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STUDIES IN CERN HISTORY

Some Socio–Historical Aspects of Multi–institutional Collaborations in High–Energy Physics at CERN between 1975 and 1985

John Krige

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> "What will never happen again is what happened to me once when I was a graduate student. [...] My thesis supervisor rang me up one day and said, "Hey I have just had a great idea and we could do it by slightly modifying our experiment. We just have to sneak in a few extra hours beamtime". And we did an experiment. And the next week we repeated it. [...] I have never done an experiment like that in high–energy physics, where you just on the spur of the moment did something, a little bit of bricolage, a little bit of playing around, got a significant measurement, went away, analyzed it for two days, published it. We're a long way from that".
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> UA1 physicist who did his PhD on a small cyclotron in the late 1960s.

One of the most striking features in the postwar development of high–energy physics has been the growth of large teams of physicists on the experimental workfloor. Before the war, experimental research, even with with big accelerators like those at Berkeley, was generally done with relatively simple equipment, and remained an essentially individual affair.[1] However, as detectors became more complex, costly, and time–consuming to build, increasing numbers of physicists, often from more than one institution, began to do experimental work together with the device. This phenomenon emerged in the late 1950s, and the size of teams has tended to expand ever since. A typical bubble chamber collaboration at CERN in the mid–1960s comprised perhaps fifteen physicists plus technical support, while an electronics or 'logic' experiment might have had about ten physicists grouped together around the equipment.[2] A decade later about 50 physicists from seven institutions routinely signed the papers reporting results obtained with CERN's largest bubble chamber (Gargamelle), while the first set of electronic experiments at the Super Proton Synchrotron (SPS) typically comprised some 20 to 40 physicists from four to six different laboratories. By the mid–1980s even these team sizes were dwarfed by the collaborations planning to do experiments at LEP, CERN's Large Electron–Positron Collider. In 1985 the Delphi collaboration, for example, comprised over 350 physicists from 37 institutes in 17 countries.[3] Needless to say, the major collaborations on the Superconducting Super Collider (SSC), appropriately located in Texas, and scheduled for completion in the late 1990s, are expected to be at least twice this size. Very roughly, then, the number of physicists in a single collaboration

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[1] For a description of the role of physicists in accelerator building in Lawrence's laboratory at Berkeley before the war, and the contrast with the individualism of research see, in particular, Wilson (1972), p. 471. See also Heilbron and Seidel (1989) Crozon (1987) provides a readable general introduction to the evolution of high–energy physics. Pickering (1984) is a sociological study of developments in physics during the period covered by this paper.

[2] The data is from Pestre (1990), p. 480. For the difference between a bubble chamber experiment and an experiment using electronic or 'logic' methods of detection, see Galison (1990).

[3] For data of this kind one can use the so-called CERN Grey Books, produced annually since 1975, and entitled *Experiments at CERN*. These books list, for each current experiment, the names and (since 1976) the institutional affiliations of the physicists involved. Though containing inaccuracies, the books are a useful general guide to the composition and evolution of collaborations at the laboratory.

taking data with the largest detectors used in high–energy physics has been doubling every five or six years since the mid-1960s, and might reach 1000 by the year 2000.

It is striking how little has been written about this phenomenon, considering its sociological and historical interest. It was something of an issue in the early 1960s, when large teams first appeared on the horizon, and a number of interesting studies of the change and its implications were made.[4] Very little was done for the next two decades, until Galison and, from a somewhat different angle, Pestre again drew attention to the changes in the organization of the experimental workplace and to some of the institutional problems surrounding collaborations in high–energy physics.[5] At the same time there has been a growing interest in the theme among the US physics community itself, doubtless in anticipation of the arrival of the SSC. In 1985 the NSF sponsored a symposium to explore the costs and benefits "of international scientific cooperation between the U.S. and other countries in big science". In 1988 a HEPAP Subpanel reported on "future modes of experimental research in high–energy physics".[6] And the Center for History of Physics of the American Institute of Physics has just compled a major study, based on interviews, of multi–institutional collaborations in hep in the United States and in Europe.

While it is difficult to generalize from this literature, one image that emerges, and which is tenacious partly because it reinforces what many 'spontaneously' believe, is of the steady industrialization of the experimental workplace in high–energy physics. Multilayered managerial structures have been imposed, hierarchical relationships have replaced free exchanges between equals, bureaucracy is rampant, and decision–making processes have become increasingly formalised. From the point of view of the individual physicist, work has become boring and repetitive, with little scope for creativity and autonomy. In short, 'basic' research in experimental high–energy physics has now adopted the work patterns of applied research. The free–wheeling, creative atmosphere of the university laboratory has been supplanted by the constricting procedures and regimentation of the large corporation.

This picture is undoubtedly symptomatic of important changes in the nature of experimental work. Yet it must be handled with care. For one thing, it is partly the result of studies which have focussed on the work done in Luis Alvarez's group in Berkeley in the early 1960s.[7] Here an assembly–line approach was indeed adopted to facilitate the processing of hundreds of thousands of photographs taken on the 1.8m hydrogen bubble chamber. As such the studies are, to some extent, specific to a certain type of detector — work with electronic detectors left far more scope for individual initiative —, specific to a certain laboratory — there is no evidence that European bubble chamber physicists went to the extremes adopted at the LBL in the

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[4] The *locus classicus* of the early research is Swatez (1970), which was based on work done at the Radiation Laboratory of the University of California from 1963 to 1965. See also, for example, Hagstrom (1964), Kowarski (1965) and Weinberg (1972).

[5] For the 1970s see, for example, Wilson (1972) and Morrison (1978), both of whom are physicists. For the1980s, see Galison (1985), (1987), (1988), (1990), Pestre (1990), chapter 8 and Traweek (1988), especially chapters 4 and 5. Pestre also studies in detail the strains which arise between the host laboratory staff and the outside physicists who collaborate with them at a facility. For more on this see Kowarski (1964) and Krige (1990a), (1992).

[6] See NSF (1985) and HEPAP (1988). See also, for example, the study by Heusch (1984).

[7] For example, Galison (1985) and (1990), and Swatez (1970)

early 1960s, if only because they lacked the technology to do so —, and specific to a certain period of time — in the late 1960s new technologies were invented which considerably reduced the drudgery in the analysis of bubble chamber film. Generalizing the factory model to the field as a whole is thus particularly hazardous in this case.

A second reason for caution is that the model is laden with nostalgia, with a yearning for a romanticised past, for a golden age.[8] It emerges particularly forcibly in the sayings and writings of people like Don Glaser, who won the Nobel Prize for inventing the bubble chamber, of Luis Alvarez, who won the Prize for developing and exploiting the technique, and of Bob Wilson, the founder and first director of the Fermi National Accelerator Laboratory just outside Chicago. Glaser left the field rather than work in what he called (in an interview made in 1983) "the factory environment of big machines".[9] Alvarez, speaking in 1967, "began to despair at an industrialized nuclear physics that had become, in his words, 'just a little dull' ".[10] Wilson has described, with masterful ambiguity, his "fight against team research".[11] It is clear, however, that all three were highly individualistic and creative people. Their remarks and attitudes, while certainly reflecting and sustaining a certain ethos in the physics community, are not necessarily a reliable guide to the actual state of affairs. Nor should they be taken as representing what the average competent physicist doing 'normal' science feels about his or her work situation.

My main aim in this paper is to lay the foundations for a better understanding of multi–institutional collaborations in high–energy physics, better, that is, than that provided by the 'factory model'. To this end I shall present the findings deriving from archival research and interviews with physicists who worked in such collaborations at CERN between 1975 and about 1985.[12] These are analysed with reference to a number of 'classical' sociological questions — how are such collaborations formed? how are they organized? how is credit attributed to individual researchers? is there scope for individual autonomy and creativity within them? My central finding is that experimental workplace in high–energy physics is far less structured, the atmosphere far more informal, and personal satisfaction far more widespread, than the factory model would lead one to believe.

