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What Does It Mean to Be an Author? The Intersection of Credit, Contribution, and Collaboration in Science

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In this article, I draw on interview data gathered in the High Energy Physics (HEP) community to address recent problems stemming from collaborative research activity that stretches the boundaries of the traditional scientific authorship model. While authorship historically has been attributed to individuals and small groups, thereby making it relatively easy to tell who made major contributions to the work, recent collaborations have involved hundreds or thousands of individuals. Printing all of these names in the author list on articles can mean difficulties in discerning the nature or extent of individual contributions, which has significant implications for hiring and promotion procedures. This also can make collaborative research less attractive to scientists at the outset of a project. I discuss the issues that physicists are considering as they grapple with what it means to be "an author," in addition to suggesting that future work in this area draw on the emerging economics literature on "mechanism design" in considering how credit can be attributed in ways that both ensure proper attribution and induce scientists to put forth their best effort.

Introduction

The attribution of credit via authorship on collaborative research projects has been the subject of much recent discussion in the sciences. In particular, it has been argued that the traditional system of authorship breaks down in significant ways when there are multiple authors involved (e.g., Biagioli, 2003; Kennedy, 2003; King, 2000; Paneth, Hemenway, Fortney, & Jung, 1998). Indeed, it can be difficult for outsiders such as hiring or promotion committees to discern the nature and extent of individual contributions to the work or to know whom to hold liable in the event of errors or other controversy (Rennie, Yank, & Emanuel, 1997). This becomes even more difficult in certain areas of research where projects have become extraordinarily large and standard practice involves what Cronin (2005) refers to as "hyperauthorship," or extremely long author lists that give approximately equal formalized credit to all persons involved with a research endeavor. In some ways, this is an instance of a broader class of problems with credit attribution for collective works such as films or theater productions (Becker, 1982).

Moreover, there is an interesting contrast that emerges in a closer examination of existing discussions of authorship. Cronin (2005), for example, pointed to differences between the biomedical and high energy physics (HEP) research communities. Biomedical researchers publicly view lengthy author lists as a problem that must be addressed via ethics policies (Claxton, 2005), discussion (Kennedy, 2003), and novel proposals for new systems of recognition (Paneth et al., 1998). In HEP, on the other hand, public discussion in journals has generally been minimal, and long author lists have been presented (e.g., by Cronin, 2001) as acceptable and generally in harmony with portrayals of this community as one that is collectivist, trusting nature and rich with structures for internal review and scrutiny (Kling & McKim, 2000; Knorr Cetina, 1999; Traweek, 1988).

These explanations for the observed differences between fields, however, are not entirely satisfactory. While they do help to explain why there do not seem to be significant concerns about liability and potential fraud due to ambiguous authorship in HEP, they do not address the issue of how individuals are evaluated in HEP. Indeed, hyperauthorship has two sides. On one hand, as Cronin (2001) noted, it is difficult to assess individual contributions to a single article. At the same time, though, it will be shown later in this article that standardized author lists in HEP mean that any given individual is likely to appear as an author on hundreds or even thousands of publications. Traditional means of individual evaluation via curricula vitae (CV) break down under these conditions. Yet, despite collectivist tendencies in the field, hiring and promotion occur at the individual level. Thus, there must be some means for assessing the nature of individual contributions to "hyperauthored" work.

This article seeks to better understand this apparent disconnect and its implications through a careful examination

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of authorship and attribution practices in the HEP community. Where prior studies of authorship (e.g., Cronin, Shaw, & La Barre, 2003; Price, 1986) have used primarily bibliometric data to understand trends more broadly, the present study focuses on a specific community and draws on rich interview data to gain a clearer understanding of how authorship is actually used and what it means to individuals. Specifically, the case will be made that despite prior portrayals to the contrary, there is significant tension in HEP between individual contribution and collective recognition, particularly with regard to contributions that are "infrastructural" in nature. The article will begin with a discussion of collaboration and the functions of authorship, followed by a look at authorship practices in HEP. It concludes with a discussion of these practices and their implications.

Collaboration and Authorship

This discussion of collaboration and authorship is particularly timely for several reasons. First, collaboration has proven essential to answering scientific questions of significant interest (Hara, Solomon, Kim, & Sonnenwald, 2003). Second, large experimental apparatus are increasingly common in research activities (Galison & Hevly, 1992), as are cyberinfrastructure and e-science projects that promote the development of sharable computing, communications, data, and experimental infrastructures too large to be funded or operated by a single researcher or institution (Atkins et al., 2003; Finholt, 2003; Kling & McKim, 2000; Nentwich, 2003). Third and finally, all of this activity has sparked recent calls for systematic social science analysis of the conditions, context, and functioning of research collaboration activities (Cummings & Kiesler, 2005; Mervis, 2005).

Moreover, there is some reason to believe that issues surrounding authorship and the attribution of credit can influence researchers' decisions about whether to engage in collaborative work at all ("Who'd want to work in a team?" 2003). As academic researchers are rewarded in a system largely focused on individual reputation (Whitley, 2000), collaboration can be risky if it is not clear at the start how credit will be awarded for contributions to a project. Plus, there are additional risks that stem from dependence on colleagues completing their work in a satisfactory and timely manner.

What Does Authorship Do?

As indicated earlier, authorship has multiple functions in the sciences. Put somewhat more formally, we can describe these as follows: (a) attributing credit for discoveries to a person or group of people, (b) assigning ownership to this person or persons, and (c) enabling the accrual of reputation.

Attribution of Credit

Authorship attributes credit for particular discoveries to individuals or groups of individuals. Foucault (1977) stated that authors' contributions to a literature enable further contributions and therefore enable the attribution of credit to others as well. Authorship is generally viewed as the accepted method for recognizing the contributions of researchers to their field of interest. Having one's name appear on a conference presentation or a journal article is typically intended to signal some form of significant contribution toward that discovery (Claxton, 2005); however, as Cronin (1995) noted, there are many types of contributions to scientific effort, and authorship is not the only way to recognize these. In many of the cases he looked at, relatively minor contributions to intellectual work were credited with formal acknowledgments in published articles, though exact practices and tradition differ somewhat by field.

