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G. HOLST AND HIS IDEAS ON INDUSTRIAL RESEARCH

by H. B. G. CASIMIR

Gilles Holst, founder and first director of the Philips Research Laboratories at Eindhoven, died on October 11, 1968, at the age of 82. To the scientific community at large this may mean little: even in the Netherlands outside the Philips works the younger generation of physicists will remember his name only vaguely. Many of his coworkers, like Van der Pol, pioneer in radio research, or Penning, authority on gas discharges and inventor of the PIG (Philips Ionization Gauge) made more lasting contributions to scientific literature. Yet all those who had the privilege of working in the laboratories Holst created will remember him as a great and inspiring leader.

Holst was not a man to write extensive papers on research management (or indeed, on any other subject). Not that he was reticent about his ideas, but they are to be found scattered in lectures and addresses and in aphoristic remarks remembered and treasured by his coworkers.

In this paper I shall try to present some of these ideas in the context of a short biography.

Gilles Holst was born at Haarlem on March 20, 1886. His father was manager of a shipyard and after finishing secondary school Holst worked for a year as an apprentice in this enterprise and in one or two factories. Then he went to Zürich to study mechanical engineering. After a year he changed over to the department of mathematics and physics. "I did not like cast iron" he told me later. To him cast iron in those days was the prototype of a material the properties of which were insufficiently understood and controlled, so that mathematically beautiful calculations of strength had to be impaired by large and fairly arbitrary safety factors.

The wish to understand and control materials was one of Holst's guiding principles throughout his active life. Let me quote from an address given shortly after the second world war: "For many years the gap between what we know about the atoms of metals and what we know about the properties of metals seemed unbridgeable. But that is now changing. I recall clearly how deeply I was impressed by a paper of Mott on the magnetic properties of nickel alloys. Reading that paper I experienced a shock. To me it meant that the time was getting ripe for tackling the study of metals and alloys in a big way and for bridging the gap I mentioned. Did not things happen in just that way 25 years ago? Then the understanding provided by Bohr's work had shown the way to unravel the complicated phenomena of electrical discharges in gases. The problem of metals is probably even more difficult because of the great influence of structure on most properties but I would not be surprised if one would get it under control during the next 15 or 20 years. And that is of greatest importance to nearly all branches of industry."

In 1908 Holst obtained the degree of "geprüfter Fachlehrer" (certified professional teacher). He stayed one more year at Zürich and then became assistant to Kamerlingh Onnes at the low-temperature laboratory of Leyden university where he remained until the end of 1913. Helium had just been liquefied and Holst collaborated in the discovery and early investigations on superconductivity. Although this role was acknowledged by Kamerlingh Onnes, for instance in a confidential recommendation for an important appointment, and although tradition has it that Holst was the first to observe the phenomenon, little of this appears from published papers. In a way this is understandable: Kamerlingh Onnes had started a programme to investigate the change of the electrical resistance of metals with temperature down to helium temperatures, and mercury was one of the first metals on his list. The discovery of superconductivity was in a way an unavoidable consequence. Yet we always felt that Holst ought to have received more credit for what is after all one of the most surprising phenomena of solid-state physics. Holst himself reacted in a characteristic way: Later, as a director of research he was extremely conscientious and generous in giving due credit to his staff and if he appeared as co-author of a paper one could be sure that he had made very essential contributions. And there are numerous examples — magnetic ferrites are a case in point — where Holst was the real originator of the work, although his name does not appear.

During his Leyden years Holst also assisted Mme Curie with some experiments on the possible influence of temperature on radioactivity. But his main work was in the best of the old Leyden traditions and dealt with the equation of state and the thermodynamic properties of ammonia and methylchloride; this formed the subject for his thesis at Zürich (1914). It was a solid piece of work but hardly in line with Holst's personal preference: he had no special liking for precision measurements and even less for least-square fitting of semi-empirical formulae to experimental data.

In 1914 Holst went to Philips at Eindhoven with the assignment to start physical research. [The Philips company was by then a reasonably prosperous incandescent-lamp works. It was managed by the two brothers Philips, Gerard, an able engineer and manufacturer, and Anton, who handled the business side, and who was a man of outstanding width of vision *).] The invention of the halfwatt lamp and the role of research at General Electric had made a great impression at Eindhoven and it was felt that the company would be unable to hold its own without research at a comparable level. It was Gerard who hired Holst, but it was Anton who later gave him much needed support and backed his ideas even though they were at variance with what was then — and sometimes now — considered sound industrial practice. Soon Holst was joined by Oosterhuis who throughout the years was an extremely valuable second in command, adding constructive criticism and sound detail to Holst's imagination. By and by the company grew and moved into new fields. So did the laboratory. In 1923 they moved into new premises. The depression brought a slight decrease of staff, but Holst was able to avoid a drastic cut down. At the outbreak of the second world war the laboratories had a total personnel of roughly 400, among whom 100 were university graduates.