That said it must be stressed that the results presented in this paper must not be generalized too quickly. They are based on a study of a few electronic experiments initiated at CERN in the mid–1970s. In fact the bulk of data comes from just two collaborations UA1, whose spokesman was Nobel Prizewinner Carlo Rubbia, and, to a lesser extent, UA2, the 'backup' experiment whose spokesman was Pierre

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[8] On nostalgia see, for example, Blume's piece in Bud and Cozzens (1992).

[9] Quoted in Galison (1985), p. 316.

[10] From Galison (1988), pp. 86–7.

[11] See Wilson (1972). For Wilson's role in the founding of Fermilab, see Westfall (1989).

[12] The interviews were conducted within the framework of a project to study multi–institutional collaborations in hep which was initiated by the Center for History of Physics of the American Institute of Physics. Funding for my part of the work, which was devoted to interviewing some 40 physicists on 5 experiments at CERN, and to identifying important collections of relevant documents, was provided by the Andrew W. Mellon Foundation. The tapes and rough transcripts of these interviews have been lodged in the Center for History of Physics archive at the AIP in New York and in the CERN archive in Geneva.

Darriulat.[13] These collaborations comprised respectively about 130 and 50 scientists around the time when they began taking data (1981). Technically similar experiments (i.e. using colliding beams with the collision areas 'completely' surrounded by the detector) now running at LEP and envisaged for the Large Hadron Collider and the SSC are far larger. It is quite conceivable that, with the step up in size to the 400-500 mark, the workplace is assuming many of the features of the factory model which I am arguing against. Indeed some LEP physicists complain that life is not what it used to be, that they have become anonymous workers on an assembly line producing physics results. Nostalgia? 'Truth'? Only further work can tell.

How are collaborations formed?

Morrison has described briefly how and why collaborations are formed and grow, and we have little to add to what he says at a general level.[14] The interest lies in illustrating, and in trying to refine and to extend some of his ideas with reference to a specific case. This we shall do here by spelling out in some detail the factors shaping the birth and growth of one particularly important collaboration, the UA1 (Underground Area 1) collaboration whose initial spokesman was Carlo Rubbia

Early in 1977 Carlo Rubbia organized a proton–antiproton study week at CERN in anticipation of either CERN or Fermilab going ahead with a major p–pbar (proton–antiproton) project. About 35 people from CERN, some European laboratories, and from the United States (one or two) participated. At the end of the week, which lasted from 28 March to 2 April 1977, a paper was prepared "for the attention of the CERN Management" which summarized the conclusions reached on the characteristics of the detectors required at a p–pbar facility. The meeting also set up a structure "to guarantee a continuing activity" of this work.[15]

During the next six months about 30 so–called ppbar notes were written. These were technical memoranda most of which discussed the features of the detector needed to do colliding beam physics at very high energies. They were written by scientists based at several different institutes (Annecy LAPP, CERN, Rome University, Saclay, University of California at Riverside...) and circulated among all those interested.[16]

On 8 November this core set of people (about 25) met formally and held a "general discussion on how to get organized from now on". Six institutions were represented. They set themselves an extremely tight schedule. 1 December was the deadline for the final sketch of the detector. The final drawing of the detector with optimal parameters was to be ready by Christmas. By mid–January the proposal to be submitted to the SPSC (the experiments committee responsible for making

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[13] Many of the documents used for UA1 were from the private collections kept by David Dallman (which is very extensive) and by Alan Norton. Both are at CERN. I would like to thank Kyoung Paik for help with sorting through the documents, and for making rough transcripts of the interviews.
[14] Morrison (1978).
[15] For the announcement of the study week see the circular by Rubbia in File DGR21298–CERN archives. The note prepared afterwards for the CERN management and entitled *Conclusions of the study on the detectors* is in (DGE21576–CERN).
[16] There is a selection of these ppbar notes in (JBA22633–CERN), for example.

recommendations about what proposals should be accepted) was to be typed. And on 31 January 1978, we are told, there was to be "Propaganda made – Proposal submitted".[17]

The 'collaboration' met formally (in the sense that minutes were written and circulated among those present) at least once a week from then on until mid–January. The numbers present stayed constant at about 25. However there were important changes in the institutions represented. Aachen, Annecy LAPP, CERN, Collège de France, Saclay and Riverside were the initial core. There was a representative from Harvard University at one or two meetings, and from the Inter–University Institute in Brussels at another: neither institution remained formally part of the group. More significantly, a number of British groups joined during this time. At the meeting on 15 November it was reported that there was an "Interest from Rutherford Lab. (where C.R. [Carlo Rubbia] and B.S. [Bernard Sadoulet] are going to make some propaganda on Monday) [...]". The trip was made, and the following week, on 22 November, Rubbia reported that "One result of the seminar held at RHEL [was] the interest of Birmingham for our project" – indeed two Birmingham representatives attended this meeting. By mid–December Rubbia had been informed in writing of Rutherford's interest in the collaboration: they would like to join "our group together with 4/5 other physicists and adhoc technical support". (The physicists were doubtless the representatives from the third interested UK group, Queen Mary College). At this point (13 December 1977) Rubbia felt that the collaboration was approaching its optimal size, and that "from now on it [would] be harder to join our group". In particular it was agreed "that any other large institution who would like to join [would] give serious problems".

The proposal for experiment P92 (subsequently called UA1) was submitted to the SPSC on deadline – 30 January 1978. It ran to over 150 pages including references, and was signed by 52 scientists.[18] 48 of these were from the nine main institutes we have mentioned – Aachen (5 representatives), Annecy LAPP (6), University of Birmingham (10), CERN (8), Collège de France (4), Queen Mary College (4), University of California, Riverside (3), Rutherford Laboratory (4), and Saclay (4). The other four signatories of the proposal were visitors from Wisconsin, Harvard and Rome.

At the collaboration meeting on 7 February 1978 Rubbia reported that two proposals and two letters of intent to do colliding beam physics had been submitted to the SPSC and that each group would have to defend its proposal at open presentations as early as 21 February. He felt that his collaboration would need 60–90 minutes to describe first the experimental set up and then the physics programme. Regarding the latter, it was felt that although "one speaker only for the physics programme would be better for the continuity of the talk", on the other hand "four speakers from different labs [would] show that we are already a working collaboration". It was suggested that Sadoulet describe the detector and that the physics be split into three topics to be

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[17] The minutes of this meeting are in the Dallman papers (see note 13). Unless otherwise stated all of the following material dealing with the setting up of UA1 is from this collection. The documents are headed *SPSppbar Project. Summary of the meeting...* or *Minutes of the meeting held on...* From about 8 March 1977 they were headed *SPS ppbar P92 collaboration.*

[18] A collection of the papers of the SPSC are in the CERN archive.

dealt with by Dowell (Birmingham), Linglin or Della Negra (Annecy) and Rubbia. Rubbia would speak on the W and the Z particles, the major discoveries that were anticipated by the collaboration.

Early in April Rubbia reported on the progress with the SPSC. Collaboration meetings were now being held every two to three weeks, and were being regularly attended by about 40 people from the nine collaborating institutions. Rubbia remarked that two out of five proposals (those that became UA1 and UA2) had been considered and refereed, and that "Nothing appeared in the minutes but ours went through without any major objection".