Historically, authors were individuals, and it was relatively easy to use authorship to gauge the value and extent of an individual's contributions to the literature (Shapin, 1995); however, the recent rise of coauthorship in some fields has made this substantially more difficult, as having multiple authors can render individual contributions ambiguous (Rennie et al., 1997). Moreover, this ambiguity is further confounded in the case of contributions by paid technicians or consultants. For example, staff laboratory technicians have historically not been included as authors on articles (Shapin, 1989). Statisticians, on the other hand, may be considered authors in some fields if they have contributed substantially to multiple phases of the research project (Parker & Berman, 1998). Thus, there is an important, though blurry, distinction between those who deserve formal recognition as legitimate contributors to research and those who do not. This becomes particularly interesting in the case of the HEP experiments described later in this article. Each of these experiments involves contributions by nearly 2,000 individual physicists, all of whom will be listed alphabetically as authors on each article published by any member of the experiment. The rationale for this is a pervasive acknowledgment that no research could be done without the contributions of all of these individuals, coupled with a strong desire to recognize this fact and motivate everybody to contribute (Galison, 1997). At the same time, however, specific analyses are not carried out by thousands of people—they are done by small groups. Similarly, articles are written by small groups as well. Thus, we can see that where author lists are long, there appears to be a fundamental disconnect in the attribution of credit that is not accounted for in the present system of scientific authorship. This will be explored in more detail later in light of data presented next.

Ownership

There are two senses of ownership that pertain to any discussion of authorship. Outside of the sciences, ownership is frequently considered with an eye toward copyright (e.g., Rose, 1993; Woodmansee & Jaszi, 1994); however, this aspect of ownership tends to be far less important in the sciences, where journal authors typically sign copyright away to publishers at no cost in exchange for the reputational and

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career benefits that will accrue from the broad circulation of their work.[1] On the other hand, the sense of the word "ownership" that is critical to the present discussion but rarely mentioned in the copyright debates is that of taking responsibility for one's work, including error or controversy that might lie within it. As has been argued repeatedly in the literature, this becomes problematic on collaborative projects (Kennedy, 2003; Paneth et al., 1998; Rennie et al., 1997). When there is more than one author, it is unclear exactly where liability rests. This was quite apparent, for example, in the late-20th-century controversy surrounding Nobel Prize Winner David Baltimore and his colleagues (Kevles, 1998). In this case, one of Baltimore's coauthors was accused of data fabrication, but Baltimore refused to withdraw the article. The accusations were aggressively pursued, eventually by the U.S. Congress, and Baltimore was forced to resign from the presidency of Rockefeller University even though an expert panel later cleared his colleagues of the misconduct charges. The important point here is that ownership of the claims in the article was ambiguous due to the presence of multiple authors.

Reputation

Third, science operates on what has been referred to as an economy of reputation (Whitley, 2000). From the time academic researchers carve out a niche in which to carry out independent work as graduate students, they are expected to cultivate a reputation as the world's expert in a particular area. Reputation is, in this sense, analogous to Bourdieu's (1984) notion of "symbolic capital" in that status in science is not determined by possession of economic capital (i.e., how much money one has) but rather by reputation based on symbolic capital. Historically, this has been accomplished by publishing articles in high-visibility venues (Franck, 1999) and by winning high-profile awards, of which the Nobel Prize is a particularly well-known (but rare) example. Moreover, Merton's (1968) Matthew Effect suggests that it is important for young researchers to carry out single-author work to avoid having their reputational credit subsumed by a higher profile name on the author list.

These publications and other accomplishments are carefully scrutinized by hiring committees, promotion and tenure committees, and others looking to assess individual accomplishment. Also note that even as collaborative work has become more common in recent years, many fields continue to place a premium value on single-authored or first-authored publications. Indeed, Birnholtz (2005) observed evidence of junior researchers in the field of neuroscience who maintained two independent research programs. One of these was typically more complex, involved human specimens, and required multiple collaborators. The other was individually conducted and geared toward the single-author publications that are so valuable for reputation purposes.

Given the value of reputation and the ambiguity of specific contributions when there are multiple authors, it is perhaps not surprising that others have observed cases where researchers listed as "authors" on articles did not make significant contributions to the work. For example, both Tarnow (1999) and Claxton (2005) discussed instances of "gift" authorships to maintain social ties or to acknowledge senior researchers who provided laboratory space or financial support.

Research Context

Data described and analyzed here were collected in the HEP community. Physicists, who have historically been at the forefront of conducting large-scale, collaborative research, have been a favorite subject of science studies researchers (e.g., Galison, 1997; Knorr Cetina, 1999; Shrum, Chompalov, & Genuth, 2001; Traweek, 1988). Experimental investigations in HEP utilize high-energy accelerators that recreate conditions at the start of the universe. By recreating these conditions, physicists are able to generate specific particles of interest that do not occur naturally under current, more stable atmospheric conditions. Large detectors are used to track the behavior and existence of these particles by recording the energy "trails" left behind. Today's accelerators and detectors dwarf all other scientific instruments. The Large Hadron Collider (LHC) at CERN, the world's frontier laboratory, is an underground tunnel 27 km in circumference. A toroidal LHC apparatus (ATLAS) detector, one of two that will sit in the LHC when it is complete in 2007, will be 20 m in diameter and weigh 7,000 tons (Close, Marten, & Sutton, 2002). The human and organizational scale of this work is similarly large. The ATLAS experiment, for example, involves over 1,800 physicists based at 140 institutes in 34 countries around the world. As has been the tradition in HEP experiments for many years (Galison, 1997), the present generation of experiments plans to list all participating physicists as authors on articles published by any member of the collaborations, thereby extending the bounds of "hyperauthorship" further than ever before.

