DISCUSSION PAPER

• ABSTRACT

This paper suggests variation in the ways in which agreement concurrence, conformity or stability in science is brought about and the need to think not in terms of one model of consensus formation, but in terms of many. One variant which can be witnessed in scientific practice is exemplified in experimental high-energy physics (HEP); this variant relocates the problem in the early stages of an experiment when the technology is fixed, the groups participating in the work are selected, and the stage is set for results which - whether they are "negative" or "positive" - cannot be ignored by the relevant scientific field. The paper argues that consensus formation/stabilization is, at least at times, intricately connected to the social ontology of a domain. It proposes the "superorganism" metaphor to articulate the ontology of HEP experiments, and describes the form of change of these experiments as genealogical change.

How Superorganisms Change: Consensus Formation and the Social Ontology of High-Energy Physics Experiments

Karin Knorr-Cetina

Consensus versus Genealogy

Not long ago, we all knew what consensus formation was: it was the process through which scientists reached agreement about their propositional attitudes, the system of sentences through which they described the world. We also knew how this agreement was brought about: by and large through a process of rational assessment of a theory's performance with regard to the evidence, and with regard to prediction, explanation or inter-theoretical reduction. At least we thought that this is what ought to have happened in the history of science, given the apparent growth of knowledge. We did not count the occasional counterexamples,

considering them as violations of the norm by fallible human beings in a less than perfect world.

This view, of course, had much going for it, among other things the notion, reinforced every day through the publication of scientific papers, that language-like representations of the world are what counts in science, and the correlative assumption that the proper means for sorting representations is a rational, rule-governed conversation. Nevertheless, the view has also been challenged, from within the ranks of both philosophers of science and social scientists, by a more naturalistic strategy for understanding science. Philosophers interested in taking scientific theories out of 'Plato's heaven' switched to models of problem solving,¹ or went as far as to adopt an unabashedly reductive strategy for explaining how the biological brain functions, to answer the question of how cognitive creatures like us embody representations of the world, learn and produce explanations.² Social scientists who marvelled at the likelihood of a Habermasian ideal speech situation being instantiated in modern science turned to the empirical analysis of scientific practice and found no reason to fall back on the language of logical inference, propositional attitudes and rational beliefs when explaining science.³ If human history could not be explained in terms of the hidden operations of idealized rule systems, why should an historical human project, the scientific study of nature?

On the social science side, the move towards a performance conception of knowledge turned up a number of results which contradicted the old model of consensus formation.⁴ It also yielded a rich picture of epistemic practice which suggests, with respect to the old model, a number of revisions. First it suggests *variation* in the ways agreement, concurrence, conformity or stability in science is brought about, and the need to think, not in terms of *one* model and vocabulary of consensus formation but in terms of many. Second it suggests that the *locus* of consensus formation or stabilization can shift, for example, from the end of a series of experiments to their beginning, and from explicit opinion formation over results to the implicit incorporation of selections into new efforts. Third it suggests that consensus formation/stabilization is, at least at times, intricately connected to the *social ontology* of a domain - or, phrased differently, it can be seen as part of its articulation.

Consider, first, variation and the shifting locus of consensus formation. Against the view that consensus formation happens

through a process in which propositional or sentential claims are contested among relevant scientists, I shall maintain that this is only one variant among others of equal or comparable importance. Other variants which can be witnessed in scientific practice include the phenomenon of '*instantaneous agreement*', where those confronted with a new piece of research seem to be trapped into its immediate acceptance and inclusion into research, perhaps even against their wishes. Instant agreement can presumably be accounted for in terms of cultural preconditions and preferences which make it likely for a research result to pass into acceptability and use without much ado. What interests me about it is precisely the relocation of the problem of 'consensus' into stages which precede or bypass explicit consensus formation: to examine this kind of agreement, one has to study how certain presuppositions which make it possible for the new element to be immediately accepted become set and entrenched in a cultural domain.

In this paper, I am not concerned with instant agreement itself but with another variant which also relocates the problem into earlier stages: I am concerned with a particular form of temporal organization of agreement formation, exemplified in experimental high-energy physics (HEP), which I call *genealogical*. Genealogical change involves distinctive 'generations' of efforts organized into overlapping sequences. It also involves a shift from 'consensus formation' to *selection* through processes of reconfiguration which establish these efforts and reorganize the field.

To explain what I have in mind several things have to be done. First, we need to consider the organization of a field that moves genealogically - its configuration in terms of long-term supra-individual entities which are called 'experiments', and which seed new generations of experiment. In other words, we consider HEP's 'superorganism' ontology - its organization in terms of 'mobile corporations', which are neither laboratories nor organizations. Second, we need to become aware of the importance, for 'consensus formation', of the beginning or 'birth' stage of these entities. The most heavy selections in the field seem to be made at the birth of new collaborations, where they become incorporated into equipment design and data generation. Third, we have to consider how the story of superorganisms and the story of genealogical time and transformations is linked, in experimental HEP, through procedural tactics that play down individual social actors, and paper over their conflicts and competition. These tactics institute

the individual not, as in other fields, as an author and speaker for him- or herself and his or her work, but as a spokesperson for the whole. But they also continually assemble the whole by creating simultaneous profiles of experimental and field efforts: within and across experiments, HEP continually 'integrates over' what it does; it sums up states of the equipment, displays the progress of measurements and simulations and articulates the methods and technologies other experiments use. This integration over itself is a method of 'consensus formation': it provides for the possibility of comparison, and makes available a 'sense' of where things move and what it pays to pursue without explicit decision-making.⁵ But integration is also a strategy of *unfolding the objects* in the technical plots of the experimental story lines which are brought together and summed over. These objects (detector parts, particle distributions, and so on) are articulated in all relevant respects; their properties become named and identified in what is the equivalent of a physicist's thick description. The ethnotactics of integration come to fruition in experiments' reproduction of themselves, in the birth and incorporation of new experiments.

First I outline the social ontology in terms of which experimental HEP is organized. Next, I sketch the practice of assembling the whole and of finding a course of action through the reflexive unfolding of technical objects. And I then address how these tactics succeed in construing the next generation of an experiment, by simultaneously sorting out technical options and retaining the people which the experiment needs.