At the end of May 1978 the so-called Coordination Committee for experiment P92 met for the first time. A member of the CERN Directorate who was responsible for the experiments to be done at the p–pbar collider (P. Falk–Vairant) was in the chair. He explained that this committee would meet roughly bimonthly, and that its task would be to "follow and supervise closely the progress of the proposed experiment". Each collaborating institution was to be represented on it. (This committee subsequently came to be called the UA1 Executive Committee). One of its first tasks was to draft an agreement setting out the responsibility of each institute in the collaboration. It would include a time table, cost estimates, a list of physicists with at least a three–year commitment to the proposed experiment, detailed information on manpower needs, and so on.

About this time too the collaboration was informed that two further institutes, Rome University and the Institute for High–Energy Physics in Vienna would like to join the collaboration. Both were ultimately accepted, though not without some difficulty, a point to which we shall return below. Indeed when the CERN Research Board accepted the UA1 proposal on 29 June 1978 the number of participating institutes was still just nine and the document distributing responsibilities between thelaboratories had not yet been drawn up. This was finally settled by 31 October 1978. A mere three years later the huge detector, which weighed around 2000 tons and which included some highly sophisticated, state–of–the–art technology, began taking data for the first time.

The most important point we want to stress about this 18–month process of formation and growth is that it occurred because of the combined effect of a number of very different considerations. Certainly the scientific interest of the experiment and the technical design of the detector were the cornerstones underpinning the formation and consolidation of this collaboration, and its successful implantation at CERN. However, on their own these cannot account for the process we have just described: a number of other social, institutional and political considerations have to be taken into account if we want to understand how and why the UA1 collaboration 'gelled'.

Firstly, there was the mutual trust and respect between the scientists themselves, the conviction that each group in the collaboration was capable of pulling its weight and delivering its part of the detector on time and in good working order. It was precisely because this trust was lacking, because it was feared that it was not a "strong group", that the collaboration initially reacted so negatively when it learnt that a team from Vienna was interested in joining.[19] Conversely, the addition of the Rome group was unproblematic. One of their representatives (Salvini) had been actively working with the collaboration as a CERN visitor since early 1977 and his name was included on the original proposal.

Another obviously related consideration affecting the entry of groups into the collaboration was the knowledge that they had the infrastructural support behind them at their home institutions needed to take on a major construction project. This was one main reason why Rubbia visited the Rutherford laboratory even though he had not worked with some of the British groups before. "We are fairly weak, hardware wise", he told the embryonic collaboration at CERN, "and we are eager to accept hdw people like RHEL".[20]

Political considerations, though never explicit, were also not far beneath the surface. The p–pbar project at the CERN SPS only received the Council's backing in June 1978. While its acceptance was always something of a formality (it was rather cheap), the fact remains that some physicists were totally against it on the grounds that it might jeopardize LEP. Others, notably in Britain, were not keen on it for fear that it would seriously impede fixed–target physics at the SPS. One way of swinging the all–important delegations from major Member States behind the project was to include physicists from institutes in their countries in a collaboration which promised, after all, to do the most exciting physics of the 1980s. Thus it is perhaps not a coincidence that Rubbia and Sadoulet went to Rutherford "to make some propaganda" for P92 only six weeks after two members of the CERN Directorate had discussed CERN's plans with UK users at the RHEL and had found a "lack of popularity of ppbar, which we must try to correct", as they put it.[21]

The importance attached by the CERN directorate to having outside laboratories in its Member States participate in the experimental programme doubtless also played a role in shaping the composition of UA1. For example in March 1978 Walter Thirring, an extremely influential Austrian theoretical physicist wrote to CERN expressing both enthusiasm for the p–pbar project and concern that "as the size and complexities of large experiments go up it will become increasingly difficult for smaller laboratories to compete with the large laboratories of the bigger memberstates of CERN".[22] The grounds for Thirring's concern are clear. The early core of UA1, as we have seen, was made of groups from CERN itself, along with those in Britain and France (and a small contingent from Riverside), countries which had large national facilities of their own. We have just seen the importance attached by the spokesman to having the infrastructural resources at a site like Rutherford deployed for UA1. University laboratories in small countries simply could not call on such resources. To

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[19] The early negative reactions to the Vienna group joining were mentioned in several interviews. I have also seen this in a document which I cannot now retrieve.

[20] Minutes of the meeting of the SPS ppbar project held on 13 December 1977 (Dallman papers).

[21] For the visit to the RHEL as also being a propaganda exercise, see the Minutes of the SPS ppbar project meeting held on 15 November 1977 (Dallman papers). For the report by the CERN management on the attitude of UK physicists see see the memo by F. Bonaudi dated 18 October 1977 (DGE21576–CERN).

[22] Letter Thirring to Van Hove, 10/3/78 (DGR21298–CERN). For the extent of the contribution made by larger Member States to UA1 see also Table 1 below.

counteract the corresponding tendency to concentration the CERN management strongly favoured, not just a wide representation of institutes in this experiment (and in UA2), but the representation of institutes in the smaller Member States in particular. And indeed, it was shortly after Thirring's contacts with senior management that the group from Vienna joined the collaboration.

This brings us to the contribution of the senior CERN management. It was crucial inside the organization. A man like Research Director–General Leon Van Hove was crucial in that he backed the somewhat risky p–pbar project from the start (despite the doubts of many physicists and the lukewarm attitude of his partner and Executive Director–General John Adams), and persuaded and cajoled all the top policy–making committees at CERN to finance the scheme. He also ensured that experiment UA1 had all the necessary infrastructural support inside the laboratory, where in the words of one of the participants it was given "red carpet treatment". The management also played an important role in the Member States. A man like Paul Falk–Vairant, member of the Directorate reponsible for p–pbar experiments, undoubtedly encouraged groups in his native France to back the proton–antiproton project. By all accounts he also strongly supported Pierre Darriulat's experiment proposal P93 (laterUA2), which had a strong French core, and which was in competition with a proposal by Nobel Laureate Sam Ting (and which apparently had Van Hove's backing).[23] In short the CERN Directorate, largely united over generalities, sometimes divided over particularities, made a fundamental contribution to the growth and consolidation of UA1 and of UA2.

There is one other actor who should be mentioned to round off the picture. This is the SPSC, the committee comprised of senior physicsits from CERN and the Member States, whose task it is to consider experiments proposed at the SPS and to make its recommendations to the CERN Directorate. It is they who supported UA1 without hesitation. It is they who made the decisive choice between Darriulat's proposal and Ting's, after consultation with external referees and a dramatic "shoot–out" between representatives of the two groups at an open meeting in December 1978. It is they who decide on the scientific desirability and technical feasibility of an experiment, who strive to draw a clear line between "objective content" and "non–scientific context", who both confirm and legitimate a choice from a "strictly scientific point of view". [24]

The formation and consolidation of a collaboration is thus a complex process which brings together a number of very different protagonists who have different institutional locations and roles, and who make different kinds of contributions to its ultimate success at different stages in its evolution. At the heart of the process there is the core of scientists who push the project.[25] It is they who have to persuade other members of the community to join them, who have to ensure that influential sections of the management back them, who have to steer their proposal through the

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[23] For more on UA2 and the competition between Darriulat's proposal and Ting's see Taubes (1986), chapter 5. Taubes quotes Rubbia (at p. 59) as saying that "There was a strong French push essentially, and the man in charge, the director of research, was also French. He had a great sympathy for those people". As well as for UA1, its spokesman may have added, given the early and important commitment of French groups to that experiment as well.

[24] For the importance of this kind of legitimation see Krige and Pestre (1986), especially section 5.

[25] Cf Morrison (1978), p. 353..

experiments committee. In short it is they who have to sell their idea at CERN and in the Member States. We are indeed a long way away from the situation described in the quotation at the start of this paper.

How are collaborations organized internally?

