SCIENTIFIC AUTHORSHIP

Credit and Intellectual Property in Science

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The Collective Author

PETER GALISON

I. The Transcendental Author

In the formation of collaborations, there are practical questions that press upon us. How is an individual scholar to be evaluated for hiring and promotion? How can error be detected when every member of a team is not in a position to judge the final publication? But these and related questions are not mine here, at least in the first instance. I am after something different, I want to explore what it means, quite literally, for a collaboration to know something about the world, and I want to ask this question of the largest, most intricately technical scientific collectives ever established—the detector teams surrounding colliding beam accelerators at the end of the twentieth century. With collaborations mounting to over a thousand participants, it hardly takes algebraic topology to reckon that quite soon only a handful of these teams will embrace the careers of nearly all of the seven thousand experimental particle physicists expected to be employed at the end of the century. But to ask in what sense a collaboration can know, argue, or show something, it may be useful to consider a Kantian analogy.

At the very center of Kant's project of a *Critique of Pure Reason* is an argument directed in equal measures against the empiricists and against Descartes. While Descartes begins his attempt to secure knowledge by the cogito, I think therefore I am, Kant wants to interrogate the "I" itself. What, Kant demands, are we doing when we assume there is a unified self out of which the *I* speaks?

> There can be in us no modes of knowledge, no connection or unity of one mode of knowledge with another, without that unity of consciousness which precedes all data of intuitions, and by relation to which representation of objects is alone possible. This pure original unchangeable consciousness I shall name *transcendental apperception.* (A 107)

Kant here contends that all our representations of the world have to refer back to some common consciousness; without that funneling back to a single point of awareness the bits and pieces of our perceptions would remain disconnected and the objects around us would be nothing to us. Here is a metaphor (not Kant's): without communication back to some*one*, the myriad of individual weather observers, each privately recording hourly temperatures, would never come to recognize the existence of a weather front. Only when there is one or more observers who can view the spatial combination of these isolated data into isotherms or isobars does the cold front, as a concept, enter. Without the unity of apperception, each one of us would be like such an unintegrated amalgam of private, uncorrelated observers. But in the absence of the unity of apperception our world would lack far more than cold, warm, or occluded fronts; it would lack the very concept of an object.

Kant's insight was this: the unity of our individual consciousness is a necessary precondition for the unity of any appearance of an object, indeed that unity of consciousness is necessary for there to be *for us* any object at all. As the weather front metaphor already suggests, my concern here is not with the traditional Kantian question so much as the correlate of this unity of apperception in the functioning of a manifestly collaborative inquiry. I want to ask here: What does it take for a phenomenon to be something to a collaboration? That is, what are the specific mechanisms used to vouchsafe the existence of the "we" invoked when the collaboration speaks to the existence of a new entity or effect in science? Who—or rather what—is speaking?

In late twentieth-century physics we are faced with collaborations such as the four detector teams at the CERN Large Electron Positron (LEP) accelerator, where each is staffed by a team of some five hundred physicists from fifty institutions along with hundreds of technicians and engineers. In the trash bins of recent history lie the two collaborative detector teams that were to have constructed their machines at the Texas Superconducting Supercollider with a thousand physicists each. Even larger is the CERN-based Large Hadron Collider (LHC) with its twin collaborations of somewhere between fifteen hundred and two thousand physicists apiece. What, we can ask, does it mean for such concatenations of institutions *collectively* to have found a particle or confirmed a theoretical contention? I want to know what the *we* already presupposes in the collaboratively-produced document. *Where* is the information, *who* has it, *what kind of unity* of the collective is already assumed when the collaboration rules something to be the case about the physical world?

In moving from the conditions under which "I" can be uttered to the conditions of possibility for a "we," violence has, of course, been done to the Kantian position in several ways. First, it is clear that my concern is at a much higher level in the hierarchy of concepts—not in the conditions necessary for us humans to say "I see a pen," or "what is needed for us to have the notion of an object in general," but rather the conditions under which it is possible for it to be said: "The OPAL collaboration has measured the Z width," or "The UA1 collaboration saw the first W decay." Second, for Kant, the "transcendental" analysis of the unity of apperception signals two features. First—and I do follow Kant in this—the transcendental argument asks what is already taken for granted: in his case, "What is already built into the thinking individual self?" here, "What is already built into the collective self?" At the same time, however, Kant employs another meaning of *transcendental* when he takes these conditions of possibility to be a priori (that is, before any experience at all). For against the empiricists, throughout these sections of *Critique,* Kant argues that we could never extract the unified "I" from experience, just as we could never get to notions of necessity from an encounter with experience. On the contrary (so he continues), the unity of apperception is needed for there to be appearances in our experience in the first place, just as the very possibility of perception (intuition) presupposes that we already have some sense of space and time. In sum, Kant took his unity of apperception to be a priori true, and since it was therefore not learnable from individual experience, it was a fortiori not changeable within history. By contrast, it is part of my argument here that (a) collaborations, even at a given time, structure their sense of "self" variously, and (b) there is a broad and clear shift in the nature of the *collaborative self* from the bubble chamber work of the postwar period to the huge colliding beam collaborations of the 1980s and 90s, and then an even stranger shift now visible on the horizon with the advent of so-called mobile agents.

Despite these synchronic and diachronic *dis*analogies between the collective "we" and the individual "I," the spirit of Kant's question remains. In the extraordinary richness of high-energy physics, what is presupposed about the unity of the "we" that lies behind the pronouncements of a collaboration? What is the process, so painstakingly worked out by these collaborations, that lies behind what one might call the constitution of the collective self? What is the "we" and how does it relate to the knowledge claims that people outside the collaboration are invited to accept?

II. The Pseudo-I

That the collective experimenter differed from previous scientific authorship was already apparent in the 1960s, as bubble chamber physics began driving the size of collaborations from single digits to fifteen or twenty. Brookhaven National Laboratory's Alan Thorndike, then the leader of one of the most prominent hydrogen bubble chambers in the world, put it this way in 1967:

> Who is "the experimenter" whose activities we have been discussing? Rarely, if ever, is he a single individual. . . . The experimenter may be the leader of a group of younger scientists working under his supervision and direction. He may be the organizer of a group of colleagues, taking the main responsibility for pushing the work through to successful completion. He may be a group banded together to carry out the work with no clear internal hierarchy. He may be a collaboration of individuals or subgroups brought together by a common interest, perhaps even an amalgamation of previous competitors whose similar proposals have been merged by higher authority. . . .