The Social Ontology of Experiments in High-Energy Physics

The social ontology approach leads to an interest in life's structural reconfigurations - those processes through which entities and their relationships are continually redefined and fixed within forms of order which differ from, and play upon, other forms of order.⁶ The ontology I explore in this paper pertains to the huge experiments in high-energy physics which can now string together collaborations of 15000 physicists and last up to twenty years, the better part of the working life-time of a member. To characterize these aggregates, I shall deploy a metaphor that comes out of biology, but has not to my knowledge been used in sociology - the

metaphor of a 'superorganism'. Biological analogies have gone out of fashion in sociology, together with functionalism, so if one wants to draw upon them one has some explaining to do. I want to begin by a denial: a superorganism is not an organism - that is, a system functioning in terms of a metabolism sustained by a fixed set of organs. I am not proposing that we revive the organismic analogies in which Durkheim still believed;⁷ although, as an interesting aside, we should note that when sociologists likened societies to organisms, biologists likened organisms to societies, and indulged in thinking of them as collectives which displayed rich internal interactions. However, the term 'superorganism' has a more straightforward connection to the term 'society'. In its long history in biological thought it has always been applied to supra-individual life-forms: the paradigm for superorganisms has for centuries been colonies of social insects such as ants, bees or termites.

Biologists, of course, have long been intrigued with the life of these organisms within thoroughly unified societies, and puzzled by the question of how in the evolution of life the transition to the social as a 'higher level of biological organization' was achieved. What interests them at present is whether superorganisms can rightly be regarded as supra-individual units of selection, or whether all adaptation must exclusively be explained at the individual or gene level.⁸ Such matters make irritating inroads into sociology when they are taken up by sociobiologists, but they are of no relevance to the present discussion: we can write them off as a disanalogy, one among many which affect comparisons between human and non-human life.⁹ I choose the notion of a superorganism for different reasons. First, insect colonies function as integrated, moving wholes which reproduce through colony fission and migrate in response to food scarcity, overcrowding, and the like. Somewhat analogously, HEP collaborations can be considered as *movable, semi-detached corporations* located somewhere between a social movement and an organization in the vocabulary of social categories, but identical with neither. The term 'superorganism' can draw attention to these mobile incorporated entities for which we have no good name in sociology, but which nevertheless exist. HEP collaborations are organized efforts, but they are unlike bureaucratic or other organizations in that they are not only spatially localized but also temporally restricted: they are formed in order to conduct specific research tasks; after these are completed, they disband and evolve into new collaborations. Collaborations spring

up and thrive within the big laboratories such as CERN or Fermilab, but they are also at least semi-autonomous units. They are formed from independent physics institutes located worldwide which join their resources (money, manpower, skills, and so on) with those of CERN to conduct an experiment.¹⁰ Collaborations, one might say, fasten upon big laboratories, hitch-hiking on their resources and facilities: they are the cargo big colliders carry in a mutualistic rather than unitary, intra-organizational arrangement. Unlike organizations, HEP collaborations until now also have no legal framework that binds them together and would allow them to bring action against malfunctioning members: participation in a collaboration is based on informal agreement, which may be spelled out in a letter or document of 'understanding' without legal consequences. Nor are collaborations simply 'networks' of independent agencies which do their own work; to achieve results in collider experiments, subunits need to join and transform themselves into a collective epistemic subject. Finally, these big collaborations are not, in my opinion, 'social movements' in the sense in which we speak of the 'environmental movement', the peace movement, and the like: they are too tightly knit together, task oriented and bounded.

A second aspect of the notion of a superorganism in biology is that it draws attention to the *pervasive cooperation* in a unified society which biologists see, in today's jargon, as a vehicle for gene survival. Pervasive cooperation is also a feature of collaboration in physics; it is a form of hypersociality which outlives and outperforms the usual interactional and structural mechanisms that govern social institutions. Sociality is mobilized so successfully in HEP experiments that it is hard to notice: the categories used to describe the relational issues, the social problems, the interactional difficulties apparent in other social formations appear no longer relevant - not because the social has died, as Baudrillard would have it,¹¹ but because for once it has been *made to work* - and made to work without much help by way of hierarchical structures and formal organization, without external supervision and hard set internal rules, without many of the conflicts, difficulties and inefficiencies present in large-scale industrial organizations, even without much of an 'informal organization'.¹² Pervasive cooperation appears as the successful implementation of a communitarian form of life - the kind communist regimes have failed to bring about in other places in this century. Yet it is an unromantic kind of communitarianism which has little to do with the ideals of common

bonds, rootedness, fraternity or group identity that are often invoked to characterize communal life-forms.¹³ Some of the mechanisms which transform a conjunction of people into a communal collective are quite acid. For example, they involve the erasure of the individual epistemic subject, a structure around which knowledge creation turns in many other sciences. I shall return later in this paper to some of the mechanisms through which the transition from an individualist or interactionist pattern to the 'hypersocial' arrangements of physics is achieved.

A third aspect to which the notion of a superorganism can perhaps draw attention is the presence, within the communal life-form of physics, of a central and centring object, the several storey-high detector which physicists build.¹⁴ An HEP experiment, in native terminology, is not just the human element of large numbers of cooperating physicists. For this, the native term is 'collaboration', which I have used before. An 'experiment', however, also includes the detector: it *is* the detector *in conjunction with* the collaboration. Displays of the progress of the measurements, for example, will make this apparent by referring not only to the people who conducted the work (for example, by authorship) but to machines and the names of experiments (see Figure

What wins prizes for the physicist, according to Roy Schwitters (the director of the Superconducting Supercollider that was to be built in Texas), is the equipment. While the physicists remain clearly distinguished from these machines, through terms such as collaboration they are also continually matched with them. A physicist's identity, expertise, reputation and social status all pay attention to his or her conjunction with the equipment. While this appears to be the case to some degree in all technical work, it is more so in HEP: within a hierarchy of timings, the detector and all of its needs and parts function as a pacemaker and coordinator of scientists' activities. Coordination arises in the experiment without centralized decision-making, without a centralized control hierarchy, with some individuals taking in information about the experiment, deciding what needs to be done, and issuing commands to other individuals who then perform the tasks. The experiment is a nested hierarchy of lower-level units in which the relationship between physicists and the technology is intense, particular and detailed. Coordination arises from these deep-felt relationships, though it also involves other vehicles, to which I shall return.

FIGURE 1
Exhibit Showing Results from the Experiments UA2 at CERN and CDF at Fermilab

Inclusive jet production cross-section around 90° , as measured by UA2 and CDF. The curves are leading-order QCD predictions.

Source: L. diLella, 'Hadron Colliders: Past, Present and Future' (CERN-PPE/90-160, 7 November 1990). The plot was used by Carlo Rubbia in his opening talk at the ECFA Large Hadron Collider Workshop: see 'A Few Considerations of Strategy on the Future of CERN', in European Committee of Future Accelerators (ed.), *Proceedings of the ECFA Large Hadron Collider Workshop*, Aachen, 1990, Vol. 1, 9-18.