Work in a large collaboration has to be organized. The size of the detector (it can weigh thousands of tons), the nature of its construction (particularly if it is modular, with different units being built by different groups), the time constraints (the need to meet deadlines and to compete with rivals), the mass and variety of data to be analysed (there are many physics topics to be studied), the sheer number of people involved (tens or hundreds working together) – all of these demand that some sort of organizational structure is set up inside a collaboration. And when one contrasts this situation with the picture of the individual scientist following freely where Nature leads, it is but a short step to identifying work inside a large collaboration with work inside a 'factory' or large corporation. In this section we want explore the plausibility of this analogy. We shall see that, while superficially there are some parallels (planning and coordination of project, division of tasks, hierarchy of responsibilities...), any simple identification of an experimental collaboration with a business corporation fails. And it fails because the qualified physicists and engineers who work in large teams tend to regard and to treat each other as *professional equals* and peers, people who are working *alongside them* to achieve a common objective.

There is one important distinction to bear in mind before we get under way. The factory model, in so far as it has any plausibility at all, can only apply to the period during which the detector was being constructed. In the case of UA1 and UA2 this lasted for three to four years (from design to commissioning), which was remarkably fast for devices of this type. During this phase the work was carefully organized and planned as we shall see. Once the detector started taking data, however, and the analysis of physics results began, a far looser organizational structure was put in place. Physics analysis, the HEPAP tells us, is "intrinsically next to impossible to 'manage'".[26] This is not to say that physicists are free to explore whatever topics they like, but simply to indicate that the constraints on what they do are not those identified in the 'factory' model – and to insist that whatever merits that model may have, its value is restricted to the construction phase of the detector.

The detectors for UA1 and UA2 were not built exclusively at CERN. Both consisted primarily of a number of interlocking modules with a 4pi geometry (rather like the layers of a cylindrical onion) along with triggers which selected interesting events and a data acquisition system.[27] These various components were shared between the collaborating institutes which generally built them at home, bringing their various subdetectors or components to the host laboratory for final testing and assembly. The division of labour between the various centres was defined in a formal

[26] HEPAP (1988), p.31.

[27] For a useful technical description of the UA1 detector see Watkins (1988), chapter 9.

"Agreement on the Sharing of Responsibilities [...]" and is shown in Figure 1, along with the deadlines imposed on each participating institution.[28]

How were these responsibilities distributed? To begin with it was clear that the central detector of UA1, the heaviest and most techically advanced part of the device, should be built at CERN. This component, as envisaged in the UA1 Agreement, consisted of "a volume filled with about 11000 drift chamber wires in order to record an image of the many tracks produced in the collisions. The electronics for the detector capable of continuous recording between [proton–antiproton] bunch crossings is entirely new", the document went on, "and must be developed", adding that CERN "accepts entire responsibility for the device including the electronics and readout". Apart from the sheer logistic difficulty of transporting such a detector from, say, Paris to Geneva, the fact that CERN had the money, the personnel, and a large number of highly–qualified people on the spot who could dedicate themselves full–time for three or four years to this one task meant that the host laboratory necessarily built this module. As for the remaining components, they were distributed on the basis of the interests, past experience, and resources of each participating institute, though of course there was also a certain amount of horse–trading between the various collaborating laboratories. The UK groups in UA1, for example, would have liked to build the rather challenging electromagnetic calorimeter. Instead this job went to Saclay , while the British groups were given the less demanding hadron calorimeter. At the same time the latter secured the trigger processors for both calorimeters, an item which played a crucial role in the data taking.

In agreeing to build a part of a detector an institution was also committing personnel, money and computing time to the collaboration. Table 1 shows the extent of these commitments as envisaged at the end of 1978. It confirms the heavy involvement at every level by the major institutions in two big Member States, Britain and France. Indeed, along with CERN, the three institutes in each of these countries were together responsible for about two–thirds of the physicists, programmers, and engineers involved in the construction of the detector, were expcted to bear over 85% of its cost in terms of material, and anticipated providing 90% of the required computing time for data anlaysis.

The coordination of the building, assembly and installation of the UA1 detector was entrusted to a Technical Committee chaired by Hans Hoffmann. This committee met every week throughout the construction period. About 25 people regularly attended these meetings. In addition many other formal meetings (in the sense of meetings for which minutes were kept and circulated within the collaboration) were held at CERN during this phase of UA1's life, each intended to deal with specific tasks. Thus we find Central Detector meetings, Muon Detector meetings. Calorimeter meetings, Trigger meetings, Gas System meetings, On/Off–Line Software meetings, Database meetings, Graphics meetings etc. In addition there were regular meetings of the whole collaboration and of the Executive Committee, these being the only two

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[28] The "Agreement on the Sharing of Responsibilities Amongst the Participants in the Experimental Programme based on a 4pi–Solid Angle Detector for the SPS used as a Proton–Antiproton Collider at the Centre of Mass Energy of 540 GeV" dated 31/10/78 is in (DGR21298–CERN).

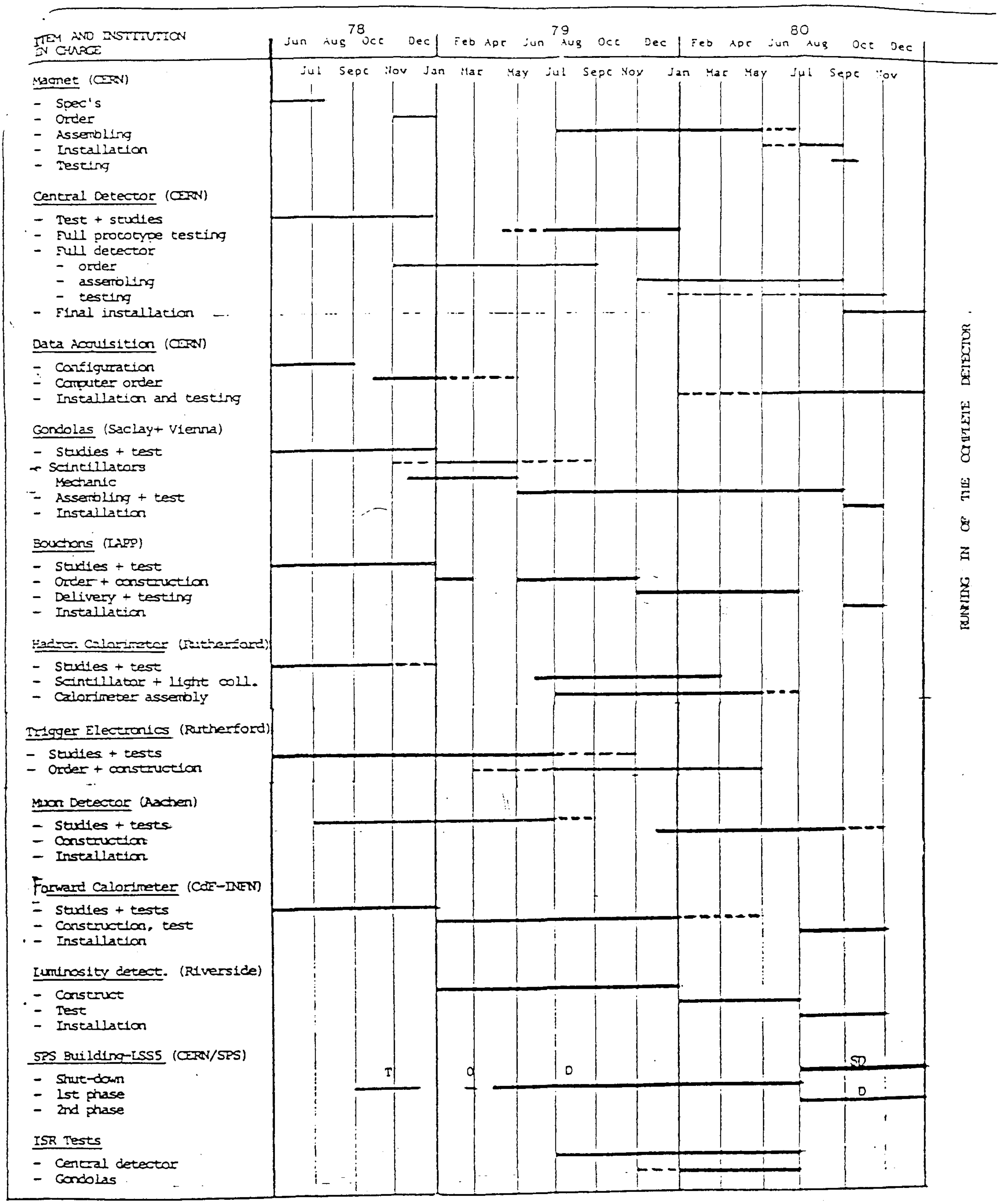

Figure 1. The timetable for the construction and installation of UA1 as spelt out in Annex 2 to the draft "Agreement on Sharing Responsibilities..." (cf note 28), and in which the contribution of the collaborating institutes is clearly identified.