> The experimenter, then, is not one person, but a composite. He might be three, more likely five or eight, possibly as many as ten, twenty, or more. He may be spread around geographically, though more often than not all of him will be at one or two institutions. . . . He may be ephemeral, with a shifting and open-ended membership whose limits are hard to determine. He is a social phenomenon, varied in form and impossible to define precisely. One thing, however, he certainly is not. He is not the traditional image of a cloistered scientist working in isolation at his laboratory bench.[1]

In this extraordinary text, Thorndike sketches the collaboration-as-author and it is just this feature that strikes me as central. One could ask other questions, questions about how individuals made their decisions to join the group or how each climbed the career ladder, but it is the much more radical import of Thorndike that intrigues me—his situation of the collaboration not as a collection of experimenters, but rather his identification of the collaboration-as-experimenter. Just in virtue of this fact, the experimenter becomes "a social phenomenon," an entity with indeterminate limits, geographical dispersion, varying form, and aleatory internal structure. As grammatically awkward as this may seem, Thorndike has captured something crucial about postwar physics when he says the experimenter has become "composite."

Despite this compositeness, the experimenter remained, in the 1960s, under the authorial name of an individual. Everyone knew the largest hydrogen bubble chamber collaboration at Lawrence Berkeley Laboratory as the Alvarez Group. Similarly, other bubble chamber groups at LBL were known by their leadership as the Trilling-Goldhaber Group, or the Powell Group. And at Brookhaven—where Thorndike was—no one would have had any difficulty locating the central figure of the Thorndike Group. For though the complex operation of a bubble chamber required expertise of various sorts, all of these expert subgroups reported back to a single center. Alvarez was as much in charge of the data-processing dominion as he was of the cryogenic engineers or the physicists. In the end, all decision-making about physics results to be published came back to him, and all funding *into* the group passed by him as well. For all these reasons, I take seriously the fact that the group carried the name of its single leader: the collaboration of the 1960s was modeled on a quasi individual, a single person who may have taken his actions in consultation and ultimately through others, but (at least to the outside) when the Alvarez Group found a new particle, it was, in a sense, as an extension of Alvarez himself. Alvarez therefore stood as the name-giving center of the group. Even while the actions of the team had already mutliplied into separate cryogenic, scanning, analysis, and mechanical subgroups, "Alvarez" referred two ways, both to the individual and to the composite, "pseudo-I," of the group as a whole.

III. Hierarchies and the Absent Center

Along with the growth in size (from experiments costing on the order of $10 million to collaborations with equipment alone in the range of $100 million to $1 billion), came many other alterations in the structure of collaboration. Bubble chamber groups, like those of Alvarez or Thorndike, had a single, leading institution: Lawrence Berkeley Laboratory in the case of the former, and Brookhaven National Laboratory in the latter. No one working at LBL in a group originating from Johns Hopkins would have had any doubt in 1967 that collaborating did not mean sharing equally in authority; LBL was the first among the many institutions with which it shared work. As the detectors shifted in scale, this local dominance could not be maintained: no longer could individuals, groups, or even nations join a collaboration in a fixed subordinate position. This fact was evidently not simply a feature of the political economy of particle physics, it reflected changes in the relations among universities in the United States, among the United States and the countries with which it collaborated (e.g., Japan), and among the various countries collaborating at CERN outside Geneva.

There is another dimension to this multiplication of centers that is at once technical and symbolic. In the bubble chamber, the apparatus itself had a certain unity—essentially a vat of liquid hydrogen, the structural and thermal integrity of the whole constituted the principle engineering difficulty. Could the chamber withstand the millions of compressions and decompressions that would allow bubbles to begin forming and then squish them back out of existence to prepare for the next round of interactions milliseconds later? Could the hydrogen be kept at a uniform temperature throughout to avoid convection currents that would make spurious curves out of "truly" straight particle trajectories? Technology shifted away from bubble chambers towards the hybrid electronic detectors that were designed to capture the detritus of colliding beams of particles and antiparticles. With that change, the technological unity of the whole began to disintegrate as well.

Two forces met. On the one side, the technical hybridization of the chamber made it easier in many respects to partition the work of planning, construction, maintenance, and analysis to different laboratories. One LEP collaboration, OPAL, divided their detector such that Bologna and Maryland took on the forward detector, the hadron calorimeter, and the beam pipe; Chicago took the electromagnetic barrel presampling detector; and Tokyo adopted the electromagnetic barrel calorimeter, the central detector, and the trigger system. Other groups—and there were tens of them—divided up the myriad of remaining detector components. On the other side, this division of "property" had a symbolic dimension as well: each group needed to have something to show, an identifiable piece of real estate that could be exhibited in slides and reports to funding agencies and, in some cases, national scientific ministries. While analytically it is helpful to separate these two dimensions (the technical and the symbolic), in the real world conduct of physics, such division is not sharp: the partition of the technological effort into identifiable component parts is at one and the same time a political-symbolic act and a practical-technical one. The bottom line was this: no one group could command the whole of a multiinstitutional, increasingly multinational collaboration. Scientific politics and work in the late 1990s would not allow a major detector to be "American" or "German," much less "Alvarez's" or "Thorndike's." And with that splintering, the group as quasi individual began to give way to a more federal association of parts; the collective self as unity disaggregated.

This shift was reflected in the leadership name, from the author name of a prominent physicist to the explicitly corporate "Executive" or "Collaboration" Council. In the detailed structure of scientific and technical protocols we can see, in detail, the de-centered authorship that mirrored these new conditions of production. Let us focus attention on some specific sites and authorship practices. It is, I believe, *only* in following the specific mechanisms by which the collaboration comes to assign its name to a result that we can see both what the collaboration is as an internal structure and how it can carry conviction to the outside world.

IV. The Protocols of Authorship

At the end of the two-mile-long Stanford Linear Accelerator Center (SLAC) lies the Stanford Linear Detector (SLD), which opened in the early 1990s. Not wanting to be caught off guard when they opened for business, SLD spelled out their author policy in a publication that predated by several years any actual measurements. Their first and simplest specification fixed who should be included. When a publication allowed only limited space for author names, authors were to be listed in alphabetical order; when printing in circumstances of unlimited space, institutions would be put alphabetically, and authors given alphabetically within those institutions.

> There should be no exceptions to the above, such as placing the student's name first if the paper originated as a thesis, as our first priority should be the coherence of the group and the de facto recognition that contributions to a piece of physics are made by all collaborators in different ways.[2]

If the thesis was written by a single person, then that might well be indicated in the first footnote. But as this stricture indicated, group coherence—the stability of the collaboration as such beyond the contribution of any individual—became a factor in the order of the author list from the get-go.