Let me conclude this section with a dñwanalogy rather than an analogy that affects superorganism ontologies in social life. This

disanalogy stems from the additional regimes in which HEP's human members are involved, but it does not detract, as I hope to show, from the usefulness of the superorganism concept in connection with HEP experiments. Among species, biologists believe, superorganism ontologies are brought on by hard-wired, evolutionary transitions which leave the individual in the lurch: the members of a colony of honey-bees, for example, cannot survive on their own, even though they are physically independent organisms which closely resemble the solitary bees from which they evolved.¹⁵ Physicists, however, are not only physically independent entities but also socially independent: not only is it possible for them to survive without HEP experiments, they may also be able to make a better living, as physicists, in the more solitary physics areas in academia, industry, government, and even in teaching. Physicists have alternatives; honey-bees do not. Moreover, even while they are committed to an HEP experiment, physicists have other lives as individuals, in other contexts (for example as doctoral students or teachers at universities). Unlike honey-bees, physicists, like human beings frequently, act as transiting, locality- and ontology-breaking creatures, stepping from one regime of being into others.¹⁶

When they switch, they carry the residues of their other frameworks with them. In HEP experiments, this means that the order of 'organisms' (individual physicists) leaks into the order of the superorganism, commands attention by it and influences the conditions under which it operates. For example, the experiment, in providing this attention, 'takes care' of individual physicists' career needs (it finds dissertation topics for them, provides public exposure, helps them to get positions) in a pastoral attitude which extends the culture of the care of the self that it exhibits elsewhere.¹⁷ The theoretical point of interest is that the order of the whole is not identical to the order of the 'components'; it neither subsumes it, nor is it simply aggregated from it. The relationship is that of one environment to another: individual subjects with their biographical involvements and career needs constitute an environment for the business of HEP experiments.¹⁸ Equally, physics institutes, of which collaborations are officially composed, and the regimes under which they operate, constitute such an environment.¹⁹ Thus, several orders and ontologies usually trespass into a social universe, where they *edge* against each other, create disturbance effects and conflicting evidence, and demand some

sort of alignment.²⁰ But like any environment, they also provide resources in a mutualistic arrangement that must be carefully kept in a mutualistic state. I cannot say any more here about the make-up of these interactions. However, I want to illustrate how different ontologies organize activities differently by giving an example from the superorganism mode of being.

My example refers to the use of time. Time plays many roles in HEP experiments,²¹ of which I want to point out one: the superorganism ontology involves *genealogical time*, a temporality which transcends individual experiments with a view to possible further generations of detectors and colliders. Collaborations have a certain extension in time during which they form, perform their work and disband. But in doing so they also seed new collaborations, the ones which succeed them and which sometimes spring into shape even before an old collaboration disbands. Collaborations think²² in terms of the next generation of experiments which provide opportunities for participation, and which they help create. This thinking is manifest in the official decision-making procedures an experiment adopts. These procedures function as a ritual display of the *habitus* of a collaboration - as a signifier that displays 'the character' or 'quality' of a collaboration (for example, its 'democratic' nature), to which it attracts new members. Another feature of genealogical time is that it entices participants in the collaboration to virtue: awareness of the need to collaborate over long *durees* and in ensuing generations of experiments incites and upholds *trust* as a glue that brings participants together and keeps them functioning within pathways of informal confidence. The advantage of trust as a mechanism is, of course, that those under its spell self-regulate their behaviour so that it becomes worthy of trust. Trust makes the more discourteous social techniques such as supervision and control unnecessary.

Assembling the Field While Unfolding the Object: Ethnomethods of Consensus Formation

Let me now return to the thesis of this paper, which is two-fold. First, I suggested that 'consensus formation' should be seen more in terms of the use made of scientific results in experiment creation, as opposed to a process of detached opinion formation on the value of scientific results. Second, I suggested that forms of con-

sensus formation may be dependent upon the social ontology of the domain in which they apply. The communal ontology of HEP experiments underwrites forms of agreement-fixation different from the more solitary ontologies found in theoretical physics, or the small-group and network regimes of bench work sciences. If 'consensus' does indeed flow at least in part from the selections caught and embodied in scientific practice, then how this practice is conducted should matter to the format of consensus formation.

In this section, I try to establish the features of some techniques whose massive deployment inside and outside HEP experiments yields, in my opinion, this field's predominant mode of genealogical change, a change through distinctive state transformations. Experimental HEP projects a picture of stepwise integration which I interpret not as an incidence of a more general conception (opposed to Kuhn's) of how the sciences develop, but as a specific outgrowth of two of this field's significant features. The first is its continual *self-assemblage* or, if you wish, its summing over its own recent histories and projected futures. Experimental HEP has a constant tendency to integrate over its own recent lines of work, of summing up its features, and of exhibiting, to itself, the *simultaneous profiles* of its ongoing technological and 'physics' stories. It is not difficult to ascertain the state of an experiment or the state of the field in experimental HEP. HEP, unlike other fields, makes these states explicit in a continuous stream of discourse which serves as the vehicle of its own integration. But experimental HEP not only creates profiles of the past: it also creates profiles of its own future, of future experiments, as we have seen before, and of future accelerators, future technology, future physics calculations and questions. Experimental HEP appears to live in an extended present composed of the simultaneous profiles of its own contemporary history and projected future. It arranges itself around the moving contours of these profiles, nudged on by the story lines of physicists' interactions with technical objects. As we shall see, these story lines unfold the objects of research in the plots of their technical dramas. But they also unfold and display to the audience the groups and physicists who are part of the stories: their financial, technical and expert *pouvoir faire*, their power to survive in the story, the kind of character they prove themselves to be. Both technical objects and physicists dangle from the 'story strings' which appear to be what moves forward in the field. Selection comes into the picture of story strings as decision

'emergence', as unfolding culminating in the 'obvious', 'reasonable' or unavoidable thing to do. What helps in these selections is the play of stories upon themselves - for example, of future stories upon contemporary histories. The future is captured in the former and made to select the stories of the past to be continued.

I now provide more details for the rough sketch I have given of the field's self-assemblage and 'sums-over-histories' approach. First, there is in HEP experiments the massive presence of relevant discourse, within the experiments themselves. Second, the discourse format for story summation is the (status) report. Third, reports make available simultaneous profiles of the state of affairs within different story lines, and the field assembles itself through similar summations, instituted in experimental HEP meetings and conferences. Fourth, story summations in the experiment and in the field provide for the continued unfolding of technical objects and physicists through which decisions 'emerge', rather than have to be 'taken'. Fifth, unfolding extends into the future and captures the behaviour of unmeasured physics processes, unbuilt detectors and undeveloped technologies.

The Collective Consciousness of HEP Experiments

Consider the first point. An HEP experiment is marked by the massive presence of discourse within an astonishingly intricate web of communication pathways: it is marked by a constant humming of the experiment to itself, about itself. The discourse and communication is made up of threads of talk, e-mails, meeting presentations and transparency exchanges. Two features are striking. First, talk is constantly activated at a variety of occasions; there exist, one might say, numerous created and proliferating discourse spaces, localities and time slots set aside for talk. Second, the form of talk most prominent at these occasions is that of the report.

