"What has Science Policy done for me – What have I done for Science Policy?"

By Heinrich Ursprung (February 1997)

As the title of my contribution suggests, this will be a case study. To introduce the first of the two questions, I would like to read a document to you:

"In accordance with our conversation I would like for you as Chairman of the Space Council to be in charge of making an overall survey of where we stand in space:

1. Do we have a chance of beating the Soviets by putting a laboratory in space, or by a trip around the moon, or by a rocket to land on the moon, or by a rocket to go to the moon and back with a man. Is there any other space program which promises dramatic results in which we could win?
2. How much additional money would it cost?
3. Are we working 24 hours a day on existing programs? If not, why not? If not, will you make recommendations to me as to how work can be speeded up.
4. In building large boosters should we put our emphasis on nuclear, chemical or liquid fuel, or a combination of these three?
5. Are we making maximum effort? Are we achieving necessary results? I have asked Jim Webb, Dr. Weisner, Secretary McNamara and other responsible officials to cooperate with you fully. I would appreciate a report on this at the earliest possible moment."

The document is typed on The White House, Washington, letterhead; dated April 20, 1961, entitled "Memorandum for Vice President", and signed **John F. Kennedy**. A similar document must have existed on conquering cancer. I do not know that document, or else I would have read it too. For it enabled the US Atomic Energy Commission and other agencies to launch important programs on cancer research. I learnt about this in an advertisement in the weekly magazine Science, where two postdoctoral fellows where sought who felt qualified to work on a project of cell differentiation, led by Drs. **Benjamin Harrison Willier** and **Clement L. Markert** at the Biology Department of the Johns Hopkins University in Baltimore; the emphasis was to be on the use of multiple forms of enzymes as indicators of gene-function, and the role differential gene regulation might play in the control of cell differentiation.

I believe it is fair to say that the Kennedy Administration set two clear priorities in its science policy: space, and cancer. Obviously for different reasons. To land a man on the moon (which happened) meant success in beating the Soviets; it was a component of the Cold War, it had to do with national pride and foreign policy. To conquer cancer was a noble ambition (which led very far into the problem) in health care primarily at the home front, it had to do with domestic policy. At least one effect was common to both initiatives: they created thousands of jobs for scientists. For me as a young scientist who had just finished his Ph.D. work, this was a very important effect indeed; US science policy gave me a job opportunity.

Four years earlier, Swiss science policy had already given me a job opportunity. In Switzerland the National Science Foundation for the Promotion of

Research had been created in 1952; my Ph.D. father Ernst Hadorn had sent in a proposal for research on immaginal disks of Drosophila, and some of the money he had gotten for this saw me through the three years between 1956 and 1959 when I worked on my Ph.D. thesis. In terms of procedures, there were differences between the two cases. The Swiss National Science Foundation had let it be known that whoever submitted a convincing proposal in whatever field of science, would get support; the author of the proposal was entirely free to choose his field of interest. The US Atomic Energy Commission hat let it be known that whoever submitted a convincing proposal of research related to cancer would get support; the author of the proposal was not entirely free to choose his field of research, since it had to be related to cancer. As a scientist working at the lab bench, I did not notice this nuance. Nor was there a difference between the two situations with respect to the level, or category, of research: in either situation, it was basic research, aiming at enlarging a body of knowledge. No application was intended of what we did, or found (I shall come back to this point later on). I would bore you stiff if I went into our research work itself, so I will not. But it went well, and I was moved up the academic ranks. In that process, I learnt a lot more about science policy, not of the macro-type (space or cancer, at the national level), but at more local levels. I shall give two examples.

First Example: Where Should we Set our Mark?

Three stars in the field of enzyme function had been called away to other Universities for their excellence that had helped our Department become so well-known. We junior fellows felt three successors ought to be found who could help maintain the Department's excellence in that particular field, i.e., enzyme function. **William D. McElroy**, our Chairman, felt and decided otherwise: he hired successors not in enzyme function, but in protein structure, out of a conviction that understanding function required knowing structure, and with the ambition for the Department not only setting standards in enzyme function ("we have left our mark") but also protein structure ("let us set another one"). His decision was far less trivial than it sounds; for one thing, it cost a lot, because the equipment needed in the field of protein structure was expensive. But more importantly, it meant that choices were made, by the Department Chairman.