"Who Is an Author?" asked the SLD Collaboration Council innocently enough in July of 1988. But the answer had important implications not only for the individuals in the collaboration but for the very process of writing and certifying. The council: "For physics papers, all physicist members of the collaboration are authors. In addition, the first published paper should also include the engineers." Now, as was evident from the specification of "physics papers," it was clear that there were to be other forms of writing with different authorship protocols. For example, there were reports to be made on the hardware, and here the protocol divided the cases into three. If a system-wide innovation was at stake—for example, to the WIC—then the relevant physicists and engineers should both sign the paper. The same was true for a contribution to the construction or functioning of subsystems. The pad electronics, by way of illustration, was a component part of the wire chamber, and the subgroup of physicists and engineers responsible for the work could sign it. Indeed, the protocol indicated no objections to either system or subsystem hardware reports being authored by engineers alone, should that prove appropriate; anyone *not* contributing to one of these efforts was encouraged to delete his or her name. Finally, there were "individual" contributions that were "envisaged as rare exceptions where one person has produced an individual 'invention.'" The system manager must first agree that the proposed paper merits such a classification. Then the author would circulate a memo to all system physicists (those participating in that particular system) declaring the intention of producing a paper and "inviting those interested to contact the author to help in writing. A draft of the paper should be circulated to provide other system physicists an opportunity to ask that their name(s) be added."[3]

As the various types of hardware papers indicate, the collaboration "speaks" in different registers or modes depending on both content and intended audience. When it comes to the physics itself, this differentiation of registers became even more refined, running from internal memos to the most crucial physics paper staking a claim to new results. We can paraphrase the genres of SLD literary production as follows:

1. *Internal memos* were not for public or even full SLD distribution. The writer(s) could freely decide on who would count as authors and what would be contained as content.
2. *SLD notes* circulated to the full SLD collaboration, and could, when appropriate, be allowed to reach the public. Author(s) were invited to circulate such writing to all people involved in pertinent data or apparatus. SLD notes required approval of the relevant system manager.

3. *Conference Proceedings on SLD Physics* were to be signed in the form "The SLD Collaboration, presented by Isaac Newton," with a footnote naming all of the SLD authors.
4. *Conference Proceedings on Instrumentation Research and Development* would list group, if well defined, and acknowledge SLD collaboration. Here too system manager approval would be required.
5. *Reviews of SLD Physics or Detector Design* would be treated as type 3.
6. *Review papers*, including but not restricted to SLD results, assuming they were rapporteur talks and not talks of type 3, would be allowed to cite unpublished SLD results "as individual efforts" and therefore identified as publications from the author's home institution.

For our purposes, the significance of these various modes is that the regime of authorship is a function of both the scope of the audience *and* the knowledge claim. When the scope of the knowledge claim is highly restricted—to the functioning of a piece of hardware, for example—the author list can simply be the writer(s) and may omit physicists altogether if the work was done by engineers. Or when the audience is sufficiently restricted, as in an "internal memo," an individual physicist may stand as a single author. By contrast, when a principal physics claim is made, such as the discovery of an anomalous decay, and is to be disseminated to the world at large, the collaboration *had* to be the author, with the individual writer relegated to the role of "presenter." Even subinstitutional "ownership" of an analysis was to be avoided—constituent groups were forbidden from giving a cover number to memos because they might be read as "coming from institution X" (rather than the collaboration as whole).

Indeed, in just the central case of a physics publication, considerable care is taken to define what is to count as a constituent author within the collaboration. It is a definition that alternates between the practical demands of the career structure of the participants and notions of what kind of work counts as author-making.

> [F]or [physics] papers, an author is defined as a physicist who has contributed (by running shifts, doing analysis, building hardware, etc.) to the results which are the subject of the paper. In normal circumstances, this means that anyone joining SLD full-time should become an author

> essentially immediately. To accommodate dry periods when no data are taken, a person who has been a member of the collaboration for at least one year is automatically included on all papers, even those based on data taken before he joined the group. Since joining the collaboration is not necessarily synonymous with joining an institution, the precise starting date is left to the integrity of the individual and to the Collaboration Council member for his institution.

Note a certain tension here. On the one hand, there are certain kinds of work that serve as a necessary precondition to authorship (shift work, analysis hardware construction); at the same time, the criteria recognize the vicissitudes of life around an accelerator (the hazards of "dry periods") by allowing someone to be an author of results obtained before he or she joined the group. On similar grounds, "A person leaving the collaboration remains an author for a time equal to that for which he was active on the experiment," unless that person requests otherwise.

When it actually came time to write a paper, the process would begin by some set of writers producing a detailed memorandum. This would then be presented at SLAC, followed by the formation of a committee consisting of five to seven people, which, upon approval, would bring the paper to the collaboration as a whole, which would then have two weeks to comment. After taking any criticisms into account, a "group reading will be scheduled," normally for three hours. "At this public reading, it is in order for all present to comment, argue about conclusions, etc. (The creative ferment stirred up in this way generally leads to improved papers.)" Out of the public reading would come a new draft with two more weeks in which criticisms could be registered, a final draft circulated for one week, at which point only corrections of "errors of fact or of blunders in English or typos are permitted."[4] Finally, when a major discovery was thought to be in hand, a press conference or press release was to be made, authorization to do so had to come from the SLAC director, and the SLD cospokesmen, with advice from both the Collaboration Council and the Advisory Group. For cause, the bona fides of the experiment were guarded by more than one set of doors.

The complexity of these rules stems in part from two desires that pull in opposite directions. Pulling towards *inclusiveness* is the desire to make the collaboration as complete and unified as possible; anyone left out might undermine the authority of the claim. Readers might ask *why* someone's name failed to appear. This is explicit in the protocol of the DØ collaboration at Fermilab, where the rules state near the very top of the document: "Withdrawing individual names because of a lack of close involvement in some particular aspect of the analysis will tend to undercut the impact of any publication and is therefore to be strongly discouraged."[5] As we saw, even a student's thesis work was to be instantly and seamlessly absorbed into the collaboration as a whole. Pulling toward *exclusiveness* is the desire to make each name stand for command of and agreement to the work. Both tendencies (inclusiveness and exclusiveness) are tied to the issue of credibility: the collaboration must function with sufficient unity for its name to stand for something. The credibility of a fragmented "we" would have the effect on the outside world not unlike an individual experimenter a hundred years ago who said that a certain substance both was and was not magnetic–the contradiction would essentially erase him or her as a contributing member of the research community.

Unity is important. Senior particle physicists remember all too clearly the flack the E1A experiment received when different members went public with preliminary and contradictory claims about the existence or nonexistence of weak neutral currents. Rapidly—and tragically for their credibility—the superb experiment with some of the most stunning results of the last half century were derided as having discovered "alternating neutral currents."[6] The fate of the collaboration and its results ride together.