Table 1. The commitments made by the collaborating institutes in UA1 in terms of various categories of personnel, of money, and of computing time. The first and last are from the draft agreement for sharing responsibilities inside UA1 dated 31 October 1978. The financial responsibilities are provisional estimates in millions of Swiss Francs made in December 1978 and exclude salaries.[29]

Institute/ Obligation	Physicists, Programers	Engineers	Technicians Draftsmen	Money (MSF)	Computing Time[1]
Aachen	6		2	2.0	
Annecy	9	2	3-5	2.5 w. CdF[3]	11%
Birmingham	10+2 students	see RHEL	1 or 2	see RHEL	see RHEL
CERN	11 +	5	5 + supp. staff	14.5	33%
C. de France	7	2	7	2.5 w. Ann.[3]	11%
Q. Mary Coll.	6 + 2 students	see RHEL	1	see RHEL	see RHEL
Riverside	3			0.4	5%
Rome	7	1	2	0.5	5%
RHEL[2]	4 + 3 RAs	4 + supp staff	2	3.8	24%
Saclay	8 + 3 phys.	3	3	2.5	11%
Vienna	3	2		0.4	

Notes.
1. The computing time was for data analysis and it was assumed that the collaboration would need 1000 hours a year on a CDC 7600 or equivalent.
2. Rutherford Appleton Laboratory, which had overall administrative responsibility for the British participation in UA1.
3. The combined contribution of Annecy LAPP and the Collège de France was 2.5 MSF.

formally constituted bodies which met regularly throughout the entire life of the collaboration, from construction through data taking and analysis.[30]

The most striking feature which emerges from an analysis of the attendance at these committee meetings is the key role played by a small core of people. We find that perhaps 20 scientists are responsible for writing the minutes of the various meetings and that they are mostly senior people: about 15 of them 'represent' their institutes on the Executive Committee. We find that although as many as 85 different people may attend meetings of the Technical Committee during a year, there is again a small number, maybe five or six, who attend regularly, week after week, people like Sergio Cittolin who was repsonsible for the data acquisition system, Bernard Sadoulet who was reponsible for the central detector, and Guy Maurin who was responsible for the overall adminsitration of the group. Finally we find that the

[29] Columns 2 – 4 are from Annex 1 to the "Agreement" cited in the previous note. Column 6 is from paragraph 9 in the same document. The cost data in Column 5 are from letter Falk-Vairant to Yoccoz at IN2P3, 14/12/78 (DGR21298–CERN).
[30] The list of meetings was collected together in the *UA1 List of Publications* prepared annually by Denis Linglin (Dallman and Norton papers).

spokesman and head of the collaboration, Carlo Rubbia, while taking the chair at collaboration meetings, and not missing an Executive Committee meeting, was far less often present at lower level meetings. This was even the case with the all–important Technical Committee, where he apparently attended about 60% of the time in 1978, about 40% in 1979, and seldom if ever in 1980.[31] The picture then *seems* to be clear, and coherent with the classic pattern of business organization. We have a pyramidal structure, with the spokesman at the apex, the 'boss', a layer of middle management, say 25 people in the centre who were responsible for the day–to–day organization of the construction of the detector, and a broad base of scientists below that (remember that there were about 130 physicists and engineers in the UA1 collaboration in 1980/1), a mass of people who were more or less excluded from the loci of power and of decision–making.

There are two criticisms that can be levelled at this model. Firstly, it is wrong to assume that because there is a hierarchy of responsibility inside a large collaboration, then necessarily the bulk of the scientists are excluded from the decision–making processes. As a general rule this is simply not so. The main purpose of the meetings that are held is not to pass on instructions but to share information, to communicate and to consult, and to decide collectively. Correlatively attendance at meetings is not an obligation imposed from above, but a response to a perceived need to be informed about things that directly concern one's work. In fact many meetings are arranged on an ad hoc basis to discuss a particular problem, and are dissolved after two are three sessions when the problem has been resolved. There is planning and there is coordination inside a collaboration, and there is a core of people who have more responsibility than others, and who have to ensure that certain things get done. But as a general rule there is not top–down management, there is shared decision–making.

The second weakness of an overly formal picture of how a collaboration is organized is that misses the ongoing, *informal* relationships between the members. The scientists and engineers in a collaboration, from the senior physicists down to the junior graduate students, are in constant working contact with one another and with the technicians, rubbing shoulders together, discussing what has to be done and how best to do it, taking myriads of mini–decisions throughout their long, often very long, working days. Those with special responsibilities are never far from the workplace, their offices arranged to ensure accessibility and to facilitate communication.[32] Meetings punctuate this ongoing exchange of information. They are pauses intended to iron out specific problems or to discuss new ideas, after which everyone plunges back onto the "shop floor".

There is one last qualification to be made before we leave this point. We have suggested above that the picture of a collaboration as having a pyramidal structure is misleading, that the managerial structures are more fluid, hierarchical relationships are more blurred, beareaucracy is less important, than any unsophisticated industrial

[31] We do not have a copy of the minutes of every Technical Committee meeting, and so we need to be cautious in our formulations. For the three years mentioned here we have the minutes of about 25 meetings, so around 50% of those that were held.

[32] On the importance of how offices were arranged in UA1 – a point mentioned frequently in interviews – see also Traweek (1988), chapter 1.

model – and the literature has not yet moved beyond that level of analysis – would lead one to believe. Indeed, one might add that when interviewing physicists in UA1 and UA2 many of them were puzzled by the notion of there being a middle management in the collaboration and felt that it was somehow inappropriate. At the same time it has to be said that, at least as far as UA1 was concerned, the picture painted above is somewhat idealistic. For here there was undoubtedly a boss, Carlo Rubbia, who by his genius, his determination, his charisma, and by his notorious inability to tolerate opposition, in fact imposed his will on the collaboration, to the extent that no important decision could be taken without his first giving the green light. At the same time it is instructive to note how many of those interviewed resented this, revolted against a structure in which there was Rubbia and the rest, as one of them said. In short, some collaborations might indeed be organized like large corporations with a top–down management structure – but it goes against the grain of scientists who believe that authority and power should derive from experience and expertise, that compliance should be the result of consultation and persuasion not coercion, and that decisions should be made collectively not imposed from above. And who like to work together, and who would like to organize their work together, around these assumptions.

