when you believe a writing project can be just as effectively, not to mention more efficiently, completed as a single author when you have been implicitly trained to think this way.

So what do we offer by way of authorship practices in writing studies, a multifaceted field that increasingly exerts its disciplinary authority as one that bridges the humanities with the social sciences? The closest thing to a policy guideline devised by compositionists can be found in a position statement released by the Conference on College Composition and Communication that describes the field's range of research practices. Tucked onto the end of a section titled "How Rhetoric, Writing, and Composition Scholars Conduct Research" is the following statement: "Scholars in rhetoric, writing, and composition often conduct and publish work collaboratively, and often eschew traditional notions of 'first author,' both because the field typically regards collaborative work as equal partnerships and because the order of names may not indicate contribution levels" (2018).<sup>12</sup> This statement is neither prescriptive, like the APA's system of numbered authorship, nor particularly suggestive, like the MLA's recommendations in its report on evaluation for tenure and promotion; if anything, it reads like a passing observation, one more suited for the fine print of a prescription-drug advertisement than an official guideline collaborators might reference to support their work. But as I've been discussing in this brief review of authorship economies, even the most articulated guidelines for accounting for and crediting collaboration often raise more questions than they answer. Moreover, standardized terms like *principal*, *lead*, and *first author* are at best semantic designations that do little more than reinforce systems of evaluation that atomize scholarly labor.

While attribution policies certainly have their place in debates about how to credit coauthored work, the more pressing need is for greater

recognition of the complexity coauthorship entails. We should not conflate a generic system of attribution, in other words, with the labor required of coauthorship. Rather than ask for clearer guidance on how to attribute this or that piece of coauthored scholarship, then, I believe those of us invested in promoting the benefits of collaborative writing should pursue the more complicated inquiry of trying to better understand what coauthorship entails and why this labor often exceeds standardized practices of attribution in the first place.

Such is the work I begin to undertake in this book by sketching the contours of an interactionist theory of collaboration that draws attention to the complexity of the discursive ecologies collaborators are invented by just as much as they invent, which in turn points to the possibility for more creative and potentially more disruptive inquiry into the economies of authorship that permeate the institutions in which we work. Thus the foray into these economies I just offered so we have a baseline for articulating the stakes collaborative writers are up against if we want to disrupt these prevailing logics that continue to overwhelmingly position collaboration as a zero-sum undertaking.

\* \* \*

In the chapters that follow I make an argument for rethinking our prevailing theories of collaborative authorship by drawing attention to the discursive mechanics that allow coauthors to produce writing in the first place. Given that digital platforms have increasingly evolved to facilitate new and streamlined methods for collaborators to share and produce work, and given that rhetoricians and compositionists are starting to contemplate the uses of new materialist and posthuman theories for understanding writing, now is an opportune time for such inquiry. But my hope for this interactionist theory of collaboration goes beyond my personal investments as both an advocate and practitioner of collaborative writing to echo Fitzpatrick's desire for all academics who are motivated to rethink the ways we approach conceptions of authorship in our theories and practices as teachers and writers, "not only because new digital technologies are rapidly facilitating new ways of working and imagining ourselves at work, but also because such reconsidered writing practices might help many of us find more pleasure, and less anxiety, in the act of writing" (2011, 51)—and, of course, the act of cowriting.

In chapter 1, I consider the current status of collaboration as a critical concept in writing studies by examining in detail a popular commonplace that circulates in the field, the claim that all writing is collaborative. As widespread as this claim is, I suggest it has little pragmatic value for collaborative writers who assert the quality of their work has been

multiplied because of their collaboration. Put another way, the claim that all writing is collaborative flattens the ontological status between texts single authors claim and those claimed by multiple authors. But what is the origin of this claim? And how does it actually get used by scholars and practitioners? The line of history I trace to answer these questions begins with one of the field's most preeminent theorists of collaboration, Kenneth Bruffee, and moves through a discussion of how scholars have tried to come to terms with collaboration alongside another concept that assumed new significance as a result of the social turn, the idea of community. I then turn to recent disciplinary interest in new materialist philosophies to consider an alternative set of ideas about the nature of collaboration—ideas more mundane but also more empirical—as the starting place for speculating about the work of collaborative composition.

In chapter 2, I step back to reconsider the function of talk in collaborative composition. Insofar as social turn approaches have foregrounded the role of dialogue, or conversation, as constitutive of collaboration itself, conceptually we are left with something resembling a chicken-and-egg problem. In what ways can we say collaborators deliberately wield their talk—Do they just take it up, that is, and tackle the work at hand? If collaboration slows down writing processes, which I discuss in chapter 1, and if collaborative writers, like all writers, can't always anticipate what their writing will produce, is it not disingenuous to present collaboration as a process or activity that can be controlled? To engage these questions, I propose the advantage of framing collaborative composition as a noninstrumental technology that affects the capacities writers bring to and foster in collaboration when they engage composing processes together. Taking up my own definition of collaboration as a mutual intervention and progressive interaction with objects of discourse, I position coauthorship as labor that hinges on the ability to anticipate remedies to the various limit-situations (a term I borrow from Paulo Freire [1970]) that all cowriters face, not the least of which is how to account for the development of a draft that does not “belong” to any single writer, or agent, within the collaboration. As I suggest at the beginning of this introduction, social turn theories of collaboration fail to account for the work of rhetorical invention, so this chapter begins such an accounting by calling into question just what it is collaborators control with their talk when they compose.