In the 475-physicist ALEPH Collaboration at LEP, one of the spokesmen, Gigi Rolandi, recently wrote, "The general principle is that no ALEPH result can be presented in public without the approval of the Collaboration. Aleph can have only one official result for a given analysis."[7] They achieve this one result by a process not unlike that of SLD. A physicist or physicists present an analysis at a regularly scheduled Thursday meeting of the collaboration, with suitable prior advertisement. At that meeting, the collaboration—or more precisely, its representatives—would vote to allow either a public presentation or a paper preparation. If the latter is chosen, the writer(s) present a draft to the chair of the editorial board (a person chosen by a collaboration spokesman), who then designates some referees. After one or two drafts, the paper may come before the collaboration for a second time; but whether or not it does, in all cases the editorial board votes on it for final approval. One could describe this process in two different but very closely related ways. On the one hand, it is a matter of the collaboration finding a way for the collective to know something—getting the paper right. On the other hand, it is a matter of structuring the collaboration's output in such a way that they have a single, persuasive message for the outside world.

In DØ, one of the two massive colliding beam detectors at Fermilab's Tevatron (a collaboration of 424 physicists and growing), the authorship document insists that all "serious" participants ought be on *all* publications. The snag, not surprisingly, is defining *seriousness*. With certain exceptions, their March 14, 1991, policy on authorship demanded the following to be an author: one had to be a senior graduate student or above, and work for a year prior to submission of a paper for publication. As in most of these collaborations, there was a provision allowing authorship to continue for a year after departure. But a putative author must have (1) done one half the average number of shifts on data runs relevant to the paper in question. And beyond that, he or she must have (2) fulfilled one of the following two requirements: " (*a*) spent at least (the equivalent of) one person-year, at one's home institution or at Fermilab, working on the implementation of part of the detector or of the software used in acquiring the data from the run(s) on which the paper was based; or (*b*) made a major contribution to the analysis of the data from the run(s) on which the paper is based. This includes writing software, performing analysis, producing DST's, writing the paper, internal review of the paper, etc."[8]

These criteria in some ways embody what might be thought of as an updated version of earlier experimentation, as criterion 1 required physical presence on site. This restrictive clause soon died as it became clear that the work structure of an experiment in the 1990s could not be tied to that specific form of physical labor. On June 2, 1994, the collaboration revised its "Rules of Authorship" toward a far broader concept of what would make one an experimenter. Now the demand became the more broadly construed demand that, to any combination of the following potpourri of activities, an author must contribute a total of twelve hours per week:[9]

1. Design, construct, or debug the detector or the DØ test facility.
2. Write software, for example, utility packages, Monte Carlo simulation of detectors; process or analyze DØ data.
3. Process Monte Carlo events or data obtained at DAB or any DØ test facility.
4. Run shifts at DAB or at the DØ test facility. (Four per month were thought to be needed to "prevent memory loss and the need for extensive retraining.")
5. Manage personnel issues for DØ, administer contracts, grants; serve as a "physics convener," or convener of a technical topic such as electron identification.
6. Write or review DØ papers; advise graduate students on DØ matters.
7. Take part in DØ meetings, workshops, or discussions; analyze physics simulations or physics analysis for paper or Ph.D. thesis.

(The authorship rules specify that items 6 and 7 require special authorization from the group superego in the form of the spokesman-appointed Committee on Authorship.)

As it came time to actually produce a paper, the DØ 1994 procedure went like this: When someone had an "imminent" physics draft note, the cospokesmen of the collaboration appointed a custom-built editorial board to review the results. That is, for each case, the editorial board was composed of the author(s), plus four other physicists: an advisor (also known as the "Godparent"), someone from the same group (that is, the same physics or algorithm group), and two other collaboration members. While one draft physics note went to the editorial board, another copy was released to the full collaboration—any member could comment via the electronically distributed DØNews. By putting the author(s) on the editorial board, the collaboration deliberately broke with the referee model employed in other groups in order to "facilitate productive exchange," to "avoid misunderstandings," and to eschew "confrontations." While harmony might be fostered by this more inclusive editorial board structure, it was at the same time the basis for a more probing inquiry. Unlike the refereeing process in other groups, the rules of authorship in DØ specifically mandated that the editorial board was to have access to all backup materials including theses, backup analyses, DØ notes, and other items as required.

Assuming that the editorial board approved, a waiting period followed: four days posting for a DØ physics note; ten days posting for a possible publication. Normally, about ten percent of the collaboration comments on the posted note. In addition, the authorship rules demanded a "public reading" at a DØ Physics Analysis Meeting, a General Collaboration Meeting, or a specially scheduled session. Finally, at the end of the waiting period, assuming the editorial board could assure itself that all objections had been addressed, the vetted physics note gets a number (unlike the unreviewed DØ notes) and is entered into the publicly available disk space of the collaboration. Those destined not to be published would be marked "Preliminary Results from the DØ Collaboration," while those headed for the world of print were launched forward toward their target journals.

My final example is the OPAL Collaboration at CERN's Large Electron Positron collider. OPAL (Omni Purpose Apparatus for LEP) consisted of some twenty-four groups distributed in their responsibilities over some fourteen "subdetectors." As with all of these very large colliding beam detectors, maintaining integration of the whole became in every respect the central and most difficult problem. When it came time to author papers, OPAL, like SLD, ALEPH, and DØ, aimed to pull the candidate publication into line with some notion of the collaboration as a whole:

> OPAL operates a rigorous internal review procedure for all physics results which are to be published or shown outside OPAL, whether final or preliminary. The aim is to ensure that OPAL results are reliable, of high quality and well presented. Results should never be discussed outside the collaboration before members of OPAL have had an adequate chance to examine, criticize and approve them.

In a procedure altogether similar to the other groups we have examined, someone with a physics idea would ask one of the physics coordinators to appoint an editorial board, a body consisting of the authors along with four other OPAL physicists, among whom should be: a native speaker of English, an expert in the specific area of the paper, a nonexpert to ensure that it is comprehensible more broadly, and someone located outside CERN. When the editorial board is satisfied—and writer(s) must leave the board at least a week to look it over—the board electronically launches the draft to all of the OPAL laboratories scattered around the world using a program called DISPATCH. For at least two weeks the paper must remain posted in this way so that comments or criticisms can flow back from the various sites. At the end of this criticism period, the editorial board, satisfied that due consideration has been given to any objections raised by the broader collaboration, schedules a public reading. This event—attended by the editorial board members as well as other interested members of the collaboration—is the occasion for last, substantive corrections before the draft goes to the collaboration for final approval.

Also, like the other collaborations, OPAL writes in different registers. *Physics Notes* are defined as "internal OPAL documents which allow the reader to understand and judge the reliability of the analysis."[10]

> Since physics notes describe the analysis of results intended for the public domain, they are made accessible to physicists outside OPAL on a restricted basis. Single copies are given on request to interested parties who have a valid reason for wanting one but they are not sent to any non-OPAL mailing lists. They can also be used, for example, by OPAL collaborators in discussions with students and colleagues outside OPAL.[11]

Note that here, as in the other "constitutions," draft journal papers require public reading—an open OPAL meeting at CERN, following which the authors consult with the editorial board and prepare a draft for final approval. And again, the wider the audience, the deeper the article must penetrate into the collaboration itself. Presentation to the outside and the creation of a "we" inside enter together.