To conclude this section one brief comment about the organization of work during the data–analysis phase. As we suggested, the general rule here is to let each physicist follow his or her specific interests. That granted, there is however one fundamental constraint: that only physicists based at CERN can hope to work on hot topics like the search for the W or the Z particles. There are many reasons for this. Firstly, there is a large number of people concentrated at CERN, people who are interacting continuously with the detector and with the data that it is producing, people who meet every evening to discuss the significance of new candidate events. No university department, say, can hope to reach the "critical mass" of scientists who have the time, the freedom from other responsibilities, and the different points of view which are needed to extract a significant signal from the background noise. Secondly, the results are generated at CERN and so can be analysed immediately. To analyse the data in the UK, for example, copies of tapes have to be flown to London, transported to Rutherford, and loaded in the computers there, all of which introduces delays which are significant when one is racing to beat a rival. And finally there is the infrastructure of CERN, the computers above all, but also services like administrative support and press relations, all of which are there to exploited if an important result has to be produced and announced quickly. In brief the need to get results fast, and by sedimentation from an ongoing process of discussion, evaluation, and reevaluation of data by a totally dedicated group of scientists, inevitably means that 'discovery' physics is, and must be, done at the host laboratory.

How is credit allocated in large teams?

There are three ways whereby scientists doing basic research conventionally gain credit for what they do: by publishing in the refereed literature, by speaking at conferences, and by impressing their colleagues and peers by their diligence and professional competence. Traditionally the first of these, publications, have been the most important means of assessing output and ability. However, with the growth in the size of collaborations, and their current policies for drawing up author's lists, other more 'subjective' criteria are coming to the fore, to the consternation of a physics community which finds itself trapped between past values and present realities.

Publication in the refereed literature is still the single most important goal of the researcher in basic science. The publication serves two main sociological purposes. Firstly, it is an indicator that the authors have, in the eyes of their peers, made a novel contribution to knowledge. As such, and particularly in an activity like basic science which is driven by competition, a publication serves to attribute priority to its authors for the results they have obtained. Secondly, publications are widely regarded as an 'objective' criterion of achievement in the field. As such, publishing articles is central to the functioning of a community which aspires to giving rewards primarily on the basis of scientific merit. Having one's name on a paper is thus of considerable importance to physicists.

How are author's lists drawn up? First, the usual basic distinction is drawn between constructing the detector and doing physics with it. The publications deriving from the former, which deal with technical innovations, are submitted to journals like Nuclear Instruments and Methods. Their author lists are relatively short and include only people who have been directly involved in the work described in the paper. There is apparently no great difficulty in settling authors lists for this kind of publication, as most physicists see such work as essential but relatively unimportant as a means of gaining credit amongst their peers – it is "considered by physicists to be a sort of second hand publication [...]" one of them said.

The situation is more delicate when it comes to publishing physics results. On the one hand, granted the work that they have done on it for many years, physicists want to have their names on papers deriving from 'their' detector. On the other hand, given the variety of results achieved with some of the 'multipurpose' facilities, they cannot possibly hope to be actively involved in all aspects of analysis. To satisfy these potentially conflicting considerations, collaborations tend to adopt a *policy of generosity*. They put the names of everyone involved in the collaboration for any length of time, who has made a significant contribution to its work, and who has a global understanding of the physics results reported, on every analysis paper. There are local variations within this scheme of course. Visitors or graduate students who were not involved in building the detector have to dedicate a minimum period – typically a year – to doing analysis along with the rest of the group before qualifying for author's lists. Some physicists who are highly specialized in one aspect of the work might only sign a subset of the papers. The very early papers might include the names of one or two people which will later disappear – the accelerator engineer Simon Van de Meer who shared the Nobel with Rubbia was given credit on the UA1 paper announcing the discovery of the W, and was then removed from the author's list. But the general rule remains unchanged. Any physicist who is seen to have made a significant contribution to any aspect of the collaboration's work signs every paper.[33]

[33] One interviewee mentioned that at one of the LEP detectors at which he now works there were 13 different physics topics distributed between some 400 physicists. All of them sign every paper produced even if they are working on a different topic.

By being generous in drawing up author's lists collaborations reduce to a minimum the potential for conflict which arises when people feel their names have been been unjustifibaly left off a paper.[34] In fact it appears that only about 5% of the names ultimately included are ever contested in a collaboration. The main source of difficulty concerns engineers and technicians. On the one hand many physicists recognize that some engineers and technicians have made important contributions to the development of the detector, and feel that they should duly be given credit for this on papers reporting results even if they are not really *au fait* with the physics. Against this it is felt that the proper place for engineers and technicians to publish is in journals like NIM which are dedicated to detector R & D, and that anyway a publication list is not as important professionally for them as it is for physicists – rewards are distributed differently in the different fields. As a result the consequences of putting engineers', and particularly technicians', names forward for authors lists can be so divisive that it takes a very determined group leader to push the idea through. As one interviewee explained, a publication bestows a very high status on a techician in his or her institute, and can lead to enormous friction not only inside the home institute itself, but with other institutes in the collaboration who are not putting forward technician's names.

Two last comments before we leave this point. Firstly, the ambiguity about including the names of engineers and technicians on physics papers is a consequence of the fundamental changes in experimental work that we are looking at in this paper. On the one hand it arises from the multidisciplinary character of the collaboration (see Table 1), from the fact that physicists, programmers, engineers and technicians work together over long periods of time around the same piece of equipment, all of them contributing in important ways to the final result. On the other hand, it is symptomatic of the changed role of the physicists themselves, of the blurring of the boundaries between the physicists and other professional categories.[35] To be a physicist in a collaboration of this kind is to master a number of very different techniques, techniques shared by computer scientists, by electronics engineers, by high–level technicians, and so on. The main criterion for having one's name on a paper reporting physics results may be that one is a physicist. The difficulties that we have just described arise because the notion of who is a physicist is itself contestable.

The second point worth noting is the confusion in physicists minds about the value of publications. On the one hand, they are extremely concerned to get the credit that comes from having one's name on a paper, and determined that justice be seen to be done in author's lists. This is because they cling to the traditional view of of the value of papers and, as importantly perhaps, because external assessors – fundgivers, faculty boards – still regard publication lists as an 'objective' measure of performance. At the same time there is a tendency for physicists to place less weight on the publication as a means of gaining reward. All those interviewed would agree that "publications count for very little" now, the credit one has being diluted by the fact that an individual is 'merely' one of tens or hundreds. The policy of generosity may avert conflict. But it imposes anonymity ("I don't even read the

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[34] Morrison (1978) at p. 359 writes that "authorship is one of the few areas where there can be serious friction and real unhappiness" in a collaboration.

[35] This point is developed more extensively in the following section.

authors lists" anymore, one interviewee said), and the obligation actively to seek rewards in other ways as well.

Conferences are the second main way for gaining credit in the physics community. They serve two important functions. Firstly, even though results are tentative and unrefereed, contributions to conferences serve to establish priority. They are particularly important in a field that is at once highly competitive and in which experimental data are thick with interpretation. On the one hand, physicists want to report their results quickly – indeed the week or two before an important conference are a time of feverish activity in a collaboration. On the other hand, physicists know that it can take a long time to converge on an agreed interpretation of their data, and for the community to accept them as reliable. Conferences are a way of resolving the dilemma, a way of presenting data fast without over–commiting oneself to them.

The second important function of conferences is as a forum for gaining *visibility* in the outside community of peers for both the individual and the group. Conferences are loci for making, or breaking, credit and credibility. One person is plucked from 'anonymity' in the collaboration and propelled into the limelight. At the same time the entire collaboration is given prominence and publicity.

That granted, collaborations obviously take considerable care choosing who is to speak at conferences, particularly when presenting their first results. There is a wide scope for diverse and conflicting interpretations of their findings in the early stages of their work. As a result it is deemed essential that highly competent, and confident, members of the collaboration speak at this phase. Of course such an opportunity further reinforces the power and prestige of senior physicists both inside the collaboration and inside the community – what Merton called the "Matthew Effect" is omnipresent in large collaborations. At the same time everyone knows that it would be suicidal to put a junior, or timid, member of the collaboration in the firing line when results are likely to be heavily contested. They have opportunities later, when the collaboration has established its credentials, when data and papers are flowing regularly off the detector, and when members are being invited and encouraged to speak at many conferences.