At the same time, though, HEP collaborations always have been large (Galison, 1997; Knorr Cetina, 1999). Are issues of ambiguity surrounding individual contributions really that novel in a field where the previous generation of experiments had 200 to 600 authors on each article (see Figure 1)? The data gathered here suggest that these issues *are* different in the context of the present experiments. First, interview participants indicated that these are the first experiments on which they have worked where they do not recognize or even know the names of the majority of their collaborators. This has critical implications for the informal systems of recognition that have evolved in HEP that will be discussed next.

Second, the global nature of the work means that support for the research is no longer funneled through a single source

[1] In this regard, journal articles stand in contrast to books, and textbooks in particular, that are frequently written to generate royalties.

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Run 1 Paper Title

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FIG. 1. Example of the standard author list for a paper from a typical HEP experiment.

(or small number of sources), as was the case historically, for example, with Department of Energy funding for the Fermilab facility in Illinois. Rather, CERN is a global facility in unprecedented ways, since it is no longer the case that individual countries (or economic/political blocs) have their own cutting-edge accelerator facilities. Those institutes that wish to be involved with this work must align themselves with one of the LHC experiments. Moreover, in the CERN experiments, only funding from the 20 CERN European member states is actually routed through or controlled by CERN and experiment leadership. The rest of the funding is controlled by participating institutes outside of Europe, who voluntarily use resources from their home countries to build components for and provide services to the LHC in accordance with the experiment's Memorandum of Understanding. Effectively, because any institute is essentially free to withdraw its voluntary contribution to ATLAS at any time,[2] this means that the elected leaders have no real power beyond gentle persuasion and what one project team leader describes as "managing by coffee." In other words, leadership becomes an exercise in continuous consensus building through informal meetings (usually held over coffee in one of the ubiquitous cafes at CERN), formal presentations, and peer review panels.

For the present discussion of authorship, the important implication of this arrangement is that there are multiple types of contributions to research efforts for which people may wish to receive credit. Some are intellectual, as has traditionally been the case, and others are financial or technical

[2] Withdrawal would, of course, carry substantial intangible costs to the institute, but it is not unprecedented.

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in nature. Biagioli (2003) referred to this as a "labor" mentality of contribution, as contrasted with an "originality" mentality found in, for example, biomedical journals.

Method

Qualitative methods were used to gather data for this study, which was conducted as part of a broader investigation of scientists' collaboration behavior. Data were collected during a 9-week visit to CERN from June 8 to August 10, 2004. Semistructured 30- to 60-minute interviews were conducted with 32 individuals affiliated in various capacities with ATLAS and Compact Muon Solenoid (CMS), the two major LHC experiments. Interviews were recorded and later transcribed by the author. Participants were selected using snowball sampling techniques, and deliberate efforts were made to speak with individuals at multiple levels of the experiment hierarchy, from 1st-year graduate students to members of the experiment leadership teams. A uniform protocol was used to conduct interviews, but the order and selection of items were periodically changed to accommodate conversational flow and respondent experience.

Analysis

Inductive qualitative techniques were used to analyze the data (Huberman & Miles, 1994). Data analysis consisted of careful reading and rereading of interview notes, field notes, and examination of photographs and other artifacts collected while at CERN. During this process, it became clear that authorship in HEP was a complicated and nuanced topic on which there were a range of opinions among the physicists interviewed. Most discussion, though, centered around three themes: (a) balancing the attribution of credit to a large group with the need of individuals to attain recognition and advance their careers, (b) whether there is a significant difference (that merits recognition) between what will later in the article be called infrastructural and discovery-oriented contributions to research endeavors, and (c) pragmatic strategies for "survival" in HEP given the nature of authorship. These themes guided reexamination and further analysis of the data, and provide the basic framework for the presentation of results.

Results

This section begins with a detailed description of how authorship works in HEP, followed by a more analytical treatment of its implications.

How Does Authorship Work in HEP?

As suggested earlier, HEP has a longstanding tradition of extremely inclusive author lists. From the recent past and into the present, this has meant alphabetically listing all members of a collaboration as authors on any article written by any member of that collaboration (provided, of course, that the article is based on data from that particular project). Given that every member is an author, there also are many opportunities for all members of the collaboration to provide feedback to the "main contributor" authors during the writing and revision process. The remainder of this section provides more detail on this process.

Becoming an author. Knorr-Cetina (1999) suggested that among physicists, the collectivist orientation of HEP experiments means that the individual is largely erased as "an epistemic subject." It was therefore surprising to find the composition of the author list to be a highly contentious topic among the physicists interviewed. To be included in the author list from a bureaucratic standpoint, a series of signatures on forms were necessary. Interviewed participants mentioned that these bureaucratic regulations stem from some amount of required "service work" to the collaboration to prevent people from "bypassing" the hard work of the design and construction phases of the experiment and joining just in time to participate in the more glamorous physics analysis tasks.

In some cases, however, participants said that the true threshold for inclusion is service contributions from anybody at one's home institute—and not just the individuals themselves. Many senior researchers, for example, noted that they were spending the final stage of their careers working on ATLAS so that the junior faculty and graduate students at their home institutes who are not currently working on ATLAS could become involved when the detector comes online in 2007 and the experiment has a data stream to generate publications. This is an important difference from prior experiments such as the Collider Detector Fermilab (CDF) experiment observed by Biagioli (2003), in which author-list eligibility was based on individual contribution. This difference is likely due to the fact that unlike smaller scale projects in the past, the 15- to 20-year time horizon of the current generation of experiments is far longer than the tenure "clock" at most U.S. universities. Because physics publications must be based on "real" physics data, as opposed to the Monte Carlo simulations that comprised the bulk of analysis work in the early phases of the experiment, junior faculty members with tenure aspirations cannot dedicate substantial efforts to LHC experiments, which have been in formal development phases since 1995, until real data are being generated in 2007.