Second example: Different Approaches in Setting Priorities

The same Department Chairman made another choice when he decided to make Developmental Biology a priority in the Department, one of only five. Obviously, there were different approaches to split the Life Sciences: by groups of organisms (virology, bacteriology, botany, zoology; this was the policy then in many European universities), or by levels of organization (molecular biology, cell and developmental biology, organismic biology; this was the policy then in some US universities). There might have been some enlightened opportunism in the head of our Chairman when he decided on Developmental Biology to become a priority: the US National Science Foundation had just announced it would support convincing training programs for Developmental Biology; actually, a program of Developmental Biology had been added to the NSF structure, headed by a Director, who became a bona fide colleague of the Program Director of Genetics,

Biophysics, and others. Furthermore, the NSF had widened its policy of support into supporting not only convincing research projects, but also convincing training projects, and our Chairman felt the Department ought to grab this opportunity and create a Ph.D. program in this important field of science. A few colleagues and I formulated a program and received generous grant money that allowed us to recruit 25 graduate students in Developmental Biology year after year.

This was a particularly challenging undertaking, because we had 100 applications, year after year, of bright young people who wanted to join the program. Knowing that roughly 50% of them preferred similar programs, at Harvard, Yale, and Caltech and would often reject our offer, we offered admission to about 5O candidates, receiving some 25 acceptances. Universities competed for the best students and had to compete with other Universities. Ranking of course went along with this, evaluation of programs by site-visits, evaluation of professors by peers in view of promotions - some of this overshot to the point that a well-known colleague projected the situation into one where at any given time more than 50% of the entire scientific community of the US would be in the air, on the way to site-visiting each other. Now that I can look back on what became of the students we had at the time, I am gratified, to say the least: those had been wise decisions of science policy, both at the level of NSF and JHU.

Thus, both Swiss and US Science Policy have given me a lot during those 13 years between 1956 and 1969: jobs and infrastructures for research in fields of my choice (no one had forced me to join **Hadorn**'s or **Willier**'s and **Markert**'s groups; I had wanted it, had grabbed the opportunity to apply, and succeeded with the application); opportunity for growth, for building up my own group in research and teaching in fields of my own choice (no one had forced me to stick to Developmental Biology as a field of interest, or to diversify into nucleic acids, electron microscopy, and fertilization biology as I did; I had wanted it and grabbed the opportunities to compete for the respective support). Of course at times I felt my field of interest or my lab deserved more grant money by NSF or more space by JHU than we had, and cursed about the forms that had to be filled out, the interim and final reports that had to be submitted, in short about research bureaucracy. But these were quantitative, or management, in nature.

And Now Let's Talk About Costs

Let me shift to the second question, now. When I was put in charge of ETH Zurich, on October 1, 1973, this institution was in a phase of rapid, substantial expansion. Not only was the new campus on Hönggerberg under construction, not only were the builders busy in many of the downtown buildings including the Main Building, but personnel growth and the growth of spending-money had been substantial. But clouds were in sight. Growth was soon stopped, not on the investment side, but on the operating budget. Since personnel costs at a University constitute some 75% of operating cost, to stop growth means to stop growth of personnel. At the time one did not know how long the decision by Swiss Parliament not to hire someone on an

existing personnel post unless someone else had left his personnel post would last (it was to last over a decade). Nor did we know precisely how the student body would grow under these circumstances (it was to grow by some 30% in the decade). Nor did we know which burning needs would become apparent for the establishment of new disciplines at ETH (it turned out to concern the introduction of computer science, materials science, biotechnology, and toxicology in the decade). But we obviously had to develop a policy how to cope with this zero growth situation.