Discourse proliferates in HEP experiments in a variety of spaces, many formal but many also informally arranged or simply situational. Almost any occasion, it seems, at which one participant in the experiment finds him- or herself in the presence of another generates talk, including jogging in the vineyards and fields which surround CERN, or the bus ride to town. A range of occasions centres around 'the cafeteria' at which participants tend to have lunch in small groups, but which also provides for 'coffee

breaks', 'drinks' and dinner between 7 am in the morning and 1 am at night. Another range of occasions centres around apparatus and equipment - for example, the testbeam area, the 'pit' (the hole in the ground in which the detector intersects the collider), or smaller testlabs. The more 'arranged' discourse spaces are those provided by numerous 'meetings' - for example, R&D group meetings, working group meetings, detector meetings, panel meetings, institute meetings, collaboration meetings, referee meetings, accelerator meetings, fixed committee meetings, submeetings of some of the former and the very important 'meetings after the meeting' (for example, in the office). Experiments are mapped into a fine *grid of discourse spaces* created by *intersections between participants*. Its existence is perhaps the most important vehicle of experimental coordination and integration.

What happens at the intersections? A number of things, of course, but prominent among them is a sort of narrating and accounting encapsulated in a form of talk, the report. The most notable variant of this is the 'status report' which comprises the vast majority of all talk given at meetings. The status report is a summary of what a group (or a person drawing upon others) has been up to and has experienced in its dealings with equipment, data sets, physics calculations and the like. It is a summarized history of physicists' meeting the object, with the added twist that it details, through transparencies, its exact reactions and characteristics. The importance of the report derives from what it unfolds: reports are called upon to exhibit all relevant wrinkles of a problem, and to convey the message that all relevant wrinkles have been aptly identified and investigated. Informal talk also prominently consists of, or contains, reports, though they may be more fragmented, interactively elicited, and need not involve transparencies or some of the other paraphernalia of meeting presentations.

The story profiles articulated in reports provide the experiment with a sort of 'consciousness': an uninterrupted hum of *self-knowledge* in which all efforts are anchored, and from which new lines of work take their leads. Superorganism-likeness may rule out the individual epistemic subject as a procurer of knowledge. But it does not rule out all traits of subjectivity; in particular, it does not rule out self-knowledge, as a form of meta-level representation of the states of the experiment. This self-knowledge, of course, is collective and dispersed. For those who still remember

Durkheim,²³ it is a version of his much rebuffed 'conscience collective' - not rooted, however, in social likeness or common sentiment, but in something as mundane as webs of discourse spaces and forms of talk. This 'conscience collective' also implicates a moral order - it sustains the moral obligation on the part of participants to act in accordance with an experiment's self-knowledge, and the desire to live up to (and base one's career on the fulfilment of) collectively conscious expectations.

Assembling the Field

Turn now to the field side of things. The locus of discourse and communication in HEP as a field, beyond experiments, is scientific meetings. The defining characteristic of international scientific meetings in experimental HEP is that they feature only talks which are *summaries* of many people's work, while possibly excluding the work of the presenter. Two kinds of talks are commonly distinguished: '*rapporteur talks*', which sum up several running experiments or other efforts in a field; and '*experimental talks*', which sum up results of just one experiment or one collaboration, to which many people have contributed. Some conferences specialize in the latter, like the annual conferences in La Thuile and Moriond, or the bi-annual $p\bar{p}$ Workshop. Others feature more rapporteur talks, like the European and American HEP conferences organized by the European and American physical societies. Both kinds of summary talks use presenters as *representatives* and spokesmen for the experiment, the technology or the physics mechanisms they talk about, but not for themselves as authors of papers. An experimental talk will be given by a member of the collaboration that conducted the experiment, but the presenter *need not have participated directly in the work he or she reports on*. Announcements of conferences are usually made in collaboration (plenary) meetings, in which volunteers interested in presenting are asked to come forward. Whether a physicist is selected (or comes forward) depends not primarily on whether she or he worked on the topic, but on how many if any opportunities the person has already had to present a paper, on whether, at any given time, he or she needs 'exposure' at important conferences with a view to securing a job, whether the conference is held in the person's home country, and so on. Listen to the announcement of such conferences during one

of the last collaboration meetings in 1991 of the experiment UA2 at CERN:

Okay, now let's discuss conferences . . . We announced last time this Aspen meeting where they wanted a talk on direct photons in UA2. Well F has finally agreed that she would go. Okay, for the next year, we have a few more [conferences], and in fact, what I've done. *I have to remind you people who have given talks in the past four years, '87, '88, '89 and '90. This is the list of speakers* so that you can yourself establish priorities in choosing speakers for the next time [passes out list] . . . (emphasis added).

Papers are collaborative products to which many people have usually contributed figures and graphs, if not text. Important talks may be rehearsed in plenary meetings before they are given in public, with the final version something that is not only collaboratively produced, but sanctioned. Rapporteur talks go further than experimental talks in that they 'report' on the aims and achievements (materials, performance, time scales, cost, and so on) of several experiments at one machine (for example, on those planned for the SSC in Texas), or on physics processes (for instance, on physics 'beyond the standard model') at different machines (for example, the SSC and the LHC at CERN), or on detector technologies in the field ('optical detectors', 'semiconductor detectors'), and so on. They sum up the field in overlapping profiles, creating a grid of conscious knowledge which physicists seek out when they go to meetings:

[We go to meetings] because summaries of the newest developments are presented there, summaries which it would be very painful to make oneself since one would have to read every paper, while at meetings one gets a good overview if one has good [rapporteurs]. This is really the main point.²⁴

Whole conferences may exclusively be constituted by rapporteur summaries, and summaries of summaries, like the 'Large Hadron Collider Workshop' organized by the European Committee for Future Accelerators. Figure 2 gives an overview of the plenary session of one of these meetings. In this meeting, reflexive summaries of work achieved in working groups before the meeting were presented in parallel sessions. The working group summaries were then summarized by designated rapporteurs in a second-order process of reflexivity for the plenary session, where they were joined with other second- or third-order summary talks presented with reference to big machines.

FIGURE 2

Conference Talks at the Large Hadron Collider Workshop, Aachen, 4-9 October 1990, Including Handwritten Changes by Participants

EUROPEAN COMMITTEE FOR FUTURE ACCELERATORS

LARGE HADRON COLLIDER WORKSHOP

Aachen, 4-9 October 1990

available transparency copies

PLENARY SESSIONS

Sunday, October 7th:

08.00 Opening Addresses

Chairmen: G. Flüge
 K. Heidecke, RWTH
 H. Stenzen, MGB
 J. Reibhaar, BFT
 C. Rüchla, CERN
 G. Brianti, CERN

not available

08.15 Introduction

08.45 The LHC Project

BREAK

11.00 SSC Status and Physics Programme

11.30 RHIC Status and Physics Programme

12.00 HERA Status and Physics Programme

Chairmen: K. Nürber
 F. Günter, SSC
 T. Ludlum, BNL
 P. Söding, DESY

LUNCH

Summaries of physics working groups

14.15 p-p: Standard Model Physics in pp Collisions
 15.15 p-p: Beyond the Standard Model in pp Collis.