Toward the end of the twentieth century, when the Department of Energy commissioned a report on future modes of high-energy physics research, the problem looked different than it did even a few years later. Thousand-strong collaborations struck the committee as unreasonable, as they skeptically looked ahead to the SSC. By the time the LHC had amassed an army of two thousand physicists, discontent had set in even more deeply: when the European Committee for Future Accelerators polled members of the big teams in 1995, they found that some seventy-five percent of their respondents disliked present publication habits, largely because it damaged the possibility of career advancement and of receiving credit for their work, and some sixty-seven percent wanted change. Yet few wanted to limit the author lists—though they did want more weight placed on internal publications.[12] Indicative of things to come was the publication of the first Fermilab claim for the existence of the top quark: some eight hundred authors signed off on the initial papers.

Unease stemmed as well from the perception of outsiders. In 1988, the Department of Energy committee uncomfortably contemplated the consequences of being inclusive in the author lists, as such endless rosters gave physicists from other fields "the impression that all individuality is submerged in high energy physics," a not irrelevant image when questions of hiring, tenure, and even field support arose. Worse, the committee feared that it was becoming impossible to know who was responsible and who understood the experiment in detail. Indeed, at the end of the day, the very length of these lists of names radically devalued the worth of the publication on an individual's cv. As a direct consequence of this publication inflation, the committee noted that evaluation increasingly was being based on recommendations, with all the problems that letters brought, and not the work itself. One idea—an idea we saw considered in many of the author regulations—was to reduce authors after first publication. But this met with strong objections, for example by those people who maintained a calibration needed for subsequent results. One young physicist put it bluntly when he said, "these experiments truly are the result of the work of 100 people and it would be fundamentally dishonest to pretend otherwise."[13]

In February 1996, Roy Schwitters—former director of the now defunct SSC—suggested in the *Chronicle of Higher Education* that new guidelines be adopted in the big teams. These included listing all members of the team when "planned" discoveries were made—discoveries anticipated at the time of construction. He also called for changes that would encourage people to publish in the area of work to which they had contributed. Machine builders would publish technical reports about component parts, experimenters would publish their analysis of data; and software engineers would produce reports directed at their peers. Finally, Schwitters advocated wide intra-experiment circulation of reports before publication, and the publication of more than one published interpretation if opinion divided within the experiment.[14]

A real-life example of explicit, published dissent from within a collaboration occurred in the spring of 1995, around research prosecuted at the Los Alamos Meson Facility. The majority of the collaboration published a paper strongly bolstering the idea that neutrinos had mass (more specifically that there were good candidate events showing oscillation between anti-muon-neutrinos and anti-electron-neutrinos). University of Pennsylvania graduate student James E. Hill disagreed—and *Physics Review Letters* published back-to-back his dissenting paper: "An Alternative Analysis of the LSND [Liquid Scintillator Neutrino Detector] Neutrino Oscillation Search Data on [anti-muon-neutrino → anti-electron neutrino]." As far as Hill was concerned only two of the nine purported oscillation events were truly good candidates. Immediately the collaboration plunged into some real soul-searching, asking questions like these: Could someone dissent from the collaboration this publicly? If there was dissent, who ought to be signatories on the paper? What were the obligations of journals like *Physical Review Letters* toward the collaborations that produced the data? Though lauded by some, including Schwitters, and condemned by others, such open clashes did little to resolve the fundamental issue of authorial splits within the pseudoindividual of a collaboration.

In fact, one can see written into the authorship protocols of large collaborations a fundamental tension between condensing authorship around an individual or small group and the equally powerful drive to diffuse authorship around the entirety of the collaboration. These opposing forces are apparent in the new literary category of "Scientific Note" that ATLAS (one of the CERN Large Hadron Collaboration detectors) promulgated in February 2000. Titled "A New Class of Publications to Recognize Individual Contributions to Future Large Experiments," the ATLASIANS posed the problem this way:

> The career advancement of Experimental High Energy Physicists at Universities and Research Institutes has become harder in the last ten years due to the large number of authors appearing on each publication within the field. This large number of authors makes it harder to evaluate the individual contribution when comparing with other fields in science. Collaborations associated with forthcoming LHC experiments are typically several times larger than existing experiments. Thus, if no action is taken, the problem of recognizing individual contributions to experiments will become even more acute.[15]

Caught between the twin exigencies of representing the experiment as a whole and individuals in particular, the ATLAS team proposed a delicate authorial dance. First, they insisted that it was "understood" that the LHC experiments would present their scientific results "under the name of the full collaboration." But immediately they went on to introduce a new literary form, the scientific note that would lie halfway between a full-bore scientific article (authored by the ATLAS writ large) and the commonly and more roughly produced "ATLAS Notes," which were directed within the bounds of the collaboration.

According to the ATLAS Collaboration Board, scientific notes would emphasize a "clarity, completeness and style" appropriate to ordinary scientific papers, would appear in refereed journals (not just in posted web and internal publications) and yet would not impinge on the territory of full–collaboration scientific results. When the full collaboration did publish their scientific results, the Board urged its ATLAS colleagues to cite, explicitly, the achievements of individuals that had been established in scientific notes. Individuals with specific names and responsibilities ought to author these notes—"subsystem communities" should not invoke this person-highlighting form. Of course for the new form to work, editors would need to restructure their pages to accommodate the new literary object: "The editors of scientific journals will be contacted to establish a new class of publications under the name of technical or scientific notes. These notes will contain results of analyses, detector development and improvements, detector and physics simulations, software, algorithms and data handling." Above all for scientific notes to boost the careers of particular persons, the collaborative habits of inclusion would need to be curtailed, as the section "Authors" made clear: "Scientific Notes should represent the work of a single individual or a small group and be signed only by the direct authors. Naturally, such work will often benefit from contributions, past or present, of persons other than the direct authors. When so, such help should be duly acknowledged, but not necessarily lead to inclusion into the author list."[16]

If the refereeing process is part of the mechanism that constructs the author, so to speak, then the statutory fate of scientific notes is quite revealing. One final piece of the mechanism had to do with content control, and, in principle, the following would govern its evaluation. First, at least optionally, the scientific note would pass through the normal procedures for the (internally aimed) ATLAS Note. Second, the scientific note would follow the procedures established for full-collaboration papers, included internal refereeing as guided by the ATLAS editorial board and subject to final approval of the spokesperson. Finally, the scientific note would be refereed like any other journal submission in physics–by evaluators outside the collaboration.