Despite these measures for restricting who gets credit, the author lists are nonetheless extremely long, and some question whether certain colleagues "really belong" on the author list:

> People have views that vary all over the field. So an engineer who did some work on a special part of the apparatus, should he be in the author list? Or even a physicist who's in a group, but never even set foot in the experiment. Should his name be there? (CERN05).

This example alludes to the fact that researchers on these projects come from and work primarily in vastly different

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institutional contexts that have an important impact on their ability to contribute to the project. Teaching loads and requirements, travel funding, and levels of graduate student and research scientist/postdoc support along with many other issues come to bear in determining the amount of time individuals have to devote to the project. These contextual factors are blurred significantly on the projects, however, and this can affect "public" perception of actual effort contributed. In other words, it is nearly impossible to tell during, for example, a meeting at CERN who has a significant teaching load and is unable to contribute to the collaboration during a particular term. Nonetheless, researchers are judged by their contributions to the experiments and their ability to "get noticed," as described later.

Publishing a coauthored article. While the exact procedures for writing, soliciting feedback, and publication vary somewhat from experiment to experiment, participants generally described processes that involve the following steps:

1. The main contributors carry out specific analyses and write these up in manuscript form, possibly also presenting the work at meetings internal to the collaboration.
2. A draft of the manuscript is circulated via e-mail to all members of the collaboration for comments and feedback.
3. The manuscript is submitted for approval (Some groups refer to this as having the results "blessed.") by the publication committee within the collaboration.
4. Once approved, the manuscript can be formally submitted for conference or journal publication and released outside the collaboration.

Note that the physicists interviewed take these procedures very seriously. Several indicated that the premature release of results that have not been "blessed" or approved could constitute grounds for ejection from the collaboration. In addition, the head of the secretariat for one of the large LHC experiments indicated that her office is responsible for the submission of all publications from the collaboration. In other words, individuals affiliated with this experiment are not allowed to submit their "own" work for publication, even when the results have been approved.

What Does Hyperauthorship Mean for HEP?

So far, it has been illustrated that authorship, and especially inclusion in the author list on articles, is a significant issue in the HEP community. This makes some intuitive sense in that the author list is the formal record of responsibility for a discovery. To further explore authorship in HEP, though, consider the three functions of authorship described earlier. In terms of credit attribution, the author list is the formal means by which credit for discoveries is attributed. As noted earlier, it is considered quite important in the community that all contributors to a research project receive formal credit for their efforts in this way. Many believe it is not fair to place a premium value on the analysis tasks that lead directly to high-profile discoveries. As one interviewed participant said:

> Every piece which is there has somebody who has thought about, has given a year of his life to make sure that a bolt is in the right place and has the right effect. Not that guy at the end [doing the analysis] who does not know that the bolt is absorbing part of the noise. . . . So I think that it is important that everybody who has worked there, even left or even died, every year people die on these collaborations, it is very bad if this memory is gone. . . . I like the idea of authorship extended (CERN24).

At the same time, all contributions are not equal. There is a clear tension between a desire to recognize all contributions to a large collaborative project, with a desire to give special credit to those who put forth particularly valuable effort:

> In a lot of ways it sort of doesn't work. You put everyone on, it sort of demoralizes some people. If there's a real creative person, you want to somehow let him get the rewards for being creative, and that's difficult because one person can do something creative but he's using the data and the work of a few thousand others (CERN03).

Thus, the individual role ambiguity inherent in multiple authorship leaves open the questions of ownership and reputation.

With regard to ownership, the data gathered here have less to say. The LHC experiments have not yet published physics results and are not expected to do so until 2008 at the soonest. Thus, there have been few opportunities for controversy on these projects in which formal notions of ownership emerge as important issues. Some participants did, though, mention instances in the past where they were hesitant to take ownership of particular results. One participant, who was atypically diligent among those interviewed, indicated that he had read all but two of the 250 articles on which he is listed as an author. He described making a point of reading every article, and removing his name when he does not feel he fully understands or agrees with the results:

> There are papers where you say to yourself 'Do I really want to be associated with this? Maybe I don't.' One in particular was high profile and I think it was wrong. And the real reason I took my name off it is I was here [at CERN] when the paper came out, and I said, you know, if somebody calls me and says 'Gee, this is interesting. You're an author. Why don't you come give a seminar on this?' I didn't feel like I could defend what was on there at least as well as the proponents could. So I said 'No, I don't really want to sign my name to that.' (CERN20)

The interesting aspect of this example is the participant's unusual regard for notions of ownership and ability to defend

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the claims in what others might consider to be his work. This is similar to Rennie et al.'s (1997) notion of guarantorship, and will resurface later in the discussion of alternative proposals for authorship.

Moreover, formal fraud and misconduct did not come up in interview discussions of credit attribution and authorship. In part, this is likely because HEP has extensive structures for internal review, such as the "blessing" of results and extensive circulation of preprints. In this vein, Kling and McKim (2000) argued that knowledge certification occurs earlier in physics than it does in other fields.

At the same time, however, participants were quite cognizant of the possibility that other individuals might seek to take ownership for the entire collective endeavor if it is successful. Many mentioned the story of Carlo Rubbia as a cautionary tale. As Taubes (1986) noted in a journalistic account, Rubbia was the controversial winner of the 1984 Nobel Prize in Physics for his leadership on the H2 experiment at CERN that also involved substantial effort by approximately 200 other collaborators.

Physicists interviewed in the present study, particularly those at early stages in their careers, were significantly concerned about how to get adequate credit for their efforts and establish their reputations in HEP. Many said that even though the LHC projects are very large, "the Nobel Prize won't be given to 1,500 physicists." In other words, individual reputation remains the coin of the realm. At the same time, though, there is widespread recognition of the fact that nobody gains anything without the efforts of all their collaborators. Thus, respondents indicated a strong need to remain alert and competitive both as individuals in need of a strong reputation within their collaboration and as a collaborative group in fierce head-to-head competition with other experiments to be the first to make specific discoveries. As shall be illustrated later, most participants indicated that ambiguity renders the formal record of contribution meaningless in hiring, promotion, and evaluation decisions. Instead, they described a system of informal recognition that relies heavily on word-of-mouth recommendations and individuals' ability to "get noticed" within the collaboration. The remainder of this section will focus on these issues of reputation.