Chairmen: C. Quigg
 D. Drevogl, Saclay
 F. Pauss, CERN

BREAK

16.45 p-p: pp Physics at LHC

17.45 A-A: Heavy Ion Physics at Very High Energies

18.45 A-A: Heavy Ion Experiments at LHC

Chairmen: P. Klügel
 G. Altarelli, CERN
 H. Satz, Stiefelhof/CERN
 H. Spocht, Heidelberg

21.00 Organ Concert

Large Hadron Collider Workshop, Aachen, 4-9 October 1990

Monday, October 8th

Summaries of physics working groups

08.30 e-p: Experimentation and Detectors
 08.50 e-p: Physics
 08.40 e-p: Higgs Search
 10.00 SSC: Experimental Programme

Chairmen: J. Dowell
 J. Feltesse, Saclay
 R. Rückl, München
 D. Zepfelfeld, Wisconsin
 F. Günter, SSC

BREAK

11.00 HERA: Detectors

Chairmen: G. Bellardini
 R. Marshall, RAL

Summaries of instrumentation working groups

11.30 D 9: Interactions Regions

12.00 D 8: Radiation

LUNCH

14.30 D 1: Detector Simulation

15.30 D 3-4: Vertex and Tracking

Chairmen: M. Berner
 R. Brun, CERN
 F. Camerini, CERN
 D. Saxen, Glasgow

BREAK

17.00 D 5: Calorimetry

18.00 D 6: Electron Identification

Chairmen: E. Tassi
 J. Colas, LAPP
 T. Åkesson, Lund

Tuesday, October 8th

Summaries of instrumentation working groups

08.30 D 7: Muon Identification

08.30 D 2: Data Acquisition

Chairmen: M. Bourquin
 K. Eggert, CERN
 N. Ellis, Birmingham
 L. Mepalli, CERN

BREAK

11.00 High-Speed Links and LHC-maintenance

11.30 SC Accelerators and Detector Magnets

12.00 Summary

12.45 Conclusion

Chairmen: K. Tittel
 G. Rüchla, Rhein-
 D. Kriesche, Interatom
 G. Kalinowski, RAL
 J. E. Augustin, Orsay

Conference invitation policy matches the summatological and reflexive nature of meetings. Invitations for experimental talks go to experiments and their spokespersons, not to individual physicists. The experiment then selects a representative *and* a topic, which may also be suggested by the conference organizers. Personal invitations to particular physicists recruit the physicist as an expert representative of technological or physics processes; invited speakers are invited as rapporteurs, not as authors of their own work.²⁵ Higher status in the field confers greater likelihood of such invitations (and acting as a rapporteur confers greater status). Phrased differently, higher status means going from a rapporteur for an experiment to a rapporteur for the field, or from first-order reflexivity to second-order reflexivity.

Unfolding the Objects (and Subjects)

Self-knowledge articulated through reports and field summations combines with a second tactic, the tactic of an avoidance and deferral of decision-making as a means of selecting the relevant options and of regulating work. 'Decision-making', of course, is an ambiguous term with multiple referents. It refers to those emphatic occasions at which we must come to a conclusion with regard to a choice of alternatives in a situation of uncertainty, or where opinions differ. But we also sometimes dub 'decision-making' the process through which a course of action emerges 'naturally' from a previous course of affairs. What HEP experiments avoid is the former process. In fact, when 'decision-making' becomes an item on the agenda this often means that a course of action which is already agreed upon needs to be made plausible to, and sanctioned by, a group. Explicit 'decision-making' is often a ritual of approval. What happens much more routinely is that the system becomes nudged into a particular direction in a process in which decisions are being left to *emerge* as 'the obvious', 'the reasonable' or the unavoidable thing to do.

How are decisions made to 'emerge'? The answer is through the process of *unfolding of the object* mentioned before, through the constant unravelling of the features of technical objects, of their details, composition, hidden sequences and behaviour brackets. Let me illustrate this by an anecdote. In 1989, the head of online operations became worried about the major online computer in

FIGURE 3
Imagined Plot of the Uptime of a Computer over Time

the experiment, the device which wrote incoming data to tape. The computer was old. Would it make it fine through the next data run, or should it be replaced sooner? What the physicist wanted was a curve that unfolded the deterioration of the device:

I tried to get some data out of Digital, the firm making these computers, by asking them, since it's an old computer of the kind of which they build 100 they could make a statistical analysis and see what is the uptime [the percentage of time running] of the computer after 1 year, after 2 years, after 10 years, after 15 years.

What the physicist imagined is a graph like that in Figure 3. Suppose in this graph that the probability of uptime decreases as displayed each year by 10% during the middle age of a computer, then drops off more sharply, and finally flattens out in a tail in old age. Would that make it any clearer at which of the yearly brackets during the middle age decrease in efficiency one should get rid of the device, or whether one should wait until the decrease had flattened off? 'No', in the sense that one would still have to decide on the acceptability of a 50%, 60%, 70% or 80% uptime. But 'yes', in many other ways. First, a steady decrease of performance by 10%

per year up to a certain age is itself important information which might prompt one to make the cut at the point where the curve begins to drop off more sharply. Second, the percentage given per year of age can be used to make further calculations of expected data losses through downtime, subsequent error increases and reductions in the probability of finding the result one is looking for - all information for which one might have clearer acceptability standards. Third, the exact probabilities make available time points at which additional considerations like the expense of a new computer versus the expense of repair costs, or the training time involved in the switch, and so on, can take hold and decide between, for example, a 70%, 80% or higher percentage of uptime. And so on. Thus, the use of unfolding is not that it eliminates any difficulty but that it *unlocks possibilities for further unfolding* and provides entry points for the insertion of other information. The ethnomethod of unfolding solves the problem through 'unsticking' the system: confronted with an 'impossible' decision, it allows us to add other loops and to extend and re-deploy its procedures. To return to the language of consciousness one could say that decisions emerge through processing becoming reflexive; and through the paradoxical pathway of avoiding making the decisions which need to be made.

Back to the Future

The technical plots unfolded in experiments and at international scientific meetings narrate not only the past but also the future: the story lines of talks frequently include, or are entirely made up of, simulated objects and processes which are expected to occur in an expected instrumental environment. The past of the sums-over-histories approach is extended into the future, in a process of 'appresentation' (to use Schiitz's term) through which the future is explored, mapped, measured and calculated, and as fully as possible anticipated. In the beginning of an experiment, one proceeds by simulating the *experiment within the experiment*: before the experiment is built, one attempts to give a full simulation of the physics one will encounter, of the detector(s) one intends to build, of the errors which are likely and of the difficulties one can anticipate. In fact it is not only one experiment within the experiment but several partial versions which are run

through the computer in several years of work, and unfold future simulated worlds. Unfolding of the past continues into the unknown, and renders it witnessable, arguable and manageable.

