Physics policy

Four of the major contributors to the discussion on 'Self-control in physics ?' at the Oslo Meeting of EPS Council have written to *Europhysics News* to set out the points they made. Our readers are invited to join in the debate by writing to *Europhysics News* with their views on any aspect of physics policy to which EPS might address itself effectively.

C.M. Braams (The Netherlands) writes:

Ganzhorn's provocative discussion paper rightly brings to the forum of the E.P.S. the issue of the role of science and scientists in our changing society. I applaud this inititiative and I will gladly try to make a small contribution to the discussion he has initiated.

What he asks is that a body of scientists takes control of the allocation to different branches of science, of the money available for scientific research, the total level of support having been pre-determined at the political level in terms of a percentage of gross national product. I do not think that what we need is such a division of responsibility between politicians and scientists, but rather a better integration of research in the process of political decision-making. If we are to establish weight factors to high-energy, solid state and other disciplines of physics, as Ganzhorn asks, we have to apply criteria relating to cultural, material, psychological, and other needs of society. We then find that scientists have no special qualifications for making such judgements, and that this is what politics is about. What scientists can rate better than anybody else is scientific merit, not social relevance.

An all-encompassing science policy would pretend to strike a proper balance between, say, research on the role of viruses in cancer and research on radiation damage in nuclear reactor materials. I am not aware of any scientific body that has successfully implemented such a science policy, nor do I feel that we need one. I would rather see medical research balanced against other measures to improve health and, likewise, see applied nuclear research as part of an integrated energy policy.

I would like to suggest that EPS set aims for itself that are less ambitious than those proposed by Ganzhorn:

- Improve the public image of research;
- Make scientific progress understandable for the public (including the politicians);
- Make scientists aware of where they can contribute to the solutions of urgent problems;
- Avoid claims of disproportionate responsibility (that is, power) for scientific bodies;
- Avoid polarization between science and politics.

J. Friedel (France) writes:

We must thank Ganzhorn for raising what are surely fundamental questions about the activity of EPS. It seems to me, however, that physicists are already more involved in the planning of research than he implies, and also that, at least in the immediate future, the activity of EPS should be channelled towards goals that are modest although already very useful.

It is clear to me that the very general questions about research involve political decisions that cannot be decided, let alone discussed, by scientists alone. I am thinking first of the relative weight to be given to research and development in the overall human activity, that is, the percentage of gross national product affected. I am also thinking of the relative weight to give to fundamental versus applied research, and of the relative weight to give to various applied fields. Even major decisions concerning fundamental research such as whether to develop in preference say physics or biology, or even nuclear physics, space research or solid state physics actually involve economic and even political consequences which cannot be ignored.

I do think that physicists are involved in such high level discussions in many European countries, perhaps more in some (see OECD report, 24.9.71) than in others. In France, for instance, there is always at least one physicist in the '12 sages' that permanently counsel the government on research and development. The organism charged to prepare and implement the five yearly plans for research is headed by a physicist, and many of them sit in the various commissions which actually prepare these plans.

One can certainly criticise the way things are running and the actual role of these organisms or of the physicists in them. I do not think EPS as such could have a very active role here, in the immediate future, contrary to what is suggested by Ganzhorn :

> there is no political framework in which physicists could be brought together with other scientists or non-scientists for discussions.

> if EPS tried to build up a body of permanent scientific experts, there is a clear danger that its advices would not be necessarily adapted to the present situation of a given country, nor that they would be followed by that country. We must avoid the pitfalls of Euratom !

Within each broad domain of physics, it is clear that physicists are very much involved in the planning and control of research. The OECD report mentioned above compares the way things are done in Britain, Germany and France, and concludes that despite apparent differences, the main characteristics are much the same : for fundamental research, balance between general long-term financing and short-term specified contracts ; balance too between university and non-university laboratories; distribution of finances through committees of specialists.

What can then EPS do in the immediate future ? I believe that it can play on the European scale very much the same role of general information and reflexion that our national societies play at a more local level. I suggest the following would be useful:

(a) a short analysis for each European country of how much money is given to research and development in all, and especially to physics ; how much for fundamental physics ; how much in various broad domains, with the number of physicists employed. The last analysis could be complemented by some divisions.

(b) a short analysis for each European country of the mechanism of planning, distribution and control.

(c) preparation by small selected committees of prospective scientific reports on research within divisional or subdivisional domains.

To be useful, (a) and (b) should be short ($\leq$ one page per country) and done within six months. (c) would take time and care.