Importance of reputation. Participants indicated that reputation has been particularly important in recent years due to a scarcity of jobs resulting from declining funding levels for physics research and ever-increasing experiment costs (Seife, 2005). It is not uncommon for a junior researcher in physics to hold two or three postdoctoral positions before moving into a faculty position, if they are able to secure a faculty position at all. One physicist described this as follows:

> I didn't have great choices. You know, you look around where you get a job. . . . ATLAS is finally a post where I have permanent contract, but before I was on three different postdoc positions, if you like. And then when that runs out, you had to see what next. So you're not free to say 'Now I want to go and work there' because you have to find some payment for what you want to do (CERN14).

One key reason for the importance of reputation is that in evaluating individuals, the "other" side of hyperauthorship becomes quite prominent in HEP. In other words, any individual may be listed as an author on hundreds of publications, and it is difficult to tell what specific contributions he or she is responsible for. To illustrate this point, the names of 20 randomly selected participants from this study were queried in the SPIRES physics publication database (http://slac.stanford.edu/spires). The median number of publications on which each was listed as an author is 105.5 (*SD* = 185.3), with a wide range from 0 (for a 1st-year graduate student) to 603. Though most were clustered around 100 publications, 6 individuals were listed on 200 or more. It therefore is not surprising that most participants reported that they appear as an author on publications they have not read. One even indicated that he is an author on several publications written in Russian, a language he cannot speak or read. The important point here is that when there is no expectation that for example, a job applicant has even read all of the publications listed on his or her curriculum vita, assessment by traditional means is a challenge.

Getting noticed. In the face of ambiguity on both sides of "hyperauthored" articles, from both the long list of names on any given publication and the long list of publications associated with any given name, the HEP community largely has turned away from formal records of contribution and taken to using informal means of assessment and evaluation. One participant, a senior researcher, described the experience of a talented postdoc:

> One of the postdocs has made quite a lot of progress in [a technical area of a large experiment]. He did pretty much all the work by himself along with one of the associate scientists, who actually happens to work for me so I know a bit about this. He gets credit, I guess, because he gets to give the seminars about that, but any publications will be strictly alphabetical. Is that fair? Probably not. But how else do you do it? (CERN04).

The important part of this example is the admission that actual credit for the research discovery does not seem to come from the publications but rather from the informal seminars and talks the postdoc will give within the collaboration. This is just one of many informal means of "getting noticed" that participants discussed.

Students and junior researchers must ensure that they establish a solid reputation within their workgroups. It was surprising, however, how many graduate students at CERN reported having minimal contact with their advisors at their home institute. Instead, many reported close involvement with a CERN workgroup or even a workgroup based primarily at another institute. The influential reference letters

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for these students will come from these workgroup colleagues and supervisors and not necessarily from their advisors. This serves to increase the imperative for researchers to distinguish themselves via individual reputation, which does not sit well with everybody in the community:

> I think that's one of the problems that younger people face. At certain points it becomes quite political somehow. It is very difficult to get credit and I find that most of it works by, it's also a very sociological thing, you have to give presentations, you have to show up yourself, so I think as much it's the quality of your work as it is the publicity that you do, which honestly I don't like as much. . . . So, very honestly, I'm thinking about whether I should stay in ATLAS, and whether I should stay in the field or not. Because although I find the physics they're trying to do very, very interesting, and very, very challenging, it's tough, you know, and I don't like to work with all these many people (CERN16).

Additionally, there is a sort of Catch 22 in effect regarding getting noticed. As shall be described later, one excellent way to "get noticed" within the collaboration is to secure a high-profile task such as representing the collaboration by giving a talk at a conference or doing a particularly glamorous bit of data analysis. At the same time, though, these tasks are generally assigned by the central leaders of the experiment, so it is difficult to be assigned to one without having been noticed already. Thus, the second important component of "getting noticed" is being noticed not only by one's colleagues but also by those in positions of authority who can assign high-profile tasks and will be aware of job openings, conference presentations, and other opportunities that may become available. This potential for broad exposure was described by a participant as a significant advantage to graduate students working on a very large collaboration like ATLAS:

> I actually think these collaborations have for young people a particular advantage in that they can situate themselves in a real international context, where there, well, where cleverness and such values can get through. Whereas, for example, if you just have a little experiment only at your university, you are completely locked in perhaps to the hierarchy of your small group of 5 people or something like that. There is maybe much less room, really to show yourself off, or it's only your supervisor or your professor who has an opinion about whether you're good or bad (CERN19).

While it may be true that large collaborations provide stars with the opportunity to shine more broadly as this participant suggested, it also is true that this places a much more significant burden on junior researchers, who need to ensure that their efforts are broadly recognized. As will be illustrated later, this can prove problematic.

How to get noticed without really trying. Interview participants described several means of getting noticed and distinguishing themselves within a large collaboration. First, it has been noted elsewhere (e.g., Traweek, 1988) that physicists prize a willingness to work hard to achieve high-quality results. Being known as somebody who is dependable, diligent, responsible, and willing to work long hours are all likely to yield positive letters of recommendation from immediate supervisors and colleagues, though this is not always true.

In addition, some physicists described a need to be known as somebody who can come up with novel solutions to difficult problems. It is interesting and important to note that these problems need not be discovery-oriented. To be sure, there is significant value in solving, for example, an analysis problem that leads to a major discovery. At the same time, though, many participants indicated that a novel solution to a difficult detector design or construction problem also could carry significant weight.