Reflexivity turns forward, then, when the experiment fictionally anticipates itself in partial, full and finessed variants whose actual construction, if they were built, would take many years. The future is captured in these simulation stories, and then slowly caught up with through being fed into the ongoing stories of actual instrument construction and use. The future is also organized, formally through 'committees' (like the European and International Committees of Future Accelerators) which coordinate meetings and groups working towards 'the next generation' of particle rings, and informally through physicists 'thinking about' and participating in efforts which 'jump' the present generation of equipment and physics. Anticipations of the next machine, the next detector technology, usually run in parallel to the installation and conduct of a present experiment, and sometimes look ahead even to the next after-the-next generation. Earlier, I argued that the genealogical time which drives such anticipations influences the present experimental social order, through enticing participants into trustworthy behaviour, and experiments into trustworthy ('attractive' in the sense of member-attracting) forms of organization. What is of interest here is that projected futures drawn into an experiment constitute its selection environment: the unfolding of the future interacts with the unfolding of the recent past, suggesting courses of events that attempt to 'trap' the outcomes one desires.

Looking now at the whole process, we can see that discourse (in the form both of formal status reports presented at meetings and of informal accounts) continually sums up experimental activities, giving them simultaneous profiles and states. The transformations of these states follow, most importantly, from unlocking the details of experimental components: from a process in which selections are made to emerge through reflexive reapplications of the unfolding procedure. The unfolding procedure works equally for objects (for example, pieces of technology) and for the social groups associated with objects - for example, for those subgroups of an experiment which, in the beginning of an experiment, favour one subdetector over another. In such a process a group loses out when it cannot muster the manpower and resources to compete in unfolding its technology with another group. But while the group's technology may be eliminated from consideration, its members,

who are needed in large collaborations, are not: since the inferiority of the group's approach 'emerged' from a *contest in unfoldings*, it becomes acceptable for participants to stay with the experiment and to take on, with others, a different approach.

The Birth of a Superorganism

If the future is built into an experiment, if it is slowly caught up with through the implementation and running of an experiment, what remains from our original problem of consensus formation? Is there a future beyond what has been anticipated? There certainly is, as something eyed from a distance and hoped for, but also as something built into an experiment through, for example, 'surprise' triggers which set the threshold for data-taking at levels at which the unanticipated 'just might turn up'. If they turn up, such results are more likely to be contested than expected results would be. Yet contestation of results in experimental HEP appears to be rare - rare not just because surprise results are unlikely, but because 'consensus' has been relocated into the beginning of an experiment, when the technology is still unsettled, the physics processes within reach unclear, and the protoexperiment is still in a fluid, unincorporated state. It is important to stress that the results of a working experiment in HEP appear not only to be published without grave objections by referees: they cannot be ignored. Even when experiments are 'unsuccessful' in terms of finding the particles or identifying the physics processes they looked for, publication and recognition seem to be assured. For its results to be recognized *and taken into account in future research*, it is sufficient that an experiment establishes itself as an experiment 'that works' - which means as an effort whose technology produces the data envisaged, and whose analysis tactics are standard or can be followed. Not every experiment can be made to perform in this sense: sometimes detector technologies do not work out, or colliders and accelerators do not provide the energy and luminosity they promised. But for experiments that work, much of the problem of 'consensus formation' seems to be relegated to an earlier stage, to the stage in which, and the question how, experiments establish themselves.

Experiments establish themselves through the long processes

of equipment construction and installation, of calibration, data-taking, data analysis, and so on. But they also establish themselves, succinctly, during the period of several years during which they and the collaboration are formed, up to the point at which the experiment is approved, and financed. Phrased differently, experiments establish themselves during a period of *configuration* and incorporation, when the technology to be chosen, together with the physics to be explored and the groups which participate in a collaboration, become selected. Participants refer to this stage as 'the birth of a new collaboration'. They refer to it, appropriately, in genealogical terms. They also appropriately emphasize the human element, the bringing together of groups with different technological investments.

My point is that much of the script for the unfolding technical drama of an experiment whose results will be watched by the rest of the field is written during the period of collaboration forming, and groups and their interests play an important role at that stage. In this process, a delicate 'negotiation' is performed by groups whose technology competes with others, and by protocollaborations and experiments which compete with each other for final approval. Groups have investments in the technology they have worked with and develop; experiments need the groups (their manpower and the money they bring), but must choose between technological options. When a new collider is being built, groups have the option to join different experimental proposals which form at about the same time, but they also do not want 'to miss the boat' (come too late to be of interest to an experiment), and they want to be with the 'flagship', or at least with 'successful', proposals. Those involved in emerging collaborations have loyalties to the groups they have worked with in the past, but they must not alienate new groups whom (since experiments get larger all the time) they need. Groups have loyalties to their old collaborations, but may wish to choose collaboration-forming for a switch - which is risky, because they may have to enter 'cliques' of participants who have known each other for a long time, and can use mutual communication and knowledge as power against them. In this situation personal offence is always just around the corner, political costs and uncertainty are enormous (for instance, offending a group can mean offending the country from which it comes; making the wrong move may mean risking experimental approval),

strategy is rampant, friendships are broken and coalitions are formed and reformed. Actors emerge from the communitarian life-form as individuals who are outspoken in these games, or outstanding in its practice.

However, it is not obvious that the most strategic actors are the most successful, or even that much can be gained from what participants themselves call the 'politics' of this stage. In fact, one sees notorious political actors being tamed into the superorganism life-form, and learning to cooperate with it. The experiment *papers* over political strategy by the public summation of the knowledge everybody creates, and by its unfolding method. It does not eliminate 'politics', but it mutes it, waits it out, attacks it in the rear, lies in ambush for it, or delegates it to external agents. The script of the machine to be built and of the results to be gained from it is written not only through some actors winning out, but through different forms of order and practice playing against each other. One has to do with the components of an experiment, the groups and individual physicists who have enormous stakes and interests in these matters: the other is the order of the emerging superorganism, in which the goals and means are set, the stakes are aligned and what remains to be done is 'working things out' (not a trivial matter!). Both forms of order involve technology as well as groups, and technical questions which implicate social resources and tactics. If you opt for a particular detector technology in the electromagnetic part of the calorimeter, you are ruling out other technologies and the groups which have an investment in them. Within the regime of individual and group components of the experiment this is done by groups trying to outmanoeuvre each other. Within the communal life-form it is done by letting the unfolding tactics do their work. As suggested before, through repeated unfolding within a communal awareness and knowledge, technologies can be displayed as inferior, and this can lead to them being 'unavoidably' and regretfully omitted. However, the basis for a display of inferiority may not be the technology and its limitations, but the lack of resources a group musters in performing the work of unfolding: for example, its lack of manpower or of financial resources to perform the necessary simulations, build the prototypes, improve the test results through introducing changes, show the fit with other technologies, and so on.

