

Succeeding Outside the Mainstream

*The Nobel Prize in chemistry awarded to Gerhard Ertl brings the Max Planck Society's Nobel laureate count up to 17 since its founding in 1948. Not to mention the 15 Kaiser Wilhelm Society scientists who received Nobel Prizes between 1914 and 1948. In their essay "The Max Planck Recipe for Success," **JÜRGEN RENN** and **HORST KANT** reflect on present-day research policy against the backdrop of past directions in science.*

Can we learn from the past? When it comes to the future strategies to be adopted by the Max Planck Society, a look back at past successes may well be helpful. How did the Kaiser Wilhelm/Max Planck Society (KWS/MPS) determine the focus of its research, where were the breakthroughs made and which structural requirements yielded such successes as the Nobel Prize in chemistry awarded to Max Planck scientist Gerhard Ertl? As simple as the questions may appear, they are hard to answer without falling into the traps implicit in conventional methods of evaluation. We are faced here with a historical research problem that has thus far gone largely unaddressed. Therefore, we must limit ourselves to a few examples that have been considered in the course of other research projects or enlarged upon in discussion with colleagues. We can do no more than touch on such historical examples here.

Progress is by no means an exclusively cumulative process; it involves rearranging our systems of knowledge. The development of modern quantum and relativity physics is a prime example, with its non-classical concepts of space, time and matter.

Such innovations are often the product, not of spontaneous paradigm shifts, but of the long-term and conflict-ridden amalgamation of heterogeneous bodies of knowledge. Quantum theory, for example, originated with the integration of aspects of knowledge drawn from physics and chemistry: Were it not for electrical engineering, the problem of black body radiation, the start-

ing point for quantum theory, would probably not have acquired such a central role in research in the 1890s.

To identify and resolve the productive internal conflicts between knowledge systems, it is often necessary to adopt a different perspective than that which first triggered the conflict. Such perspectives tend to originate on the periphery of the mainstream rather than at the core. Thus, when the KWI for Chemistry was founded in 1912, radioactivity was initially studied only by a small department headed by Otto Hahn, while issues of organic and inorganic chemistry were the province of large institute departments. However, by the 1920s, research into radioactivity had become the main focus of the institute, and the heads of the department, Otto Hahn and Lise Meitner, ranked among the world leaders in their field.

Constant growth within fixed boundaries

The long-term, heterogeneous and discontinuous character of scientific progress makes particular demands on the manner in which research is organized, which is not necessarily coincident with the undisputed need to pursue the mainstream. One of the reasons for the success of the KWS/MPS has been its ability to address this challenge more effectively than other research organizations. Examples range from nuclear fission of uranium to the discovery of organometallic catalysts for the polymerization of olefins at the MPI for Coal

FIG.: DIEMAR GROSSE



Research, which in 1953 led to the development of the low-pressure polyethylene process – a breakthrough that had then-unforeseeable economic consequences. Other notable successes include the establishment of molecular electrophysiology (at the MPI of Biochemistry), the structural definition of ribosomes (at the MPI for Molecular Genetics) and the origination of research into both “Bildungsforschung” and lifespan psychology (at the MPI for Human Development). It is evidently in the nature of the MPS to act as a catalyst for the re-arrangement of knowledge systems.

The traditional image of scientific progress is one of continuous growth within fixed boundaries, with certain exceptions. From this, two types of success can be derived. The first is successful participation in the mainstream, made evident by the impact factor. The second is an individual and pre-eminent achievement often associated with a reorientation of knowledge. Such achievements are frequently recognized only when the “new” is no longer new, but has instead become part of the mainstream. Consider the time lag in awarding Ernst Ruska a Nobel Prize for developing the electron microscope.

On the other hand, given the key role played by the restructuring of knowledge in the advancement of science, these criteria for success are inadequate insofar as they both relate to the mainstream. Among Einstein’s early works, it was his little known dissertation that had the biggest impact in terms of citations, simply because it came closest to the mainstream.

the KWI for Chemistry. Willstätter was persuaded by Fischer to move to Berlin to conduct research into the promising field of plant pigments. Later, as President of the MPS, Otto Hahn saw to it that Wolfgang Gentner succeeded Walther Bothe by promising him an MPI for Nuclear Physics that was independent of the MPI for Medical Research.

Such judgments and their consequences were, of course, to a large extent fortuitous, but was that all they were?

The particular manner in which research is promoted by the MPIs mostly mirrors the long-term, heterogeneous and discontinuous character of scientific progress. The principles underlying the research strategy of the MPS include subsidiarity, an interdisciplinary approach and the Harnack principle. Subsidiarity here implies adopting a broader outlook than that of the mainstream, for instance in consideration of effective long-term research strategies of a kind that universities cannot always afford: The leading role played by the MPS in, for example, the long-term development of gravitational wave research is due not least to the Society’s backing for this work in the 1980s, when no one else would put up the money. The leading position of the MPS in the study of international law is also attributable to decisions based on the principle of subsidiarity. Of course, in a changing academic landscape, this principle must constantly be redefined.