Moreover, many participants indicated that talks and presentations are another way to achieve visibility. These can happen both internally as part of "collaboration weeks" where hundreds of collaborators convene at CERN and externally when the collaboration is presenting work at a field-wide HEP conference. Both of these are important in getting noticed. While Knorr-Cetina (1999) described the assignment of these presentations as a collectively oriented activity that considered which students or junior researchers "needed" visibility at the time, participants here described a somewhat more individually focused process. While it was certainly true that they described some effort to be "fair," it also was widely acknowledged that individuals must be vigilant about getting credit for their contributions to the project. For example, the spokesman of one of the LHC experiments indicated that more than 100 talks per year are given at conferences by members of the collaboration, and he tries very hard to be sure that "they are given to people who really deserve it, are competent (CERN19)." Another participant described her experience in trying to give talks and be otherwise visible:

> So I think that's something that if you are not careful you go bad. You know, you have to take care on your own that all of the credit is given to you. It's difficult . . . but it's definitely based on personal effort (CERN09).

The final class of methods that participants described for achieving visibility or "getting noticed" involves providing exemplary or exceptional service to the collaboration, generally by taking some sort of leadership role in the overall collaboration or one of its components. These roles are important to mention in that they can be nontrivial to secure in two ways. First, many of the high-level leadership positions are elected and therefore require some prior exposure. But even leadership positions within subgroups are difficult in that they require substantial presence at CERN. Thus, one must be affiliated with CERN or with an institute that can support frequent travel to CERN and also cope with long absences from the home institute. Thus, several informants reported needing to secure permission (which is not uncommonly denied) to take on additional responsibilities within

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the collaboration. In one particularly interesting case, a participant indicated that in the ATLAS hierarchy, he is a superior to his lab director to whom he reports at his home institute. When asked how he balances this, he noted that:

> Basically it depends on the subjects we're discussing , you know, on who takes the lead. If it's an ATLAS point, then it's me who says 'Well, we need to do this, I need to do that.' If it's a [home institute] point, it is usually one of my colleagues. Now I, of course, have to have permission to have this job from my lab director at [my home institute], so he could have said that he didn't want me to stand for re-election this period. He could have said 'I want you to go back to [home institute] and do something else.' If he had said that, then I have little to discuss with him. But he would have been fully in his right (CERN11).

The interesting point here is that the hybrid system of seniority can confuse political relations and influence people's ability to get noticed within a collaboration. This is just one of many situations where efforts to get noticed can break down.

When getting noticed breaks down. Having discussed the importance of "getting noticed" and distinguishing oneself within a large HEP collaboration, note that it also is quite easy to get lost or even "crushed" in the crowd. Breakdowns in informal systems of recognition, of course, are not a novel result on their own. What distinguishes the present discussion is the almost complete absence of a formal record to fall back on.

First, there are situations in which individuals work diligently and provide exemplary service to the collaboration, but these efforts are, for some reason or other, not noticed or properly credited by their supervisor. For example, a participant described her situation as follows:

> It's been a very frustrating experience because I do know that I have been one of the few who has performed exceptionally well. We have done it on time and whenever there was a problem I was able to re-arrange, re-steer and adjust the problem and all that. . . . Despite that, upper management insists on assigning someone else the responsibility for being in charge officially in the [org chart]. . . . He was never in the lab. He doesn't know what we are doing. The only time he came to the lab was to borrow screwdrivers which he did not return. So it's been very, very frustrating, and he gets the credit officially for the work, so this has been very tough (CERN21).

The apparent problem here, as she describes it, is that her supervisor wishes to take credit for her efforts, and she describes herself as having few options for official recourse. Several participants, particularly women,[3] felt that the informal system of recommendation and credit attribution can be particularly difficult when one is faced with a supervisor that does not take one's contributions seriously. For example, a participant described her take on her level of influence on the overall experiment as follows:

> I am represented by somebody, you know? I am in a group which is 80 people and we have a project leader that represents all of us. So I have no idea what he takes of my opinion when he goes to the big board and makes decisions. So I don't think there I have any influence at all. Really, definitely. I mean, we are like little ants. That is what we are (CERN09).

Here, we see again that supervisors and leaders in HEP collaborations wield power over their subordinates that is, in some ways, different from that found in other fields in that there is no effective and formal record of authorship that at least denotes some level of contribution to a research effort. Rather, because credit and contribution are tracked informally, individuals must be particularly vigilant and are, at some level, at the mercy of their supervisor.

In another example, one junior researcher who held a CERN fellowship described his own experience as a mixture of institutional politics and the nature of the positions that he held:

> In [the CDF experiment at Fermilab], I was at Chicago, that was the most powerful institution and so they gave me a lot of responsibility. Here at CERN it's also I guess a bit with tradition. And also the fellows, since you're only here two years they know you're only here two years. And they give you less responsibility because they know they cannot rely on you because you're going to leave (CERN16).

In each of these situations, we see that the informal system of attribution in HEP relies heavily on the good faith of supervisors and leaders, who are themselves trying to "get noticed" in many cases.

Discussion

In this section, the case will be made that lessons derived by studying HEP are more broadly applicable. Next, these lessons will be explained along with an attempt to lay groundwork for moving the discussion of authorship forward.

Is This Really That Different From Everything Else?

HEP research is so much larger in scale and scope than is research in most disciplines that an important question is whether useful lessons for other disciplines can be derived from the careful study of HEP. In other words, one wonders if physicists are somehow different from other scientists or whether HEP is somehow different from other fields. If the former is the case, then there are few lessons that can be drawn from an examination of authorship in HEP that will be applicable to other disciplines.

[3]Note that women remain a small minority in HEP, comprising around 10% of the field.

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If the latter is the case, on the other hand, there is arguably a great deal to be learned from a careful examination of HEP, both because physicists themselves need to survive within the existing system of scientific credit and rewards and because large collaborations in other fields are occurring with increasing frequency. Physics presents an extreme case that is useful for consideration of how we might cope more broadly with the collision of collaboration and authorship. This interpretation is supported by a comparison with astronomy, another field that is characterized by large, shared apparatus.