Genealogy and Ontology

Two points remain to be stressed. One is that genealogy (in the sense of change through generations of corporate efforts called experiments) and ontology (in the sense of the superorganism form of being of these experiments) hang together to sustain each other. The summatological approach in experimental HEP responds to and signifies the erasure in this field of individual epistemic subjects; but it also, at the same time, creates the epistemic collectivity which takes the subject's place. The investment in a certain kind of discourse, the experiment's constant humming to itself about itself in unfolding its technological stories, allows members to ascertain for themselves the state of affairs in the experiment. No central structure that scoops up information, decides what needs to be done, and then dispatches commands to participants, is necessary; just a little pressing to nudge people into helping with the most immediate tasks. Unfolding and exhibiting creates the superorganism form of cooperation, in creating a collective consciousness and obligation. Yet in the beginning of an experiment, summatology and unfoldings of the recent past and future are instrumental in extracting and developing new options, and in forging a selection of technology with a maximum number of people into new generations of efforts of special 'entitvity', called 'experiments'. This entitvity, the fact that selectivity is incorporated in the superorganisms being 'born', makes for the generational picture of change - change that appears to come in breeds which are genealogically related (old experiments seed new ones), while at the same time being communally incorporated.

The interdependence of a way of change and a way of organizing a field is worth repeating. When an experiment spawns 'actors', as it occasionally does, or when it fragments (when the equipment and collaboration cannot be made to work), genealogical change tends to make room for other variants, which may include public controversy.²⁶ However, I want to conclude this paper not with an illustration of this case but by another point that bears repeating. In order to create an HEP experiment one needs to accomplish, and deal with, a series of disjunctures: a physicist's work needs to be disjoined from his or her authorship; groups need to be disjoined from their technological investments and accomplishments (when 'their' technology is eliminated and they are asked to join other efforts in an experiment); experiments are disjoined from

the laboratory in which they exist through the financial and organizational independence of the 'institutes' of which they are composed. During the birth of an experiment, this work of separation, through which the experiment becomes constituted as a distinctive and powerful structure in its own right, is carried out. It is not just a work of rearranging groups or technologies but of rearranging the social order, of *breaking components out* of other ontologies and of configuring, with them, new structures of 'Self-other-things',²⁷ new entities and their relations in terms of which a new ontology constitutes itself. The repackaging of efforts accomplished during the birth of a new experiment is also a repackaging of social composition and the creation of a new form of life. In the case described, the winning superorganism order papers over the more interactional, more 'human' social orders which its elements carry with them. It subdues the social energy that actor and interaction-based ontologies seem to need and to sustain, and with it to some degree the social itself - at least as it appears today in much of social life.

• NOTES

This paper was made possible by grants from the Mellon Foundation and the Deutsche Forschungsgemeinschaft in Germany. It is based on an ongoing ethnography (or 'laboratory study') of experimental HEP at the European Centre for Particle Physics (CERN in Geneva, Switzerland) conducted since 1987. The study involved the direct observation of experimental physicists at work, my regularly attending and taping their collaboration, detector, working group, R&D group (and so on) meetings since 1987, the study of physicists' internal proton-anti-proton notes, their experimental proposals and published papers, and interviews which by now fill several thousand single-spaced pages. For a technical description of the kind of experiment studied see C. Sutton, *The Particle Connection* (New York: Simon & Schuster, 1984). For a journalistic description of these experiments and the road of one to the Nobel prize, see Gary Taubes, *Nobel Dreams: Power, Deceit and the Ultimate Experiment* (New York: Random House, 1986). For some of the theoretical aspects of HEP, see Steven Weinberg, *Dreams of a Final Theory* (New York: Pantheon, 1992). For earlier laboratory studies, see Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, CA: Sage, 1979); Karin Knorr-Cetina, 'Producing and Reproducing Knowledge: Descriptive or Constructive? Toward a Model of Research Production', *Social Science Information*, Vol. 16 (1977), 669-96; Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford: Pergamon Press, 1981); Michael Lynch, *Art and Artifact in*

Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory (London: Routledge & Kegan Paul, 1985); M. Zenzen and Sal Restivo, 'The Mysterious Morphology of Immiscible Liquids: A Study of Scientific Practice', *Social Science Information*, Vol. 21 (1982), 447-73; and particularly the pertinent study of Sharon Traweek, ***Beamtimes and Lifetimes: The World of High-Energy Physics*** (Cambridge, MA: Harvard University Press, 1988) on high-energy physics in the US and Japan. An anthropological study of physics in general is provided by M. Krieger, ***Doing Physics: How Physicists Take Hold of the World*** (Bloomington, IN: Indiana University Press, 1992). For the contemporary history of HEP and related aspects, see the excellent studies by Peter Galison, *How Experiments End* (Chicago, IL: The University of Chicago Press, 1987); Galison (ed.), *Big Science: The Growth of Large Scale Research* (Stanford, CA: Stanford University Press, 1992); and Andrew Pickering, ***Constructing Quarks: A Sociological History of Particle Physics*** (Chicago, IL: The University of Chicago Press, 1984). See also Lorraine Daston, 'The Moralized Objectivities of 19th-century Science', paper presented at the Davis Center Seminar (Princeton University, May 1993), on the older history of international collaborations.

1. Thomas S. Kuhn, ***The Structure of Scientific Revolutions*** (Chicago, IL: The University of Chicago Press, 1970), and Larry Laudan, *Progress and Its Problems: Toward a Theory of Scientific Growth* (Berkeley, CA: University of California Press, 1977).

2. P. Churchland, *A Neurocomputational Perspective* (Cambridge, MA: MIT Press, 1989).

3. See, for example, David Bloor, *Knowledge and Social Imagery* (London: Routledge & Kegan Paul, 1976).

4. See, for example, H.M. Collins, ***Changing Order: Replication and Induction in Scientific Practice*** (London: Sage, 1985), and Stephen Cole, *Making Science: Between Nature and Society* (Cambridge, MA: Harvard University Press, 1992).

5. Perhaps HEP can be seen as the cultural culmination of the project of self-knowledge which Foucault associated with the history of the subject, or which Kaufmann described for the emergent bourgeois society in 'Madness and the Fear of Madness in the Emergence of Bourgeois Society in Germany, 1770-1850' (unpublished lecture at the Department of History, University of Michigan, Ann Arbor, 17 September 1991) - it challenges the idea of subjectivity as something attached to individual human beings. HEP explores the Durkheimian project of collective subjects, and the idea of self-knowledge and reflexivity in relation to embodiment, quite apart from human beings.