Reading the authorship protocols of the many collaborations discussed here, including ALEPH, SLD, DØ, OPAL, and ATLAS, complicates Schwitters's considerations. Some collaborations (as we have seen) actively *discouraged* the withdrawal of names from the author list because such actions could be seen as dissent undermining the force of the argument. Others specifically forbade an individual or group of individuals from signing a publication under their own names. The coherence of the group counts for much: for credit, yes, but also for the continued legitimacy of both the group and its productions. Yet other collaborations banned a university group from publicly assigning a number to the document. To print on a particular preprint the line "University of Michigan 97–23" on a collaboration document would (on the view expressed in some of the authorship protocols) be to arrogate both credit and responsibility. Even when, as with the newly coined Scientific Notes of ATLAS, the collaboration sought to reinstate the individual with partial-group authorship, the collaboration not only demanded final say on the paper, it exempted main-claim scientific results of the collaboration from being treated by a handful of physicists authoring a scientific note. Disunity of authorship appeared to many if not most participants in these large collaborations as tantamount to epistemic subversion. All 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. In short, they aimed to secure the integral structure of "we," in ways that often cut against the grain of Schwitters's proposals. The whole of these massive authorship protocols aim to form the "self" of a monster collaboration so that the "we" of the collaboration can produce defensible, authored science.

V. Mobile Agents Confront Kant

So far we have followed an economic system of credit that pitted the individual I of a single authorial name reward against the pseudo-I of the group. Some of the procedures (like the Schwitters proposals or the renegade publication at the Los Alamos Meson Facility or the compromises implicit in the ATLAS scientific note procedure) aimed to reinforce the individual's claim to accomplishment. At the same time, groups inaugurated other mechanisms to *prevent* individuals from claiming identifiable contributions—among these were protocols to discourage people from removing their names, procedures to enforce a group endorsement of individual publications, regulations to control who can speak and where.

But even to pose the credit economy in this way is to assume that individual contributions could, in principle, be isolated. Formally, and this is stamped into the software, we are dealing here with a more or less fixed network of contributors, dispersed institutionally, of course, with complementary specializations. In a large colliding beam experiment, one group, say from U.C. Riverside, might control one component, say a muon calorimeter, and be responsible for writing and maintaining the software that collects and formats data collected by that component of the larger machine. That hardware and its software image constitute the group's claim to authorship in the collective.

In the factory-style structure of the postwar laboratory, the center (such as Alvarez's LBL Bubble Chamber), was in every sense the centerpiece of the collaboration. LBL was where the center control on everything from software to detector control to scientific judgment took place, even if occasionally Alvarez distributed bubble chamber film to be analyzed and even published elsewhere. But even as that center-directed structure dispersed from 1975 forward, there remained a certain legacy of the centered collaboration in the handling of data. In particular, groups continued to interact through client-server relationships. By the late 1990s, if the center computer needed to distribute computational workload, many large systems were outfitted in such a way that it could download certain programs; in other, applet programs, the user could deliberately download a program. Yet there was no question, at the end of the twentieth century, that the computers at CERN (or Fermilab) constituted the centers of their respective collaborations. Hierarchies of computational capacity, access, and control filtered the process: from CERN through national computers, down to laboratory, group, and to the individual's workstation.

Each element of this rigid regulation of capacity, access, and control presented problems for early twenty-first century collaborations. One response within the largest collaborations has been to introduce *mobile agents*—self-propelled programs capable of leaping from one computer to the next, with the ability to dissolve the hierarchical relations of access and control.[17] Acting like solvents on the posts and beams of classical collaborations, these agents complicated, in striking new ways, the constructed subject position of the experimenter. Let me explore this line of development further, as it bears directly on the future of massive-collaboration authorship.

In the 1990s, four of the largest, most data-intensive experiments then under construction allied themselves in a metaexperiment known as GriPhyN, the Grid Physics Network.[18] This project did not only seek to alter the four projects (LIGO, the Laser Interferometer Gravitational Wave Observatory; SDSS, the Sloan Digital Sky Survey; along with the two huge CERN Large Hadron Collider collaborations discussed earlier, CMS and ATLAS). Each of these four collaborations expected in the early years of the century to be shuffling hundreds of petabytes of data, where a petabyte is a thousand terabytes and a terabyte embraces a thousand gigabytes. Such massive data sets, along with vast associated computational needs, outstripped the memory and computational power of any computational network expected to be workable in a reasonable time. In response, the GriPhyN (meta)collaborators intended to introduce mobile agents, where these mobile agents could hop from computer to computer anywhere within a network at times it, rather than following a path that a central computer, deemed appropriate. In hopping, the mobile agent could reproduce itself or could simply leap, taking stock of where it is in its computation so it could resume computation in the new host.

There is more. Mobile agents work around the usual hierarchies that ordinarily segregated the highest level centers (like CERN) from national centers (like France) from laboratories and, in turn, from the individual uses. Just because they must establish, in each locale, means of coordinating different priorities, security arrangements, performance, reliability, and so on, the mobile agents do not resemble the procedures dictated from above. GriPhyN's goal was to use the myriad of self-activating wandering programs within the four collaborations to create a model of a coherently managed distributed system, "where national and regional facilities are able to interwork effectively over a global ensemble of networks, to meet the needs of a geographically and culturally diverse scientific community."[19] The effect of these agents is to render different kinds of equipment and protocols transparent, from the massive processors at laboratory or regional centers all the way down to workstations. Take the Sloan Sky Survey, a project to map a large fraction of the northern sky to faint magnitudes (including about ten million galaxies with highly processed images of each one), along with a comprehensive survey of each object's spectroscopic signature. Scientifically, SDSS could hardly have a broader ambition, and as such was more a platform for collaborations than a well-defined collaboration per se. The Sky Survey would provide the basis for a statistical survey of the galaxies, stars, and quasars in such a way that it would shed light on issues ranging from cosmogenesis to galactic structure. Imagine that an astronomer user (or group of users) of the Sloan Digital Sky Survey wanted to examine correlations in galaxy orientation induced by the gravitational lensing effect of intergalactic dark matter. Data needed for this task could be stored in a network cache, in a remotely located disk system, or in a deep and compressed archive. For her project, the astronomer would need a computer system that could find all these data and images, produce any images not previously constructed from pixels, and then actually compute the correlations—shuffling terabytes of data and computational programs back and forth, and possibly manipulating an even larger simulation file to compare against the actual data.