Granted the importance that physicists attribute to speaking at conferences – to the extent that sometimes they even contest the order of the programme –[36], it was striking to find that those interviewed were generally satisfied with the way talks were distributed. There are two main reasons for this. Firstly, neither interesting results nor conferences at which to present them are a scarce resource for large, successful collaborations, so that most people get an opportunity to speak on their work sooner or later. Secondly, physicists seem to accept with resignation the uneven, hierachical distribution of rewards inside a collaboration, to accept that some people regularly speak at the more important conferences than

[36] Taubes (1986) at p. 220 attributes these words to Rubbia, after the UA1 spokeman had been told that UA1 would present its results the day after UA2 at an important physics meeting: "If this is not changed", he allegedly told one of the organisers, "I do not think we go. This program makes us look like a spare wheel on a car. [...] Either we get basic symmetry of UA1 and UA2 in these subjects or we boycott the program. I don't see any other choice".

others. That 'resignation' is not a sign of subservience, though. Physicists inside the collaborations studied had clear ideas about the competence of their colleagues, and felt that it was in the common good that the best speakers presented the most significant results at major sites and meetings. It was good publicity for the individual. But also good publicity for the collaboration as a whole.[37]

The third and last way of gaining credit inside a collaboration is by making an individual contribution to an aspect of the collaboration's work. This could be anything from designing and commissioning an important piece of detector hardware to tackling a particular physics topic in an interesting and unususal way. The key thing is to do something which individuates you from the other members of the collaboration – and to ensure that other people in the group know what your contribution is. As one interviewee put it, there is no point having bright ideas if you do not tell others about them, and there is no point either in burrowing away on your own if no one else is aware of what of you are doing. In short, it is increasingly difficult inside large collaborations to gain recognition simply because one is a good physicist. One also has, to a certain extent, to 'sell' oneself, to make sure that one's efforts are visible to the rest of the collaboration. What you know matters. Who you know – and who knows you – also matters, and increasingly so.

Is teamwork antithetical to individual autonomy and creativity?

The 'factory model' of large collaborations reflects and reinforces another pervasive view about work in large teams: that it leaves no space to individual autonomy and creativity. Individual researchers, as Robert Wilson puts it, are conventionally seen as "doing creative, poetic, and enduring work [...]" while team research is regarded as "superficial, uncreative, and dull; [...]".[38] Mertonian sociologists would go further. Since "basic science is an individualistic enterprise", team research cannot be compatible with basic research.[39] As we shall see in this section, all of those interviewed confirmed Wilson's feeling that these attitudes are little more than "preconditioned responses" and "cliches". They persist because they are part of a constantly regenerated ideology which pivots around images of the scientist as an individual creative genius. They are increasingly irrelevant, not simply because they do not square with the realities of an individual's life in a large collaboration. More fundamentally, I shall argue, they are inappropriate because physicists working in such teams have a very different idea to their predecessors of only 20 to 30 years ago of what doing physics actually means. They draw – they have had to draw – the boundary between their activity as physicists and the activities of technicians and engineers in ways which are new, at least for Europe (as opposed to the USA).

But more of that later. First, let us try to capture what individuals working in the collaboration we studied felt about team research. While there were obviously

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[37] Cf Traweek (1988), who writes "Oral communication is fundamental to the operation of the particle physics community and successful senior physicists are masters of the form". (at p. 117).
[38] Wilson (1972), p. 468.
[39] See Hagstrom (1964), p. 241.

differences in emphasis between the respondents, one of them summed up the situation in terms which would probably be acceptable to all. "I feel sorry", he said, "that teams have become so big. On the other hand, we have to live with it. And [I would] say that we [have] managed a lot better than I could have [foreseen]". This attitude is confirmed by the findings of an American HEPAP subpanel who were also surprised to find that even young investigators were not disenchanted with teamwork. "We happily transmit the view from within large collaborations", the panel reported in 1988, "that – at least for many – life is far more challenging and far less anonymous than it sometimes seems to be from without, despite all the frustrations".[40] Teamwork then, is not fundamentally incompatible with individual fulfilment and job satisfaction, an observation which would surely be utterly banal and unsurprising but for the pervasive grip of the myth of the lone scientist.

The most basic reason why individuals do not feel crushed inside large collaborations is that there is a high degree of fragmentation and distribution of tasks (see Figure 1). As a result physicists find themselves actually working in small groups, sometimes of only five or six people, groups that will be responsible for a particular part of the detector or for the analysis of a particular set of data. Within these groups there is considerable scope for individual autonomy and creativity. In fact the detectors are so complex, and the data so profuse, that there is an enormous *variety* of work to be done: hardware R & D, electronics, computing, analysis... Ironically, then, and quite contrary to what conventional wisdom would have us believe, there can be *more* scope for individual autonomy in a large collaboration than in a small one.

That autonomy, of course, is not *a priori* guaranteed. On the contrary – and this is another reason why the reality of group research does not square with the myth –, individual physicists and institutions take deliberate steps to try to ensure that they are not dominated in a collaboration. They are careful about whom they team up with. As one university physicist in UA2 put it, he preferred to work in collaborations with five or six other groups rather than in a very large collaboration like UA1 because in that way "a rather smallish group as we were could have a major role". In similar vein the three British teams went into UA1 as "one strong group because we felt that we had to put up a united front and because we felt we would work better that way". Participating institutes also try to take responsibility for a crucial part of the detector as this will give them more weight , e.g. by building the trigger processors for the calorimeters in UA1 the UK groups were guaranteed a central role in the collaboration. Finally when it comes to data anlaysis, physicists do their best to ensure that they can work in an area which interests them. As one group leader put it, he had "always been very careful about the behaviour of my group inside the collaboration", making sure that "we are doing interesting things", not just building detectors for other people, but "doing our physics". In short, if physicists find that they have space for individual satisfaction inside collaborations it is also because they adopt deliberate strategies to protect their autonomy and that of their group.

So far I have concentrated on structural and strategic explanations of why work in collaborations is compatible with individual autonomy and creativity. There

[40] See HEPAP (1988), p. vii.

are also more personal considerations. Above all there is the pleasure of being involved in a collective effort directed towards a shared objective. This might mean working night and day with 50 or 100 people down in a humid and cold pit to assemble a detector as quickly as possible. Or it might involve spending hours with one's colleagues discussing the significance of the data coming off the device. These are aspects of group life which are simply not accessible to the individual worker or, indeed, to the worker in a small team.

This brings me to the last advantage of team research that I want to mention: that there are a large number of people available to discuss results during the analysis phase.[41] This is invaluable given that novel data off a detector are open to a wide range of diverse interpretations, and that convergence on a shared meaning requires an intensive exchange of ideas. By meeting with frequently with their colleagues – every day if they are working on a hot topic (cf. above) – the members of the collaboration slowly build a rationally justifiable version of the phenomena which they believe in, and which they can present to their peers as a "result". Seen in this light, group discussions surrounding data are not only satisfying to the individuals who participate in them. They are epistemologically essential.

What of the disadvantages of research in very large teams, what do the participants feel they have 'lost'. The feature most often mentioned by those who have worked in smaller groups is that they can no longer contribute to, and master, all aspects of the experiment. They are forced to specialise, and increasingly so as the teams get bigger. As a result they do not feel that they are 'in touch' overall with the equipment they are using, that somehow the detector and its data are out of their control.

We have argued above that doing experimental physics in a big collaboration can indeed be satisfying to individual participants. And as we have remarked, at one level this finding is banal, little more than a useful antidote against a number of clichés and "preconditioned responses" about the nature of team research. At the same time, from another, more interesting point of view, this result is of considerable significance. For it indicates that experimentalists in large collaborations have a conception of their role, of what it is to be a physicist, which allows that it can be creative and satisfying to spend four or five years – perhaps more – of one's life designing and building a piece of complex, heavy equipment, that that too is 'doing physics'.