6. The notion '(re)configuration' refers to the (re)casting of the ontology of a domain, of the *kinds* of entities which populate it and the *forms of order* within which these entities relate to each other. For illustrations of these terms, and exemplification of the social ontology approach, see K. Knorr Cetina, 'The Couch, the Cathedral and the Laboratory: On the Relationship between Experiment and Laboratory in Science', in A. Pickering (ed.), *Science as Practice and Culture* (Chicago, IL: The University of Chicago Press, 1992), 113-38; and Knorr Cetina, ***Epistemic Cultures: How Scientists Make Sense*** (forthcoming 1995), Chapters 2, 5 & 7.

7. Emile Durkheim, *The Division of Labor in Society* (Glencoe, IL: Free Press, 1933).

8. D.S. Wilson and E. Sober, 'Reviving the Superorganism', *Theoretical Biological Journal*, Vol. 136 (1989), 337-56.

9. The concept of superorganism offers only a crude (and perhaps even rude) interpretation of HEP experiments. In this paper, I actually go from biologicistic to more subjectivistic notions. I am oscillating, just as the field oscillates between references to 'experiments' which include human collaborations and the detector and 'collaborations' which refer to human collectives only. The point is that from a social ontology perspective *any* analogy which captures local reconfigurations should be admitted. Besides, I find pertinent the infrastructural connotations of the superorganism metaphor, and the connotation of their relative autonomy.

10. Samuel Ting provided a good example of the relatively autonomous character of collaborations when he submitted proposals for an experiment at the Superconducting Supercollider which was to be built in Texas, and at CERN. Collaborations (and experiments) can choose between, and move between, laboratories. On the other hand, Ting, who is spokesman and initiator of the L3 experiment that currently runs at CERN, did not succeed in Texas. Mobility and independence are an important development in that they suggest a new relationship between experiments and laboratories, but the move of a whole collaboration to a new laboratory has yet to be achieved in practice.

11. Baudrillard's argument is that in an era of mass society the social (in the form of social relations, human interaction, and the like) has been absorbed and annihilated by the masses and the media, which concern themselves solely with simulations of social processes: see, for example, J. Baudrillard, *In the Shadow of the Silent Majorities* (New York: Semiotexte, 1983), 25-6.

12. I hasten to add that there are *some* rules and *some* structures, even if they involve being 'informal' and 'democratic', and that experiments differ in the structures they feature in interesting ways. What I mean is that these structures do not seem like the kind of bulwark they are in other organizations - bulwarks created to stem the tide of conflicts, failures, breakdowns or organizational non-functioning. They are not the kind of grid which makes organized work come to life.

13. See, for example, Charles Taylor, 'Cross-Purposes: The Liberal-Communitarian Debate', in N. Rosenblum (ed.), *Liberalism and the Moral Life* (Cambridge, MA: Harvard University Press, 1989), 159-82.

14. Superorganisms in biology centrally involve the presence of an edifice (the hive, hill, tunnel or nest) which members build and in terms of which they are positioned. The reader might ask whether I would go so far as to suggest that participants in HEP experiments live in their detector? The answer is that they do live in it, too - it is easy to be 'in' a several storey-high apparatus set up in a special 'hole' in the ground of which some parts (electronics) are dispersed in separate rooms - but, above all, they 'live with it' for so long, as the physicists say, that they 'love' their detector, and are brought together with it in discourse and references on most occasions.

15. T.D. Seeley, 'The Honey Bee Colony as a Superorganism', *American Scientist*, Vol. 77 (November-December 1989), 546-53, at 548.

16. Individuals who live with their detector but who are also moving and ontology breaking ... the reader might ask whether I am not bringing individuals back in as the master-entities behind all others? I believe I am not: individuals emerge from an (institutional) biographical regime which involves records of

identification, schooling, health, family, career positions, and the like, but this regime is just one among others of equal or superior relevance. With respect to the conduct of experiments, the superorganism ontology is dominant.

17. Knorr-Cetina (1995), op. cit. note 6, Chapter 3.

18. The notion that biographical actors constitute an 'environment' for HEP experiments is borrowed from differentiation theory which rigorously conceives of modern society in terms of a proliferation into 'subsystems': see for example N. Luhmann, *Soziale Systeme: Grundriss einer allgemeinen Theorie* (Frankfurt: Suhrkamp, 1984). I use the notion of the individual as an environment for the communal life-form of HEP to suggest that the distinction between the collaboration and the person is maintained and reinforced. Communal life-forms do not simply 'absorb' or 'integrate' individuals into their existence: they reconfigure them and in reconfiguring them they extend them - but they leave other configurations intact. In fact, the subject which has its stronghold in a biographical regime strikes back - I do not have the space to develop this here, but I discuss it in detail in Knorr-Cetina (1995), op. cit. note 6, Chapter 8.

19. There is also the environment of the laboratory which the natives call, alluding to its administrative-bureaucratic make-up and its life-supporting function for experiments, 'the structure'. The laboratory has its own added relationship to experiments, and its (one? or rather several?) agenda(s) in the birth of new collaborations.

20. The reader may find that I have a geological picture in mind here, a sort of plate tectonics of social life, when I say ontologies edge against each other This is in fact quite right. Only I do not see the strata all piled on top of each other, and it is not time which creates the pile up.

21. See, for example, Traweek, *Beamtimes and Lifetimes*, op. cit.

22. What does one mean when one says that collaborations 'think'? A revival of Durkheim's notion of collective consciousness? This is indeed what I do shortly. What I mean here is that one can find references to future generations of experiments in participants' talk and practices.

23. Durkheim, op. cit. note 7, Chapter 3.

24. These are the words of a senior physicist at CERN and longtime member of UA2, in an interview conducted in meetings by myself. The description corresponds to that given by many other physicists in informal contexts.

25. How do rapporteurs create the syntheses they produce? In fact, I don't know that well enough. Much of it seems to be assembling together transparencies from speakers who give the more experimental talks. But, obviously, selections are made, and further investigation is needed.

26. The claim that forms of agreement-formation change when ontologies change can be illustrated by the example of Carlo Rubbia, who won the Nobel prize for the discovery of the W and Z zero vector bosons at CERN in 1983, and then managed to create a controversy when he announced additional discoveries, which he later retracted. Rubbia is indeed generally credited with being an individual actor ('a personality', 'a man', 'a politician', and so on). More examples of this kind would be needed. But my point is that ontology and forms of agreement-formation are not really two different issues: if there are individuals who are set up against each other, then their controversies also contribute to and affirm individuation.

27. M. Merleau-Ponty, *Phenomenologie de la perception* (Paris: Gallimard,

1945), English translation: *Phenomenology of Perception* (London: Routledge & Kegan Paul, 1962).

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