Another characteristic feature is the Society’s practice of researching promising peripheral areas where fruitful conflicts can be found. An impressive example here is the interdisciplinary work of the MPI for Psycholinguistics, to which we owe an understanding of the system of language production and other breakthroughs that have established this area of study at an international level.

And lastly, the promotion of research at the MPIs also echoes the Harnack principle, which may be taken here to mean the opportunity to allow new scientific perspectives to be absorbed into the organization of research in the long term, even when this involves a degree of risk. Thus, for example, when Max Planck himself, as President of the KWS, appointed Walther Bothe in 1934, he was willing to accept a reorientation of the Institute of Physics established only a few years previously at the KWI for Medical Research. This would shift the focus away from medical physics and toward the fledgling science of nuclear physics, simply to establish a home for this new and forward-looking field of study at the KWS.

However, research outside of the mainstream is not advantageous in every scientific situation. Planck’s idea in 1932 to establish a KWI for Dowsing and Earth Radiation was rightly not pursued.

Researching peripheral fields presupposes an interdisciplinary coherence that, in reality, can fail to tran-

scend departmental borders. Experience shows that cooperation is sometimes more effective between departments at different institutes than between the units at a single institute. Time and again, the potential of the network constituted by the MPS as a whole to support successful interdisciplinary research has proven nearly impossible to

overestimate – and the line of research being pursued need not always lead to a progressive breakthrough. There are also such things as opportunistic or even regressive successes. Opportunistic successes are often the result of an excessive pressure to succeed, which in turn can lead to the conventionalization of initially innovative work. As an extreme example of a regressive breakthrough, consider the racist research conducted by the KWS under the Third Reich.

From an historical perspective, other factors, too, have played a part in the successes of the MPS, in particular the choice of topics and institutional efficiency. In terms of the selection of topics, various strategies have proven successful, one among them being to reflect on the status of the subject itself. The crisis facing classical physics at the start of the 20th century was evident to many physicists, even if they had no idea how to resolve it. Such was their awareness that leading physicists and scientific administrators resolved to set about founding a KWI for Physics that would be dedicated to this fundamental crisis, and to recruit Einstein to head this institute.

Reflecting upon the status of the subject can lead to the conclusion that the task of an institute should be to act as a catalyst to nurture already existing innovative perspectives. After all, scientific breakthroughs also need to be sustained. Take, for example, Konrad Lorenz’s ideas for comparative behavioral research, which set a new paradigm that was adopted with the foundation of the Max Planck Institute for Behavioral Research in 1954.

Political opportunities, too, can be utilized to further the cause of interesting research prospects, as in the case of the foundation of the MPI for Extraterrestrial Physics under Reimar Lüst in 1965. The “Sputnik Shock” caused the USA to cast around for European partners to assist in space research. The political constellation at the time allowed enough scope to found a new institute to exploit existing avenues of research at the Max Planck Institutes for Physics and Astrophysics, Aeronomy and Nuclear Physics, and complement theory with experimental practice.

It is likewise helpful to consider in which areas the MPS can best deploy its particular strengths. For the humanities, the position occupied by the MPS in a society dominated by the sciences offers a unique research con-



text. Institutes working in these fields are able not only to build bridges between what may be described as two cultures, as is happening at the MPI for Human Cognitive and Brain Sciences and the MPI for Evolutionary Anthropology, but also to overcome some of the traditional fragmentation within the humanities themselves. The two art his-

tory institutes operated by the MPS in Italy also demonstrate, in differing ways, the potential for innovation still to be derived from established research traditions nested in a privileged cultural environment.

The issue of development processes today plays a vital role at many Max Planck Institutes, from cosmology, developmental biology and brain research to evolutionary anthropology, science history and human development. Research of this nature can only be conducted with a long-term perspective, and requires precisely the kind of institutional support that the MPS offers. Just where is such staying power called for? Certainly in research into nuclear fusion along the lines pursued by the MPI for Plasma Physics, the largest center of fusion research in Europe. Back in 1950s, it was anticipated that nuclear fusion would be commercially viable within about 20 years. Today, it is expected that it will be at least 2050 before such power plants are available.

Surprising prospects

It is by no means exceptional in the history of successful research choices to take up one of the challenges facing society. It was an often-overlooked tradition at the KWS to establish institutes dedicated to applied basic research, frequently with generous industrial funding. The Institutes for Coal Research, Fiber Chemistry and Leather Research are just three examples. Current examples of applied or potentially applicable basic research being conducted within the MPS include work on international and foreign law, human development, aging, biotechnology, earth systems and, of course, energy.

One strategy adopted in selecting research topics has proven to be particularly promising: the development of new perspectives derived from what was originally peripheral research. A recent example in the field of biology was the discovery of the agrobacterium *tumefaciens* by Jozef Schell (MPI for Plant Breeding Research) in the course of his work on plant tumors. This unexpectedly paved the way for DNA transfers in plants, and thus provided a basis for green genetic engineering.