This has not always been so, at least not in Europe. Certainly physicists have always understood that equipment was needed to do an experiment, and have often designed and built it themselves, perhaps with the help of one or two technicians. But this kind of work was done *quickly*, exceptionally in a few weeks (see the quotation at the head of this paper), more likely in a few months, at most perhaps in a year. After that they would get down to taking and analysing data, doing physics with a big P as the practitioners usually call it. However as the timescales for detector building have extended, and as physicists have become involved in all aspects of construction, so

[41] For the importance of constantly discussing one's results see Taubes (1986) Book II, and Traweek (1988)p. 117 et seq.

they have come to redefine what physics is (to the extent of being willing to give a PhD in physics to a graduate who works entirely on developing a piece of detector hardware). And to redefine their identity as physicists.

Of course these attitudes were not uniformally shared among those we interviewed. There was some nostalgia for the past. Therewas also the usual conservatism about the future: while it was "reasonable" to spend three to five years building a detector (as they had done), doing so for eight years, the time needed for some LHC and SSC devices, was "another thing". But the central image was clear:

Interviewer:	Did you yourself play a role in building equipment?
Physicist:	Yes.
Interviewer:	You stopped doing physics?
Physicist:	No. That's doing experimental physics.

The contemporary experimentalist's concept of 'doing physics' is not simply different, it is also obviously far broader and richer than that of his or her predecessors of only a generation ago. The following sequence of quotations give one an idea of what is involved. The physicist just cited was asked if he would not have liked to be taking data on another experiment while building the detector for UA2 (which took over three years of fulltime effort). He replied:

> No, I can't do that. I mean I really want to be, when I have an experiment to do, [involved] from the beginning. I can't do other things. [...] That's my problem. I mean in fact, when you design and build a calorimeter [...] you don't actually go blind into a certain design. You build a prototype and then you take this prototype to a beam and then you play with the beam and you change the components [...]. You design a system of flashlights which send artificial signals to the photomultipliers to keep the stability under control, and this requires writing a program that manages all this pulsing by computer, and writes files of calibration constants. Then you know you change the thickness of the lead and the scintillators to see how much you can influence the linearity [...].

This takes about a year, whereupon the design is frozen, and discussions with industry begin in earnest. Since the photomultipliers have to be very stable

> "you have to do a lot of searching among the various photomultipliers on the market to find out which one is the most stable. You have to discuss with industry. That's all physics. And then eventually you write technical notes and you publish in technical journals. Its not only screwing screws. Its development, its R & D."

Once the order is placed,

> "it takes a few months before you have the first pieces coming back for the assembly, and during that time you start thinking about physics again. You develop simulation programs, you write special physics routines which will eventually be used in the final analysis. And then when the things come back from industry, and they're assembled, then our calorimeters have to go back on test beams for calibration. [...] We spent a year [...] at the PS, calibrating everything in the calorimeter cell".

Building detectors, in short, involves a variety of activities and mobilizes a number of very different skills and techniques, all of which are now seen to be an integral part of doing physics, not a distraction from its main purpose, all of which are included in what it means to be a physicist.

Included too, as these quotations show, is a relationship with industry which was more or less foreign to European physicists working at CERN in the late 1950s and early 1960s. At that time it was the engineers, the accelerator builders, who were actively engaged with industry, who designed and built protypes, who exchanged knowledge and experience with their counterparts in firms, who pushed suppliers to the technological limit. For physicists, on the contrary, the relationship to industry was essentially passive. It was seen as a supplier of sophisticated though standard equipment, which was bought off the shelf and treated more or less as a 'black box'. This is no longer so. The relationship with industry is far more dynamic, interactive. Physicists now see it as a source of new ideas and techniques to be exploited and adapted to their novel purposes. CCDs, or Charge Coupled Devices, are a good case in point. Developed in the early 1970s , the technology was originally limited "to expensive and complex military systems". By the mid–1970s it appeared that the technology "may be on the verge of making a 'big splash into low–cost high–volume applications'". And an informal note was circulated inside the embryonic UA1 collaboration explaining their potential for "use with charged particle detectors".[42] Put differently, the concept of being a good experimental physicist now includes being aware of what new products industry, and especially high-tech industry has to offer, and of being able, as Dominique Pestre put it, "to use industrially available material in new and interesting ways".[43]

This new identity, these new attitudes among European physicists, are in fact indicative of a generalization of the role of the physicist which emerged in the United States between the 1930s and the 1960s. Basic science was transformed in this period, above all by its integration into the military–industrial complex. A new way of doing physics emerged, a new kind of researcher was moulded, a researcher who, to quote Pestre again, "can be described at once as physicist i.e. in touch with the evolution of the discipline [...], as conceiver of apparatus and engineer, i.e. knowledgeable and innovative in the most advanced techniques [...], and entreprenuer [...]", i.e. capable of mobilizing and managing important human and material resources.[44] Until the early 1960s this transformation in the role of physicist was restricted to the United States, where it was embodied in the activities of men like Luis Alvarez: European physcists were largely excluded from it. But then a new generation came on the scene, the men and women of whom we are speaking here. They completed their PhDs in the early 1960s. Most of them have spent at least two or three years working in the States. And – competition *oblige* – they have internalised the role of a physicist which working in large collaborations around big detectors demands of them.

In our interviews there is another, interesting symptom of the internationalisation of the 'American' conception of what it means to be a physicist. It is the view that physics is fun. In fact it is striking that those we spoke to hardly if ever assessed their experience in large collaboration in terms of the space allowed them for 'creativity' or for 'freedom to follow their own ideas'. These concepts are more or less

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[42] The remarks about CCD devices are from P. Davies, B. Hallgren and H. Verweij, *Short Study of the Charge Coupled device CCD 321*, ppbar Note 31, 5/9/77 (JBA22633–CERN).
[43] D. Pestre (1990), chapter 13.6, which contains a general discussion of the difference between the American and European ways of doing physics in the early 1960s. See also Pestre & Krige (1988)
[44] Cf previous note.

irrelevant, relics of a bygone ideology, appropriate to the impoverished poetic genius of myth, not to the hardnosed professional of reality. For them what counts is having fun. This 'hedonism', Forman has argued, emerged in the USA in the late 1950s, where it was at once indicative of the new social niches being filled by physicists, and of their rejection of the old idea of themselves as morally superior beings.[45] Its implantation in Europe is yet another indicator that the Old Continent has, at last, 'caught up' with the New.

While at one level this notion of fun, admittedly vague, apparently means the satisfaction which comes from playing with new ideas, it seems to have another significance for those working in teams. It refers to the quality of life in the collaboration. For one physicist it was what was lacking in UA1, undermined by the ever–present danger of a bruising conflict with the spokesman. As he put it, thinking back over his thirteen years in the collaboration, "it was exciting but it should have been more fun". Fun is what one has with others, and it is based on building up meaningful and durable links with colleagues. These links are established through spending minutes and hours, days and nights, months and years working together around one piece of equipment. They are the result of hard work and dedicated collective effort. They are reinforced at countless collaboration meetings, workshops, summer schools, and conferences, many of them in exotic places. And they require an atmosphere which leaves space for individual freedom and for collective play and relaxation. They are the backbone of a community which is concentrated more and more at a few research sites around a few huge detectors. And whose solidarity and internal organization are so formidable that they are able to raise, and to go on raising, the money that they need to do increasingly expensive physics – and to have fun.

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[45] Forman (1989). For other interesting remarks on how the postwar transformation of the field has changed the attitudes of physicists, see Holton (1985).

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