At first Holst did much of the work himself. He had to assist manufacture in many ways and to establish reliable photometric methods. Next he turned his attention to gas discharges. He also studied the properties of tungsten, the sputtering of metals and the properties of thin layers. In these first years he wrote an impressive number of publications. He was one of the pioneers of image transformers. Teves, co-author of the first paper on infrared tubes, later applied the same idea in his image intensifier which came to play an important role in X-ray diagnostics.

In later years Holst's personal publications are less important but all of us knew that he was the driving force behind much of the work.

How Holst managed to get the laboratories through the occupation years of the second world war is a story by itself. Somehow he convinced the Germans that he was working for a bright and distant future and escaped in that way doing work of military importance. Of course there were hectic moments, razzias for people who had found shelter in the laboratories, investigations on programmes and so on, and Holst himself spent some time in a prison camp. The liberation in September 1944 came just in time: the occupants started to ask for results. In 1946, when Holst retired, the laboratories were again in full swing **).

During the next ten years Holst remained active as a member of the board of the Philips company, as a chairman of several government committees and as a "Curator" of the Technical University Delft, where he only partly succeeded in his fight for reforms (streamlining of curriculum, shortening of time of studies,

*) Cf. P. J. Bouman, Anthony Philips, Weidenfeld and Nicols, London, 1958.

**) Readers interested in more historical details may turn to: "An Anthology of Philips Research", edited by H. B. G. Casimir and S. Gradstein, which contains a number of papers on the history and the work of the Philips Laboratories.

etc.). In later years one saw him only rarely at meetings but his interest stayed alive until shortly before his death.

I shall now try to analyse some of Holst's guiding principles. When Holst came to Philips he had no special experience or knowledge concerning light sources and lamps. He was a well-educated, versatile physicist. That was the type of man Holst himself tried to find later. Usually he did not look for specific skills; he might ask a Ph. D. in organic chemistry to tackle work on oxidic semiconductors or a Ph. D. in low-temperature physics to dive into T.V. circuitry, and in that way could bring a variety of competence and abilities into the attack of specific problems.

One might argue that in the twenties it was still possible to be an allround physicist, an applied mathematician and an engineer all in one but that today specialization is unavoidable. To a certain extent this is true, yet at Philips we continue to pick new staff for general competence rather then for specific skills. I was asked the other day whether I would hire a Ph. D. in nuclear physics. My answer was affirmative; as a matter of fact the plumbicon T.V. pick-up tube, one of our most successful developments of recent years, was to a large extent the work of a Ph. D. in nuclear physics.

Related to this is the fact that Holst rarely gave his staff accuretely defined tasks. He tried — and usually succeeded — to make people enthusiastic about the things he was enthusiastic about; he knew that one can order someone to sweep floors, perhaps even to perform routine measurements or routine calculations but that one can hardly expect really creative work from a man who does not believe in the project that he is ordered to undertake.

Anyway Holst did not believe in a strict hierarchic structure. I quote from his inaugural address at Leyden, where he was a "professor extraordinary" from 1929 to 1939: "If organization is carried too far, it leads to a narrowing rather than to a widening of our vision. Another serious disadvantage is that one cannot take into account the personal characteristic of the individual researchers of whom especially the most original ones have difficulty to adapt themselves to an organization."

As the Philips Laboratories grew and we had to tackle bigger projects, we, Holst's successors, have found it unavoidable to institute a somewhat more elaborate organizational structure. Perhaps a man like Holst might have managed without it, even today. But we try to remember that such a structure is a necessary evil, not a goal in itself and that it is our first duty to make it possible for really original scientists to work in our laboratories.

In those cases where a job was too much for one man so that a team had to be created, Holst tried to create multidisciplinary teams. This is now many years later becoming fashionable in educational centres, for instance at M.I.T. Holst definitely refused to divide his laboratories into chemistry, physics, engineering, etc. A team working on, say, magnetic ferrites, would comprise physicists, but also chemists, crystallographers and electrical engineers (although these might be "on loan" from other groups of the laboratories).