We, that was essentially the President (at the time myself), the Rector (at the time **Heinrich Zollinger**), and the Head of Administration (at the time **Eduard Freitag**), together with some colleagues in the Rector's office and a very small number of administrative staff. Our goal was to maintain strength at ETH where strength existed und was necessary, and to create new strength where new strength was needed. In order to measure strength in research, we instituted a funding mechanism within ETH that followed the pattern of the National Science Foundation (we actually used its forms), with a peer review committee composed of eminent scholars who passed a judgment on the quality of the proposals they received from their colleagues. For running this scheme we needed money. We got it essentially by reducing the non-personnel related operating budgets of all Institutes - some 80 at the time - by l5% over a two-year period.

The first assumption was that most institutes in the course of the preceding growth phase had become comfortably funded and could stand this reduction; this is the lawn-mower approach to budget reduction and is as such not a policy, but merely a form of management. The second assumption was that the academically stronger institutes would very quickly produce proposals of high quality so as to siphon off more money from the central pool than they had contributed to it - to the disadvantage of the academically less convincing institutes; this was science policy.

On which Fields of Research Should we Put our Emphasis?

After a few years, the rejection rate became so high that new criteria had to be introduced, in addition to the criterion of scientific merit. The insight grew that ETH lacked important key disciplines. A motion of Parliament required the establishment of an Institute of Toxicology. Fact-finding in industry convinced us that computer-science needed a major booster. Some imaginative colleagues from microbiology carried a torch for biotechnology, others one for materials science; either discipline was lacking, at that time, at ETH. These gaps were evident, but had few advocates. We made ourselves advocates of the gaps, which helped. The Planning Committee, with its strong-willed president, **Fritz Widmer**, helped.

But implementing these plans' meant, because of the personnel freeze, that we had to shut down a few of the existing units in order to free personnel posts, space, and operating money for the new ventures. Arriving at decisions to reorient some of ETH Zurich had required endless debates internally, locally (with the neighbouring University of Zurich), nationally, and internationally. Measuring the frequency of publications and citations, over time, worldwide, helped us in judging

the relative importance of fields. In the process we arrived at the conclusion that it was better for ETH to move into those new fields than to continue on some other tracks. My own field, zoology, was one of the victims of these deliberations; other victims were found in architecture and civil engineering, chemistry, photography, earth sciences, to mention but a few. All in all, 28 professorial chairs that became vacant were reoriented into new fields; over 400 personnel posts were shifted into new fields. This policy hurt those who had to serve as donors, and was taken for granted by the receivers. Looking back at the results, I feel it was worth the effort.

Are we Making a Maximum Effort?

After 13 years and 5 months I was kicked upstairs, from the local to the national level. At the national level, my efforts in Science Policy followed three routes of attack. The first: improving task-sharing among all Universities of our country. I was and am still convinced that the quality of a small university is improved when it tries to be selective rather than comprehensive in its offering (there are only small universities in Switzerland). Some success has become apparent. It had been recognized that five departments of Pharmaceutics constituted an overdose for our small country; the government of the Canton of Berne acknowledged this and closed the Department of Pharmaceutics at the University of Berne. Bernese students of Pharmaceutics find the necessary admission capacities in Zurich or Lausanne.

It had also been recognized that two complete schools of Architecture, 65 km apart, one each at the University of Geneva and the Swiss Federal Institute of Technology in Lausanne, were too expensive, and not necessary; the two Institutions agreed to a task-sharing. It was recognized, thirdly, that ten laboratories of Swiss Universities engaged in high energy physics at CERN were too expensive, and not necessary; consequently, several universities decided to reduce their efforts in this field of science. Many other insights exist on cases where a better task-sharing would reduce cost and perhaps even enhance quality by regrouping efforts of under critical size into more competitive units, not only in natural sciences, engineering, and medicine, but perhaps even more so in some social sciences and the humanities; I had already mentioned Contemporary History for a long time, feeling that this discipline was underrepresented if compared with Medieval History. Or Ancient Greek and Latin if compared with Sinology and Japanology. One could go as far as to say that some universities in these fields looked far more intensively into the distant past than into the present or the future. The rate of success of these efforts has been far inferior to what we were used in the ETH domain, largely because of the absence of a clear line of command. On the institutional side, some cantonal Universities began to reflect, perhaps because of this experience, on a systems approach, designed according to the ETH model. BENEFRI is one example, with true success yet to be shown. The intention of the rectors of the Universities of Lausanne and Geneva, to merge their institutions, points in the same direction.