HEP Versus Astronomy

Astronomers, like physicists, have a long history of sharing instruments (namely telescopes) that are too large, scarce, and expensive to be possessed by individual researchers or facilities (McCray, 2000); however, astronomers have not engaged in the practice of hyperauthorship that we see in HEP. The two primary reasons for this appear to be that (a) astronomers' telescopes are more general-purpose tools than are particle accelerators and detectors, and (b) astronomers seem to be less involved in telescope construction than are their counterparts in HEP.

First, detectors in HEP are typically built with a specific physics result in view, such as the existence of the Higgs Boson and supersymmetry in the LHC case. Virtually everybody involved with the LHC collaborations is, at some level, interested in this discovery and the related physics, and contributes to the design and construction of a detector that will (hopefully) enable this result. A shared detector implies shared research interests. In astronomy, on the other hand, this is not true. Telescopes can be used for a range of purposes and discoveries, so there is no implicit need to credit all users of a telescope when one particular user has a novel finding.

Second, astronomers themselves appear to be generally less involved in the construction and operation of their shared instruments, which is done by paid technicians (who are, for better or worse, not generally included in author lists—an interesting issue that is beyond the scope of this article, but discussed by Shapin, 1989). Even where shared data or instruments are used by researchers, there is a strong tradition of using formal acknowledgments to credit colleagues rather than extended author lists, as Cronin (1995) illustrated with the example of Verner's (1993) "Astronomy Acknowledgement Index."

Physicists, on the other hand, are quite actively involved in the design and construction of their detectors. Professional engineers and technical staff do carry out some of the work, as discussed by Galison (1997), but design and many assembly tasks are carried out almost exclusively by physicists. The University of Michigan's contribution to the ATLAS experiment, for example, was the construction of several thousand "Monitored Drift Tube Chambers" (MDTs). MDTs are each several feet long and consist of a hollow aluminum tube that contains a specialized gas formulation and a wire that must be hand placed within a few microns of precision. The bulk of this assembly work was done by undergraduate physics students in an area of the laboratory often referred to half-jokingly as the "sweat shop." The point here, however, is not to focus on labor practices but instead on the fact that physicists are extremely actively involved in the design and construction of their accelerators and detectors, and seek credit for these "service" contributions to the collaboration.

Moreover, the larger point is not simply that infrastructural contributions are downplayed in astronomy because they are performed by technicians and considered significant in HEP because they are carried out by physicists. Though this is arguably true to an extent, the more important consideration is that infrastructural contributions in HEP are result specific and can involve the manipulation of thousands, if not millions, of technical parameters. In astronomy, on the other hand, instrument construction is more general purpose and involves fewer parameters.

What Does This Mean for Authorship?

All of this discussion of the means for authorship and attribution of credit in HEP suggests that there are very large questions confronting this field. As was argued at the start of this article, however, HEP is not the only field facing these issues, and still other fields are likely to follow soon. As Cronin (2001) argued, hyperauthorship is changing what constitutes the contribution of an author on a publication, along with causing many to question the value of authorship itself in this scenario. There is a strong sense in which this is a cause for those who study science and scientists to think carefully about what it really means to be an author, what sort of contribution merits authorship, and how different types of contributions are to be recognized. In many ways, this discussion is already in progress. So far, though, it has largely occurred in discipline-specific forums (e.g., Claxton, 2005; Rennie et al., 1997; Saffran, 1989). In addressing these issues, consider the three roles defined earlier for authorship.

Attribution of credit. Historically, individual researchers took responsibility for their entire experiments. They, perhaps with some assistance from technicians and/or students, designed the experiments, gathered the data, analyzed the data, and wrote up the results; however, the advent of "big science" (Galison & Hevly, 1992) has changed this. We saw earlier that there appear to be two components: (a) enabling discovery and (b) discovery itself. In the HEP collaborations, hundreds of researchers have devoted the bulk of their careers since 1989 to the design and development of a truly gargantuan research apparatus that will not be completed and generating data until 2007, at best. Without all of this effort, the data could not possibly be generated, much less analyzed. These contributions can be called "infrastructural." The contribution of these individuals is clearly significant and is deserving of some recognition.

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At the same time, however, infrastructural contributions also are different in an arguably fundamental way from the more discovery-oriented work that will take place once experimental data are being collected and analyzed. Specifically, an infrastructural contribution is generic to *all* articles published on the experiment. Moreover, these contributions were shown earlier to be quite effective in helping individual researchers to build their reputations, particularly when a difficult problem was solved. Indeed, no research can be done without these people's effort. The latter type, on the other hand, is publication specific. These contributions will be referred to as "discovery-oriented." A group seeks to answer a specific component of a research question in a specific way, collects appropriate data, and carries out the necessary analyses. The present system of authorship does not allow for a distinction between these types of contributions.

Ownership. In both HEP and biomedical fields, some have argued that the discovery (my word, not theirs) contributors should be the only ones listed or that they should be somehow listed separately. Paneth et al. (1998), for example, suggest making all authors "contributors" and providing an explicit indication of individual contributions to the overall effort. Others have made similar suggestions (e.g., Davenport & Cronin, 2001; Saffran, 1989; Smith, 1997). On one hand, this makes sense from a liability standpoint. These are the authors most likely to be able to defend the results at a fine level of detail, are most likely to be experts in the subarea explored by the article, and are ultimately responsible for the analyses contained within the publication in question. At the same time, though, the liability of the infrastructural contributors is crucial and should not be reduced below that of the author. Indeed, responsibility for an error in software or hardware that compromises the analytical results should not be pinned on those who did the analyses but on those who developed the hardware and software (though certainly the discovery contributors should have checked their results more carefully). As indicated earlier, one case was observed in which a physicist elected to remove himself from the author list of a publication to which he was an infrastructural contributor but did not agree with the specific analytical results presented. This is difficult in that he should be listed as an author to take responsibility for his infrastructural contributions, even though disagreed with the results presented.