Suppose GriPhyN succeeded in providing a truly centerless, scalable, heterogeneous computer resource for these four multipetabyte collaborations. Suppose that these mobile agents could so effectively wander through the system that no one need care about the dimensions of the collaboration. That is, assume that as groups and individuals join, withdraw, or move to other tasks, their computers continue to provide partial time storing, and computing and to recreate data. Who or what is the experimenter emerging here? Something is in construction that no longer quite fits either the "I" or even the well-defined, bounded "pseudo-I" that we expect to find as the presupposed subject of the statistical sky object. This new subject is coordinated but not commanded from a point, functioning more like a hive than a hierarchy.[20] Asked *where* the data are or *where* the data are being reduced, we would have to answer: in the hive-I of the Grid. If this is right, then the knowing subject presupposed by the establishment through GriPhyN's version of SDSS of a gravitationally induced lensing of galaxies is truly without fixed boundaries. Whatever fictions are demanded by the apparatus of prizes, promotions, and publication, there would be neither a unified individual nor even a bounded team at the (metaphorically) small end of the telescope. In the place of Kant's transcendental unity, we would have an ever-fluctuating mobility of apperception. The "amorophous-I" is a new species of author whose claims will require new forms of evaluation.

VI. Conclusion: Authorship and the Collective Self

Intriguingly, during the last several decades, two strands of inquiry into the nature of authorship have existed side by side without interacting. In addition to the scientists' own efforts to grapple with the problem, there is the literary-philosophical literature of authorship to which the French have contributed so extensively, especially the work by Roland Barthes that was so strikingly reconceptualized by Michel Foucault. In part, Foucault, though in an utterly different idiom, was also grappling with the problem of individuation of the authorial self, and it is worth considering the relation of these two sets of consideration to one another.

For Foucault, one set of problems involves the establishment of what counts as the "work" of an author. He asks: Are we to attribute the status of work to everything he or she wrote, and if so what will count as "everything"? Observing that only certain bits of speech are seen as singular, that is differentiated from everyday remarks that could have been uttered by anyone at all: "What time is it?" except in special cases, is not part of the language we call authored.

> The author's name serves to characterize a certain mode of being of discourse: the fact that the discourse has an author's name, that one can say "this was written by so and so" or "so-and-so is its author," shows that this discourse is not ordinary speech. . . . On the contrary, it is a speech that must be received in a certain mode and that, in a given culture, must receive a certain status.[21]

Assigning authorship to a certain body of texts or utterances has consequences; authored speech is characterized by a certain "mode of existence, circulation and functioning of certain discourses within a society." [22] But Foucault cut science out of his analysis. In his view, after the seventeenth century, the author's name no longer conferred authority on scientific texts, since the truth of the sciences was in principle always "redemonstrable." Foucault argued that author names served only to label theorems and otherwise decorate the *results* of science. Given the exceptional lengths to which both individuals and collaborations go to protect their "good name," however, Foucault's diametrically opposed categories of science and the rest seems, on the face of it, to fly in the face of the lived world of scientists long after 1700.

But suppose we omit Foucault's demarcation of scientific authorship from authorship more generally. Two questions then emerge from his analysis. First, one could ask how works are associated back to a given author, how, in a historically specific way, authentification actually functions. Quoting St. Jerome, Foucault showed how, in Jerome's time, there were rules that included or excluded particular writings. Were certain works of notably lesser quality, for example? Were there certain places where doctrine flew in the face of well-attributed assertions made elsewhere? Were there sections or works with references to times subsequent to the putative author's death? These each were signs of imperfection, of the failure of a particular text to have been by the author in question, and as a result, Jerome struck them from the canonical list of authentic productions. No doubt the problem of authentification could be extended into more recent periods, and perhaps even in some domains of science—the whole minifield of scientific misconduct would be informative here. But for various reasons (including the economic viability of the objects studied, the very different structure of team research, the availability of individuals' work for inspection, the modes of honorary authorship, and perhaps even the scale of collective authorship), particle physics, astrophysics, and observational cosmology—unlike immunology, clinical epidemiology, molecular genetics—has not been caught up in scandals of fraud and the powerful institutional framework for its detection and prosecution. Indeed, I do not know of a single instance in high-energy physics where fraud, fabrication, or authentification of authorship became a pressing issue.

But there is another, and much more interesting direction in which Foucault proceeded: not from author to work, but from work to author. Beginning with the work itself, he asked: What kind of author does this work presuppose? In posing such a question, Foucault took up a variant of the fundamental Kantian question with which I began this essay, though now about the individual author rather than the "I" per se, and now in a historicized frame, not in the form of an a priori structure. Foucault asked of that which was written:

> What are the modes of existence of this discourse? Where has it been used, how can it circulate, and who can appropriate it for himself? What are the places in it where there is room for possible subjects? Who can assume these various subject functions? And behind all these questions we would hear hardly anything but the stirring of an indifference: What difference does it make who is speaking?"[23]

Here the author protocols of contemporary physics *do* intersect Foucault's questions. The modes of discourse are variable, and importantly so: draft physics notes, physics notes, scientific notes, technical publications, rapporteur talks, conference presentations, physics publications—each had its characteristic content, form of review, specified author names, and intended range of circulation. Who could speak was also strictly regulated; each authorship committee (or its equivalent) precisely determined what could and could not be said "publicly" (where public was itself variable). And finally, who counted as someone who could participate in the author list was itself a finely tuned affair—from temporary participants to masthead members; from engineers allowed to sign on initial papers and technical reports and forbidden from signing physics; from authors left off papers to others actively dissuaded from removing their imprimatur.

What distinguished turn-of-the-century authorship in high-energy physics and astrophysics from other domains of collaborative scientific work is the confluence of three factors: First, there was the raw scale of the collective author, in particle physics moving upwards from five hundred to two thousand people. The metacollaboration of GriPhyN must, in some sense, be understood as a collaboration of more than five thousand scientists and an equal number of engineers and technicians. Second, there is the highly structured system of control over what can be said, when, and to whom. Finally, there is in many colliding beam experiments a fundamental *heterogeneity* that makes the collaboration as supraindividual author more than the additive sum of many individuals all executing similar tasks. The team supplanted the individual not because the individual was just articulating a widespread murmur of the group. No, the team replaced the individual because the individual did not (could not) *know* the length and breadth of the experimental problem. When the spokesperson spoke, she did not necessarily articulate the general consensus, she spoke of things that no one in particular could ever possibly fully know, but that the group could, in the end, assemble. That assembly was two-fold: a construction of the group and a construction of the argument it presented.

Perhaps the distinction might be put this way. After Foucault's lecture "What Is an Author?" Lucien Goldmann, the great philosopher-literary historian, stood up to say that he understood how the author as such had died. After all (so claimed Goldmann), his account of Pascal in *Le Dieu Caché* showed Pascal to be uttering something belonging in a sense to a group, not to the products of an isolated and genial mind. In light of this, Goldmann concluded, he—and Foucault—were both saying something similar: A focus on the group, not the individual, was necessary to understand the broad and deep manifestation of a single rule-governed collective voice.[24] Foucault, of course, did not here or elsewhere claim that the author was dead—his own interest was in using the fact that people were claiming that the author had died to understand the way that "author-hood" had altered its functioning in recent culture. And it is in that spirit of philosophically-motivated empirical research into the function of being an author that I am intrigued by the physicists' concerted efforts to define and shape the idea for a kind of writing that has no precedent.