Also, cantonal legislation here and there begins to see the advantages of an increased autonomy of Universities, their uncoupling from ministerial

bureaucracies, as we have reached it with the new law on the ETH domain of 1990; I feel that this new law is an important piece of evidence of Science Policy.

Were we Achieving the Necessary Results?

The second route of attack at the national level was to speed up valorization of scientific insights into industrial exploitation. This, I believe, constitutes the limiting factor of the success of Swiss science. You know some of the evidence. **Werner Arber**'s and **Heini Rohrer'**s discoveries are of crucial importance for much of Industry's future. And yet bio- or nano-technology firms are sprouting abroad, hardly at home. Why? One of the reasons, I was told, is an insufficient similarity of intentions on the side of Industry and on the side of University. I felt this could be tested experimentally by launching research programs that were designed jointly by Industry and University, carried out jointly, and financed jointly.

So we launched a half a dozen so-called Priority Programs, six years ago, in opto-electronics, power electronics, biotechnology, computer science, environment, and materials, more recently in micro- and nano-systems technology. Some results look promising, some are good. The best example thus far is the power electronics program; based on some of the results obtained in that field of research, companies were founded and jobs were created.

Choice of the Fields of Research as Open Issue

It is at this point that I would like to come back to my earlier statement, when I said that I had liked the opportunity to apply for money in a program on cancer research, but that nobody had forced me to do so. When we first launched the Priority Programs, there was much opposition by colleagues who were afraid that their academic freedom would be curtailed. This was not true, at least not at that time, because the CHF 357 million for the Priority Programs were incremental money, and because nobody was forced to participate. These colleagues also argued that this additional money ought to have been made available not for the support of research oriented towards certain questions, but as a supplement for the support of free research, by the NSF e.g., where the rejection rate had reached a level of 50% or more. While I agree in principle - free researchers being specialists of the “unexpected” - I had to disagree in view of the political fact (not a fact of science policy, but of straightforward politics) that many members of our Parliament as of Parliaments the world over, consider NSF projects as peer-review-driven sprinkling cans, and Parliaments consequently refuse to augment their budgets by quantum jumps. It was very clear that our Parliament would agree to quantum jumps - if at all - only if it was to have a say on the content of the additional research to be supported, to be shown as a significant contribution to the creation or maintenance of jobs in the labour market. The debate in the Houses of Parliament on the new Priority Program on Micro- and Nano-Systems technologies was particularly clear on this point. Equally clear was the active interest of Industry - which matched the CHF 56 million voted by Parliament with CHF 65 million of industrial money. Now one could argue that if the scientific community, via NSF, would itself organize such

programs, the sprinkling-can argument would be void. Yes. But thus far the NSF has not done so.

And then it was argued that Priority Programs were evil because they were installed in a top-down manner. Here, a clarification is needed: All we did, at the outset, was to signal the possibility that Priority Programs would be launched, if convincing proposals were submitted. This was indeed the top-down announcement of an experimental set-up. The elaboration of the contents of the Programs, however, evidently occurred bottom-up by the scientists concerned. Evaluation of the proposals, again was handled by the authorities, who consulted scientists internationally - is this top-down or bottom-up? The final decision was in the hands of Parliament, by a vote on credit-lines. But this is no different from the decisions on allocations of non-earmarked money to NSF. Some representatives of the Social Sciences and Humanities protested because there was no Program for them. True, but they had not proposed. But then, they continued to argue that this was the case because their science did not lend itself to work in well orchestrated programs. I disagreed, and still disagree. The transition from planned to free market economy; migration; Europe; drug abuse; the shadows of World War II constitute excellent fields of research for scholars of Social Sciences and Humanities, with results of burning interest to today's Society, including Industry and the Tertiary Sector. I hope that with the recent launching of a Priority Program in these fields, an esprit de corps will grow there, too.