He was convinced that one should publish; this is made possible by the existence of patents. "Publication of results has another great advantage, the possibility to have first-class scientists working for you. I am convinced that many of the very best physicists that are now working in industry would have changed position long ago if they had had to keep their results secret."

So much for his relations with his staff. In his dealings with the product divisions, he insisted on a high degree of independence of the research laboratories: they reported to the top management of the company, not to the divisions. Of course divisions could formulate problems, and in really urgent cases Holst was always willing to muster a team to come to the rescue, but the final decision rested in practice with Holst and his staff. Only in very rare cases would top management — of which Holst was part — give definite directions. The final stages of product development and the day-to-day trouble shooting had to be done by the factory laboratories, but he was always willing to encourage the transfer of suitable staff from research laboratories to development laboratories. Holst had to fight pretty hard for his freedom. Today we are convinced that in a highly diversified multinational firm like ours it is the only viable solution.

He was strongly against detailed budgetting and even more against regarding expenditure on research that leads to a specific product as initial costs for that specific product, but he always carefully considered whether it was justifiable to have so and so many men working in a specific field *).

All these principles would, however, have been of no avail without Holst's extraordinary gift for entering a field of research at the right moment. The quotation at the beginning of this article shows clearly that he believed that real progress could only be made on the basis of understanding. Gas discharges on the basis of Bohr's theory of the atom, solid-state phenomena on the basis of quantum mechanics.

He started work on gas discharges soon after coming to Philips. The laboratories made important contributions to their understanding; superhigh-pressure mercury lamps, the PIG and the sodium lamp were among the practical results.

Radio valves were for Holst not entirely unrelated to gas discharges. Soon he realized the enormous potential importance of radio and electronics.

Solid-state work was seriously started in the thirties and then particularly successful in the field of magnetism. And he became interested in nuclear physics after the discoveries of Cockcroft and Walton, Joliot-Curie and Fermi.

Perhaps his choice of specific development projects was not always as for-

*) I have elaborated this point in one of the articles reproduced in the Anthology quoted.

tunate as that of fields of research. His feeling for immediate market potential was occasionally at fault, whereas his sense for trends of scientific and technological development was unrivalled.

It is fashionable these days to distinguish between "phenomena-oriented" and "mission-oriented" research. In the case of Holst I wonder whether the distinction is much to the point. Holst was definitely interested in phenomena and their theoretical explanation, but at the same time he was always thinking about applications. He became interested in phenomena when he began to see a possibility of harnessing them for industrial purposes.

Holst's principles might be summarized in the form of ten commandments:

(1) Engage competent scientists, if possible young, yet with academic research experience.
(2) Do not pay too much attention to the details of the previous experience.
(3) Give them a good deal of freedom and give a good deal of leeway to their particular idiosyncrasies.
(4) Let them publish and take part in international scientific activities.
(5) Steer a middle course between individualism and strict regimentation; base authority on real competence; in case of doubt prefer anarchy.
(6) Do not divide a laboratory according to different disciplines but create multidisciplinary teams.
(7) Give the research laboratories independence in choice of subjects but see to it that leaders and staff are thoroughly aware of their responsibility for the future of the company.
(8) Do not try to run the research laboratories on a detailed budget system and never allow product divisions budgetary control over research projects.
(9) Encourage transfer of competent senior people from the research laboratories to the development laboratories of product divisions.
(10) In choosing research projects be guided not only by market possibilities, but also by the state of development of academic science.

It may be remarked that those principles are established years ago and that time is changing. This is true, and some modifications in organizational structure have been necessary. But the principles as such remain valid. Of that my collaborators and I are strongly convinced.

While writing this article the image of Holst as we knew him has constantly been in my mind. To recreate that image also for those who did not know him, to describe his persuasive enthousiasm, his quick and witty repartee — and his lack of formal oratorical skill —, his way of approaching people — essentially kind but wavering between shyness and bluntness —, is beyond my skills as a writer. Holst himself used to say that he was more interested in things than in people. I wonder whether this was true. Perhaps he realized that setting objective goals was the best way to encourage people and even to help them overcome personal difficulties. He created good working conditions for his staff and during the war he took considerable risk to keep them out of the hands of the Germans. We, who succeeded him, admire the things he did, but above all we treasure the memory of a noble, unselfish and inspiring man.

Eindhoven, May 1969