R. Press (UK) writes :

The lively two-hour discussion, among Council members at Oslo demonstrated how successful K. Ganzhorn had been in stimulating discussion among physicists in Europe on the future of physics research and research planning. Indeed the interest and scope of this informal discussion suggested a healthy readiness on the part of EPS to grapple with the problems outlined by Ganzhorn, and provided encouragement for those who had advocated a place for a 'Physics and Society' topic in the programme for the Second EPS General Conference in Wiesbaden.

Views expressed at Oslo ranged from a 'do-nothing' attitude, other than simply demand funds for research and leave all associated or consequential problems to politicians, to the need for a strategic research planning body so specialised and institutionalised as to have no non-specialist counterpart for an effective dialogue. It was more generally suggested that physicists should concern themselves with such problems, as indicated in the Ganzhorn article, but that they should seek to represent their individual viewpoint within their respective environments and advise, wherever possible, on local problems. The issue was seen as one of degree and method ; in so far as total science budgets were concerned the physicists' viewpoint should only be one of a number of viewpoints leading to ultimate decision.

An important question was whether scientists really should be responsible for ultimate policy decisions. Many such decisions must in the end be political but these should be reached with fullest scientific advice, and politicians should at the same time become increasingly aware of the scientific factors in the decisions which were their responsibility. Some doubts about the success or ability of a purely scientific body to control and allocate research funds to competing interests, were answered by reference to the success of the Science Research Council in the UK. Incidentally, in this connection, some misapprehension about the scope of the Rothschild proposals in the UK was corrected. It was pointed out that these proposals recommended that a 'customer/contractor' principle be adopted only in respect of applied research and development commissioned by the Government. Subject to this principle they recognised a continuing need to preserve the existing Research Councils under the sponsorship of the Department of Education and Science. For the time being at least, the work of the Science Research Council would not be subject to this principle.

It appeared that the task of EPS was not to seek institutional responsibility for social or political decisions. They would find as much difficulty as do national societies in reaching general agreement on particular issues. But they should seek to make their work more understandable to the general public and to help politicians to deal with those issues involving scientific factors. They could help by providing advisers where necessary ; they should not isolate themselves from either the decision-making machinery affecting their professional work nor from the concern of those affected by the consequences of their work. In sum, the discussion revealed a healthy recognition of the fact that the time for proceeding by the strongest lobby is no longer enough, and that the time for an associated responsible attitude towards society at large has arrived.

J. Volger (The Netherlands) writes :

The main points of my contribution to the Oslo discussion were the following :

I expressed my belief that the public at large will ultimately not appear to be against science as such, including so-called basic research, if only there would be a reasonable information on it and not more than a decent amount of money spent.

Furthermore, I remarked that all problems discussed by Ganzhorn and the Council had also been discussed by the Advisory Committee on Physics and Society and I quoted from the working paper which the committee will publish : ' the Committee recommend that — at top corporate and government levels — effective co-ordination functions be established to strengthen interdepartmental links, to set-up strategic capacities for problems analyses, to stimulate integrated planning and programming of R and D efforts, and to facilitate the implementation of scientific progress.'

To mark the award of the 1971 Nobel Prize in Physics to Dennis Gabor, this abstract is produced with the permission of the Nobel Foundation who have copyright of the full text.

Holography 1948-1971

Abstract of Nobel lecture, 11 December 1971, by D. Gabor

Dennis Gabor opened his lecture with a statement about the essential nature of holography: 'I have the advantage in this lecture, over many of my predecessors, that I need not write down a single equation or show an abstract graph. One can of course introduce almost any amount of mathematics into holography, but the essentials can be explained and understood from physical arguments.'

In 1947, Gabor had been very interested in electron microscopy which was disappointing because it had stopped short of resolving atomic lattices. The practical limit stood at about 12 Å. Gabor described how his invention came about:

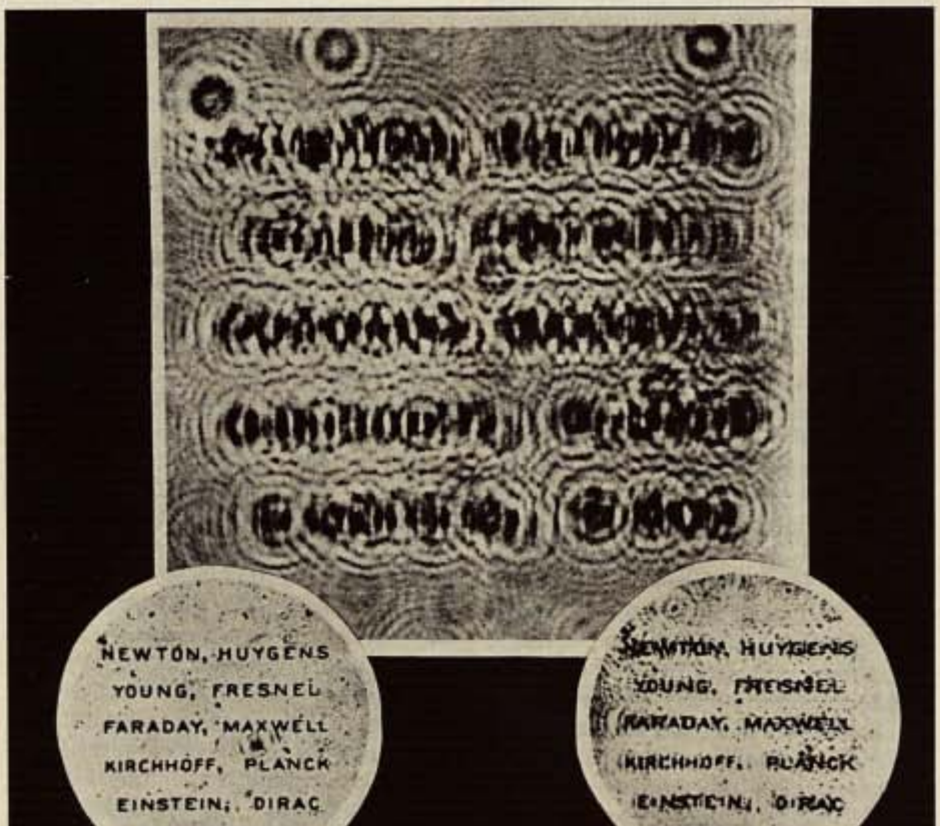
'After pondering this problem for a long time, a solution suddenly dawned on me, one fine day at Easter 1947. Why not take a bad electron picture, but one which contains the whole information, and correct it by optical means? It was clear to me for some time that this could be done, if at all, only with coherent electron beams, with electron waves which have a definite phase. But an ordinary photograph loses the phase completely; it records only the intensities. No wonder we lose the phase, if there is nothing to compare it with! Let us see what happens if we add a standard to it, a 'coherent background'. For the simple case when there is only one object point, the interference of the object wave and of the coherent background or 'reference wave' will then produce interference fringes. There will be maxima wherever the phases of the two waves were identical. Let us make a hard positive record, so that it transmits only at the maxima, and illuminate it with the reference source alone. Now the phases are of course right for the reference source, but as at the slits the phases are identical, they must be right also for the object point; therefore the wave of the object point must also appear, reconstructed'.

Gabor explained that he called the interference pattern a 'hologram' from the Greek word 'holos' meaning whole, because it contained all the information. In devising the optical system, Gabor acknowledged that he had stood on the shoulders of W.L. Bragg and F. Zernike. Figure 1 shows one of his first holographic reconstructions. The systematic defect in these first pictures was that there was not one image but *two*, and Gabor set out to defeat the second. He achieved his purpose by taking an electron hologram with a lens with spherical aberration, so that he could afterwards correct *one* of the two images by suitable optics; the other had then twice the aberration which washed it away completely. Figure 2 illustrates the improvement obtained. Gabor paid tribute to the early workers who responded to his first papers on wavefront reconstruction — G.L. Rogers, A. Baez, H. El-um, and P. Kirkpatrick.

It was at this point that Gabor hit the only note of regret in his Nobel Lecture. With his collaborator, W.P. Goss, he had constructed a holographic interference microscope, in which the second image was annulled in a rather complicated way by the superposition of two holograms 'in quadrature' with one another. However, the response of the optical industry to this achievement was so disappointing that the paper by Goss and Gabor was not published until 11 years later in 1966. During that time holography went into a long hibernation.

Gabor went on to report on the revival that came suddenly and explosively in 1963, with the publication of the first successful laser holograms by E.N. Leith and J. Upatnieks. The superior results of Leith and Upatnieks could be directly attributed to the availability of the laser which enabled them to use the 'skew reference wave' method to eliminate the second image. (Incidentally, Gabor claimed that he had thought of the principle of the laser in 1950 but had been unable to persuade his best Ph. D. student to tackle the problem — because he could not *guarantee* to find a suitable crystal!)

From 1963, progress became very rapid, and Gabor sketched the highlights of the development of holography since then. First, he described the production of diffused holograms which are complicated figures, the diffraction patterns of objects, which



An early hologram by Gabor.