In chapter 3, I extend the discussion started in the previous chapter to consider the question of agency and how to understand the discursive *techné* that allows coauthors to compose in ways that transgress

the boundaries of their rhetorical capacities as individual composers. Concern about the dynamics between individual and group agency in collaborative endeavors has been at the forefront of the field's scholarship on collaboration, which I review in this chapter by focusing explicitly on the politics of consensus in these debates. Taking up the work of Byron Hawk (2007), Marilyn Cooper (2011), Laura Micciche (2017), and others who have proposed or contributed to theories of rhetorical agency that take seriously posthumanist and new materialist approaches, I theorize cowriting agency as an emergent capacity coauthors develop in relation to the developing object of a shared text. But I also locate this cowriting agency in the sociological theory of George Herbert Mead, who articulated a philosophy of communication that was problematical in scope, meaning that communicative interaction between interlocutors is always motivated by the desire to maintain a common world, one that "is continually breaking down" (1964, 341). In this way, I position cowriting agency as a set of discursive relations collaborators produce that allows them, however contingently, to maintain a "common world" with/in their writing.

Chapter 4 tackles the fraught question of practice and what, if anything, an interactionist theory of collaboration means not just for how we teach collaborative writing but for how we pursue it ourselves. In this chapter, I turn to Bruno Latour and actor-network theory, specifically Latour's notion of what it means to write a "risky account," as a potential site of practice for enacting the rhetorical mechanics of collaborative composition in ways not just more visible but also more useful and interesting, if not provocative. Here I consider what collaborators might do if they want to inscribe their own risky accounts, but this discussion ultimately leads me to suggest that while I do not believe we can accurately render collaborative writing *in practice*, that is, represent the complex rhetorical assemblages of coauthorship in terms that reduce those assemblages to step-by-step processes, I do think we can depict, and see depicted, some of the traces (to borrow a term from Latour) of the ideas and impressions left in the wake of coauthorship as they are processed and articulated by the very people who experience them.

In chapter 5, I return to the topic of authorial economies and suggest that questions about how to qualify coauthorship are ultimately questions about how to coauthor. In an attempt to trouble the major schemes with which academics are asked to account for the labor of collaboration, I turn to a discussion of postqualitative inquiry, which is an increasingly popular approach to research in the social sciences, especially in educational research and psychology. Postqualitative theorists

draw on an array of philosophical influences, perhaps none more heavily than the poststructuralist and psychoanalytical work of Gilles Deleuze and Félix Guattari. The primary project of postqualitative inquiry is to question the representationalist assumptions—including the strict boundaries and normative structures—of what its practitioners term traditional “humanist qualitative methodology.” One of these assumptions is that a set of data speaks for itself and hence it is the qualitative researcher’s job to make sure such data get rendered as objectively as possible. In response, postqualitative thinkers have embraced writing as a mode of inquiry, the methodological interest in which has led to a robust postqualitative conversation about collaborative writing and the politics of authorship in qualitative research. Building from this work, I suggest the possibility of what I call, adapting concepts from Deleuze and Guattari, a *minor literature of collaboration* as an active response to the “major languages” of authorship that circulate in most academic disciplines.

Finally, John Pell and I enact our own version of a minor literature of collaboration in chapter 6 to evidence some of the debts and influences that have impacted how we’ve developed as collaborative writers and how the ideas presented in this book (which I’ve written “alone”) and across some of our other collaborative work together have influenced how we understand the work of writing in general, not just coauthorship.

Before proceeding, however, I’d like to point out two things about the organization and scope of this book. As you can see from the table of contents, the six chapters that compose the body of the book are organized into two parts, which I’ve respectively labeled “Speculations” and “Enactments.” Even though the chapters in part 1 are more theory oriented and the chapters in part 2 are more practice oriented, I eschew such theory-practice dichotomies because like Thomas Kent (1997) I’ve come to recognize theory itself as a kind of practice, one that finds its virtue in allowing us to continually critique and reconstruct our beliefs and assumptions about phenomena that matter to us as we experience them (158). In part 2, the practices I discuss, and the ones John and I enact, are speculative and thus meant to suggest potential consequences for the ideas developed in part 1, but I hope my articulation of these possibilities remains elusive enough that readers are encouraged to develop their own ideas and observations about the potential uptake for the theory proposed in these pages.

And this gets to my second point. While this project doesn’t pursue the kinds of empirical or qualitative research other scholars of collaboration have undertaken, my aim with this book is not to cast aside

the research on collaborative writing that practitioners in rhetoric and composition have produced over the last three decades. It is instead to engage a specific line of inquiry that outlines a different set of theoretical starting points for understanding what happens when writers compose together, ones I hope might reinvigorate how we advocate for the importance of collaborative writing in the work we do as teachers, scholars, and administrators. With that said, the field of writing studies has no shortage of experienced collaborators who are passionate about their work and more than capable of accounting for it. Thus I hope this book can serve as an invitation to renew these conversations as we continue to develop as teachers and writers who promote the benefits of writing together.