Supporting the restructuring of knowledge systems at an institutional level makes substantial demands on the fertility of the MPS and its institutes, and their own ability to mutate. Institutional efficiency also implies a capac-

In the right place at the right time

Neither criterion has value as a forecasting tool, for, as indicators of a reorientation of knowledge, the impact factor often comes too soon – and the Nobel Prize too late. Otto Warburg was awarded the Nobel Prize in 1931, shortly after he was appointed Director of a Kaiser Wilhelm Institute of his own, whereas his pioneering research work was done beforehand at the KWI for Biology. Werner Heisenberg, on the other hand, had to wait 10 years after receiving his Nobel Prize before he became a Director at the KWI for Physics.

Nevertheless, in the past, the MPS consistently managed to achieve structural successes. In each case, the scene was set by judgments regarding the right person in the right place at the right time. Around 1912, Max Planck, Fritz Haber and Walther Nernst discovered Einstein as a candidate for the KWI for Physics, then due to be founded. Emil Fischer discovered Richard Willstätter and Otto Hahn for the KWI for Chemistry: Fischer had already given Hahn a job at his university institute back in 1907 to research radioactivity, a field that enjoyed little acceptance among organic chemists, and in 1912 he ensured that Hahn was given a department of his own at

ity to accommodate new and unexpected changes in the direction of research and surprise shifts in emphasis, as well as offering opportunities for development at all levels, from individuals to research groups and even entire institutes.

Numerous cases in the history of the MPS underscore the principle of fertility – in other words, the ability of institutes to come up with new topics and the capacity of the MPS to afford these an appropriate institutional basis. One example is the development of chronobiology, from the pioneering work done by Jürgen Aschoff in the mid-1950s at the MPI for Medical Research to the subsequent institutionalization of this field at the MPI for Behavioral Physiology. The MPI for Biophysical Chemistry was spun off in a similar manner in 1971 from the MPI for Physical Chemistry, which itself originated under Karl-Friedrich Bonhoeffer in Göttingen in 1948 as an offshoot of the KWI for Physical Chemistry in Berlin.



Mainz, Meteorology in Hamburg and Biogeochemistry in Jena are voluntarily committed, and which also embraces cooperating departments at four or more other institutes, is a prime example of how to efficiently address interdisciplinary issues.

This example also points to another dimension of institutional efficiency: choosing the right scale on which to operate. On the one hand, it generally makes sense to start with smaller, flexible units. Yet on the other hand, out-of-the-ordinary research projects often need to achieve critical mass in order to prevail over mainstream activities.

The MPS, with its specific mission, plays an important role in the academic division of labor in Germany. Its success is substantially dependent on its political freedom to choose its own organizational forms and research topics in the wider field of basic research. However, the MPS also has a niche role at a global level. It will make better use of its structural advantages if, in the future, it becomes an increasingly global player – always provided that it simultaneously manages, internally, to preserve a collective and concerted awareness of its particular task. This, in turn, places a natural limit on its conceivable expansion in the form of a communication horizon, beyond which the Society's identity is at risk.

One of the future challenges facing the MPS will be the need to heighten its profile as a scientific organization by offering a unique freedom of research – in other words, the opportunity to step outside of the mainstream, combined with guaranteed continuity of research and openness toward new research orientation. Sharpening the profile of the MPS in this way will necessitate greater flexibility in both the internal structures of the institutes and their external relations.

No research plan, however cleverly devised, can succeed without the MPS placing greater emphasis on an internal as well as external awareness of its specific role in the division of research effort. Given that the concept of excellence is now politically charged and becoming increasingly less trenchant as a unique selling proposition of the MPS, public perception of the Society's particular task will be decisive in determining how successful it is in attracting both resources and young scientists. Thus, internally, the MPS ought to develop a reflective culture oriented toward its role as a catalyst for structural changes in systems of knowledge. Externally, it must be proactive in seeking recognition of this role.

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Valuable cooperation between institutes

Institutional efficiency can also be reflected in the ability of entire institutes to mutate. Outstanding examples here include the KWI/MPI for Coal Research and the Fritz Haber Institute of the MPS, which have consistently reinvented themselves against the backdrop of significant scientific achievements. The history of the Institute for Coal Research began with the problems inherent in processing coal to increase its value, followed by the concept of converting coal directly into electrical energy. However, successes such as the Fischer-Tropsch process of extracting liquid hydrocarbons (1925) and Karl Ziegler's low-pressure polyethylene process (1953) already mentioned above steered the institute in other directions. The Fritz Haber Institute, too, has successfully shifted its focus on several occasions, with the main emphasis of its work continually dominated by larger issues, such as the search for a comprehensive understanding of catalysis. The 2007 Nobel Prize in chemistry awarded to Gerhard Ertl of the Fritz Haber Institute for his work on catalytic processes shows that staying power has its rewards.

The development of genuine cooperation in or between institutes can be of key importance for the institutional efficiency with which a particular line of research is conducted. Success is dependent on whether there is a clearly defined focus for convergence, whether cooperation is supported by the use of shared research resources or service departments, and whether inter-departmental project groups are endowed with the necessary degree of adaptability. The earth systems research network, to which the MPIs for Chemistry in