Speeding up Research Work by Participation

The third route of attack was to facilitate the participation of Swiss scientists in international scientific efforts. A small country simply cannot afford to stay isolated when Europe, North America, and the Far East are tooling up to launch new, massive research efforts into novel fields that would assume key importance in a near future. In Europe, this meant finding a way that Switzerland could obtain access to the Framework Programs of Science and Technology. The agreement on the European Economic Area would automatically have provided for this access, because it was part of the agreement. The negative vote on December 6, 1992, meant that other ways had to be found. Our government very wisely included Research in the basket of bilateral negotiations that was proposed to the European Union, and the latter accepted this idea. In order to start such negotiations, we needed a pledge of money. Parliament voted the credits that became necessary - CHF 477 Mio - in a memorable vote a mere two weeks after the referendum of December 6, 1992, and an additional CHF 554 Mio two years later. Had the bilateral negotiations been successful in time, this would have meant that these sums would be sent to Brussels, and would flow back' into Switzerland as a function of the quality of proposals of Swiss origin or with Swiss partners. Parliament had said yes, therefore, to exposing some of Swiss Science to international competition (the bilateral agreement on research, although virtually completed in June 1995, has not yet been signed to this day, because the European Union insists that the entire package of bilateral negotiations should be completed in parallel; other items in the negotiations such as the free movement of labour, or transports, progressing more slowly, the research agreement must wait. Nevertheless, the Swiss

may participate in the FRP on a project-by-project basis, though without the right of launching or leading projects).
Also, there is no flow of Swiss money to Brussels; those Swiss whose proposals pass peer review at the EU-level obtain the necessary funds domestically (this arrangement is based on a general agreement that had been wisely negotiated in1987 already). But I repeat: our Parliament agreed to a policy to facilitate access of our scientists to international programs in defined fields of interest, and to let them compete for the necessary funds internationally. It turned out that many members of our scientific community found the access into the European programs with ease because of he experience gained previously in Priority Programs. "Without LESIT (our own power electronics program) we would not have made it into ESPRIT (a corresponding European program)", as one prominent Swiss scientist put it.

Knowing that the US and Japan, but also smaller countries like e.g, South Korea and the G-7 nations, too, still believe in the approach to complex, promising fields of research by launching corresponding programs, we felt it was important to find ways of access into such programs as well. To that end a necessary prerequisite are agreements to exchange information about such programs at an early stage, and such agreements we have been preparing in recent years, engaging, as early as possible, active scientists in the corresponding' visits so as to make sure that once paths for cooperation were open, they would be used.

In his effort particularly, it has been a gratifying experience for me to note that an increasing proportion of our scientists, both from University and Industry, understand that Science Policy works not against, but for Science. As a scientist working at the lab bench, way back, I had hardly noticed the problem, or the issues associated with it. I have learnt it over decades, and tried never to overlook that Science Policy cannot be done without **involving** the scientists. **Philip Handler** brought it to an interesting point at this rostrum in 1980. He argued that both scientists and non-scientists often fail to disentangle rigorous **scientific evaluation** from **ethical, moral**, **social**, **economical** evaluation. **Scientists**, he said, **should step out of their laboratory coats when they take issue with the political dimensions of their work**.

Quelle:Schrift zum internationalen wissenschaftlichen Symposium der Professur für Wissenschaftsphilosophie und Wissenschaftsforschung, welches zum Anlass des Rücktritts des Staatssekretärs für Wissenschaft und Forschung, Prof. Dr. Heinrich Ursprung, unter dem Patronat der Schulleitung der ETH Zürich am 5. und 6. Februar 1997 im Auditorium Maximum, ETH Zentrum, durchgeführt wurde. Die Ansichten des Autors müssen sich nicht mit denjenigen des Projektteams von ETHistory decken.