At this point, the obvious needs to be restated: all groups are not alike. The high-energy physics collaboration functioned not at all like a collection of homogeneous agents of which one could be the spokesman just because of his typicality. Indeed, it was precisely because of the heterogeneity of the collaboration that the fundamental practical paradox of authorship arises. Each subgroup *is* necessary precisely because its special function is needed. If authorship means having contributed work that is a sine qua non for the result as a whole, then indeed each subgroup can and must be counted among the indispensable. But at the same time, when the question is asked: "Who did this work, that is, who is fully in command of this particular analysis and all on which it depends?"—the answer must always be deferred. It is entirely possible, even likely, that no one individual (much less a group of individuals) is entirely in control over the full spectrum of justificatory arguments that feed all the way down into the guts of the forward hadron calorimeter, the analysis code, and the calibration methods. Even that degree of instability is incomplete: with mobile agents and perimeters, transparent hierarchical levels of collaborations, even the collaborative pseudo-I, yield to the hive-I of open-ended coordination. The answer to "Who Is We?" in the context of a two-thousand-strong fluid collaboration must always remain unstable, oscillating between the desire to make scientific knowledge the issue of a single conscious mind and the desire to recognize justly the distributed character of the knowledge essential to any demonstration.

In a sense, every detail of these complex authorship protocols is part of a never-ending struggle to stabilize this instability and to reconcile these irreconcilable goals of centralization, distribution, and open-endedness. It may well be that experimental knowledge in the age of massive collaborations never comes back to a single center, but rather only to partially overlapping, complicated, inchoately bounded assemblages. Yet given the circumstance that the fates of individuals, groups, departments, and even national scientific efforts ride on the apportionment of credit, the attempt to localize authorship is not likely to end soon. On one level, then, the authorship struggle might be relegated to the special configuration of this sector of the physics community. My own suspicion, however, is that the conundrum of the massive collaborations now forming around particle physicists, astrophysicists, or theoretical biologists is not so atypical, after all. Rightly conceived, the tension between the felt need to condense scientific work to the single point of a pseudo-I and the recognition that knowledge is piecewise interconnected into a broad, blurred reservoir of expertise is not a parochial difficulty. It characterizes an unremovable instability in the securing of knowledge itself.

Notes

1. Allan M. Thorndike, Brookhaven National Laboratory. From *Bubble and Spark Chambers*, vol. 2, ed. Shutt (New York: Academic Press, 1967): 299–300. See Galison, *Image and Logic* (Chicago: University of Chicago Press, 1997), esp. chapters 1, 5, 7, 9.
2. "SLD Policy on Publications and Conference Presentations," 1 July 1988, 1.
3. Ibid.
4. Ibid., 3–4. Note that here, in contrast to the "invisible technicians" of the seventeenth century, the first papers were statutorily to include the names of engineers who had contributed substantially to the development of the apparatus.
5. DØ Executive Committee, "Criteria for Authorship of DØ Physics and Technical Papers," 14 March 1991. Hereafter, DØ 14 March 1991.
6. Cf. Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987), chap. 4.

7. Rolandi to Peter L. Galison, e-mail, 30 January 1995.
8. DØ 14 March 1991.
9. DØ, "Rules on Auhtorship of DZero Publications," 2 June 1994: "Retirement and Severance Benefits" allows authorship to be extended beyond departure for a period prorated at six months to a year service up to a maximum of three years. The "masthead" is defined as those members of the collaboration who are active along with those who are "quiescent" (coming aboard); the masthead and the authorship list are not coextensive. For example, someone who leaves may during his extended presence be an author but not on the masthead; someone coming on board may be on the masthead but not yet an author.
10. Dave Charlton and Peter Maettig, "Analysis: Basic Guidelines for OPAL Physics Notes and Papers. General Editorial Policy and Procedure," 10 January 1997, 3.
11. Ibid., 4.
12. ECFA 95/171, "ECFA Report on Sociology of Large Experiments."
13. "Report of the HEPAP Subpanel on Future Modes of Experiemental Research in High Energy Physics," July 1988 (DOE/ER: 0380), 50–51.
14. Roy Schwitters ["The Role of Big Science," *Chronicle of Higher Education*, 16 February 1996, B1–B3.]
15. "ATLAS Guidelines for the Publication of Scientific Notes," approved by the ATLAS Collaboration Board, 11 February 2000. My thanks to Bertrand Nicquevert for this information.
16. Ibid.
17. On mobile agents, see, e.g., David Kotz and Robert S. Gray, "Mobile Agents and the Future of the Internet," <http://www.cs.dartmouth.edu/~dfk/papers/kotz:future2/>, and the less optimistic view of David Reilly, "Mobile Agents–Process Migration and Its Implications," <wysiwyg://zoffsitebottom.12/http: //www.da. . .com/topics/software_agents/mobile_agents/>. The discussion of mobile agents in the context of large physical science collaborations is principally based on the GriPhyN project cited below along with the (massive) documentation independently fielded by the constituent collaborations (LIGO, SDSS, CMS, ATLAS).
18. For a discussion of GriPhyN, see <http://www.phys.ufl.edu/~avery/mre/proposal_final.pdf>.
19. Ibid., 4.
20. I have adapted the term *hive-I* from William Gibson's partially related notion of hive mind in his trilogy that began with *Neuromancer* (New York: Ace Books, 1994) and also from *multiplicity* in the work of Gilles Deleuze: "Multiplicity must not designate a combination of the many and the one, but rather an organisation belonging to the many as such, which has no need whatsoever of unity in order to form a system." From Deleuze, *Difference and Repetition*, trans. Paul Patton (New York: Columbia University Press, 1994), 182.
21. Michel Foucault, "What Is an Author," In *The Foucault Reader*, ed. Paul Rabinow. New York: Pantheon Books, 1984), 107.

22. Ibid., 108.
23. Ibid., 109. "A reversal occurred in the seventeenth or eighteenth century. Scientific discourses began to be received for themselves, in the anonymity of an established or always redemonstrable truth; their membership in a systematic ensemble, and not the reference to the individual who produced them, stood as their guarantee. The author function faded away, and the inventor's name served only to christen a theorem, proposition, particular effect, property, body, group of elements, or pathological syndrome."
24. Goldmann's intervention is in Foucault, *Dits et écrits: 1954–1988*, vol. 1 (Paris: Gallimard, 1994), 812–16.