The question of ownership also raises the possibility of adopting a system analogous to film credits. The "infrastructural" versus "discovery" distinction is analogous to the "above the line" and "below the line" terms frequently used in discussions of Hollywood, where big stars and directors are generally considered "above the line;" however, film credits also have a much finer grained level of distinction. There are a variety of standard roles in film production (e.g., "gaffer," "key grip," "second assistant director") that have standard expectations associated with them for which individuals will get explicit credit (Bechky, 2006). As Becker (1982) noted, this is not so fine-grained as to indicate what an individual's specific contribution to the production was on the second day of shooting, for example, but it does give outsiders a sense of the scope of an individual's likely role. And it provides a loose framework in which an individual can advance one's career in moving from project to project.

Reputation. When considered in light of big science, reputation via first and single authorships presents another mismatch. In the traditional scheme, first and single authorships are given almost exclusively to those who make discovery-oriented contributions. Those who make more infrastructurally oriented contributions are rarely recognized in such a high-profile way, but they do accrue informal reputation as detailed earlier and in Cronin (1995). This lack of formal recognition is clearly not lost on researchers, as evidenced in particular in Taubes' (1986) account of the intense competition for recognition in the H2 experiment at CERN. As has already been made implicitly clear, however, infrastructural contributions are critical to the research enterprise.

What's Ahead?

This is a topic of active discussion in the HEP community. While many interview participants expressed satisfaction with the current system of authorship, others described their enthusiasm for various alternatives that have been proposed. While there are many specific variants on these alternatives, most rely on some means for distinguishing between the two types of contributions outlined earlier. These proposals are generally analogous to proposals in other disciplines cited earlier in this article, and address the following general classes of concerns:

- All authors should be able to defend the research presented in a publication if, for example, they are challenged by a colleague from another experiment at a conference or meeting. Thus, people not closely familiar with the work should not be listed as authors (or at least as "major" authors).
- Authors who have made major contributions need means for distinguishing themselves from the rest of the people on the project. If hyperauthorship is to be used, there should be a way to draw this distinction. Indeed, many people already do this by listing their major efforts separately on their curriculum vita—but there is no formal way to verify these claims.
- Authors should be familiar with the work when they are listed as authors. This is similar to the first class, but here the point is that individuals should request to be removed or demoted if they are listed as an author/major author and are not familiar with the work. This raises interesting issues in the case of publications in people's nonnative languages.

Incentive Compatibility and Mechanism Design

While this list touches on issues of importance to scientists, it is not all-encompassing by any means. It was noted earlier in this article that researchers may be hesitant to get involved with a collaborative project if it is not clear at the outset how they will get credit for their contribution.

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Moreover, an analysis using game theory by Engers et al. (1999) has suggested that alphabetical author lists can theoretically result in research of a poorer quality than if contribution strength is signaled by author order. Even where researchers do become involved in collaborative work, such projects frequently take a back seat to individual efforts with a better defined payoff (Birnholtz, 2005). These points raise the issue of what economists refer to as "incentive compatibility constraints." In other words, how can we design a credit attribution mechanism that makes engaging in collaborative work attractive to scientists on the dimensions that are important to them?

There is a great deal of recent economics research in the area of "mechanism design" for the provision of public goods. In his classic discussion of what are now called Vickrey-Clarke-Groves mechanisms, for example, Groves (1973) discussed strategies for inducing individual members of teams to behave in ways beneficial to the team. This idea is directly relevant to the present discussion of authorship. If we consider credit for contributions to collaborative work to be the "public good" in question, the issue becomes one of how to structure the payoffs from collaborative work in a way that properly credits individual contribution and makes collaboration an attractive option. One problem with thinking in these terms, however, is that we do not have a sufficiently clear understanding of how reputational payoffs in science work. Specifically, most of the existing economics literature deals in readily quantifiable means of compensation (e.g., cash). To quantify the various aspects of scientific reputation and make use of this research on mechanism design, understanding of the issues involved must be improved. Thus, there are two key issues in understanding authorship that are ripe areas for future research: (a) better understanding reputational payoffs and (b) inducement of best efforts.

Understanding reputational payoffs. It is clearly the case in most fields that certain publications are more prestigious and have higher impact than do others (Price, 1986). In the LHC case, the first journal article to announce the discovery of the Higgs Boson will be tremendously prestigious. Many of the researchers interviewed want to be listed as authors on that article because of the associated prestige. As indicated earlier, many physicists also feel that it is only fair if all contributors to this effort are listed as full authors. On one hand, this is puzzling given that most of these same participants admitted that authorship means little when there are so many authors on the article. At the same time, it raises the intriguing question of how reputational value really accrues from publications and citations. Is it really true that being one of 2,000 authors on the article announcing the discovery of the Higgs Boson is more valuable than having one's single-authored internal ATLAS note cited in this article? In other words, what is the real value of being the ith of N authors, and how does this value vary as N changes? What is the value of being cited by a highly influential publication? How does this value compare to other means of attributing credit? These questions are difficult to answer in a meaningful way, but are the types of issues that must be considered as we critically assess the system of authorship in the face of new ways of conducting science.

Inducing best effort. Getting people to put forth their best effort is a classic problem of management and incentive alignment. In cases where rewards are not directly coupled to quality of effort, for example, there is some incentive to "game" the system. In science, for example, there can be pressure to maximize the number of articles one writes from a given dataset because the length of one's curriculum vita can affect perceptions of effort and influence. Though no cases of this were observed directly, one can imagine an incentive to game the system of getting noticed in HEP as well. Because strong reputational value is associated with solving difficult design problems, one could imagine putting forth a mediocre initial design, putting off until later the solution of these problems to maximize accrual of informal credit (A somewhat more sophisticated version of this also could be imagined that involved "trading" deliberate design flaws with a fellow collaborator, where each person includes a flaw that the other person heroically fixes.) Thus, we must carefully consider how to better align incentives with the inducement of best effort.

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