CONFERENCE REPORT

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Conference Report

Science Impact

Rethinking the Impact of Basic Research on Society and the Economy

International Conference
10–11 May 2007, Vienna

European Science Foundation
SETTING SCIENCE AGENDAS FOR EUROPE

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Science Impact – Conference Report

Rethinking the Impact of Basic Research on Society and the Economy

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Thursday, 10th May 2007

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19:00	Open-ended

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Conference Summary

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Christoph Kratky

President of the Austrian Science Fund, AT

"The discussion about the relevance of basic scientific research is pandemic, and it is anything but trivial."

On behalf of the Austrian Science Fund, it is my great pleasure, to present to you the report of the international conference "Science Impact – Rethinking the Impact of Basic Research on Society and the Economy", which was hosted by the FWF in cooperation with the European Science Foundation (ESF).

All across Europe, it is appreciated that research is one of the foundations of technological innovation and hence is a key to meeting the challenges of global competition. However, this beneficial property is often only attributed to applied research, where the economic impact is obvious. Basic research, on the other hand, has the air of being a luxury for extravagant scientists who indulge in their hobbies at taxpayer's expense. This prejudice is particularly prevalent towards those disciplines where economic exploitation of scientific results is unlikely, such as in the humanities. As we all know, utility is easily confused with benefit.

The FWF is Austria's central funding agency for basic research. Our key value is scientific quality, i.e. we consider a research project worth taxpayer's money if scientific excellence is attributed to the project through an international reviewing process. It is evident that this approach is valid and relevant for the scientific community, but it is much less obvious that society shares this attitude. Indeed, the discussion about the relevance of basic scientific research is pandemic, and it is anything but trivial. Its results are of immediate relevance to politics, economics, funding agencies and, last but not least, to the scientific community.

This conference represents a milestone in the discussion on the impact of basic research.

Christoph Kratky

Ian Halliday

President of the European Science Foundation, FR
and Chief Executive of the Scottish Universities
Physics Alliance, Edinburgh UK

Across the globe science and innovation are seen as the way to advance wealth, health and prosperity. The universities are seen as key players in generating new knowledge. The challenge for governments and research and innovation funding agencies is "How do we optimise our investment between long term blue sky research with unpredictable wealth creation outcomes, with funding high risk ideas with commercial or technological promise and with forcing the development of technology needed by society; all the time avoiding government funding of R&D that firms should fund privately."

This is a challenging managerial problem for companies and governments. There are a wide variety of assumptions and models on view across Europe and elsewhere. These models are often in implicit or explicit conflict with assumptions made elsewhere in government: state aid constraints versus government driven innovation, entrepreneurial universities versus state controlled universities, government defining societal needs versus the market knows best, ...

In this meeting these assumptions and their consequences were explored. How can Europe in particular regain its appetite for innovation and wealth creation shown in the 18th and 19th centuries? Where are the real barriers?

Ian Halliday

"How can Europe in particular regain its appetite for innovation and wealth creation shown in the 18th and 19th centuries?"

SCIENCE IMPACT CONFERENCE

„Science Impact – Rethinking the Impact of Basic Research on Society and the Economy" – this was the title of the conference the Austrian Science Fund FWF, in cooperation with the European Science Foundation ESF invited a distinct audience from 10th to 11th of May 2007 to Vienna. The goal of this international event was to discuss functionalities, results and ways to improve the impact of basic research.

In total 23 experts from nine countries could be welcomed as lecturers; a list of the best of practicians and theorists in the relevant fields.

In chronological order of their appearance at the conference the speakers were:
Karl Aiginger (Austrian Institute of Economic Research), **Haim Harari** (Weizmann Institute of Science), **Sheila Jasanoff** (Harvard University), **Herbert Gottweis** (University of Vienna), **Andrew Webster** (University of York), **Joel Mokyr** (Northwestern University), **Luc Soete** (UNU-MERIT), **Erik Arnold** (Technopolis), **Ian Halliday** (European Science Foundation), **Susan Cozzens** (Georgia Institute of Technology), **Chris Mombers** (Technology Foundation STW), **Helga Nowotny** (European Research Council), **Ben Martin** (University of Sussex), **Benoît Godin** (INRS Montreal), **Wolfgang Polt** (Joanneum Research), **Stefan Kuhlmann** (University of Twente) und **Gerhard Widmer** (University of Linz)

The contributions of these renowned scientists were complemented by early stage researchers, which were identified by an international call, namely: **Shaul Katzir** (Bar Ilan University), **Laurens Hessels** (Maastricht University), **Thomas Bechtold** (Research Institute for Textile Chemistry and Textile Physics, University of Innsbruck), **Sam Wong** (University of Leeds), **Claire Donovan** (Australian National University) und **Soile Kuitunen** (Net Effect Ltd.)

The organizers were pleased to welcome approximately 300 guests from more than 35 countries from four continents.

The conference comprised three sessions: „The Impact of Basic Research: Theory, History, Expectations", „Funding Models and their Influence on the Impact of Basic Research" and "Scope Limits and Role of Impact Evaluation". After the respective speeches given by the scientists and inputs from the early stage researchers, the floor was open for discussion, which was made use of extensively.

This report summarises the contributions of the speakers, the discussions with participants, and the conclusions drawn by the rapporteurs.

In addition to this report, the speeches can be obtained from the conference website. As a special service the FWF will provide a complete video-recording of the conference, made available as video streams speech by speech. In addition, all "follow-up actions" related to the conference, developments in this field, and political statements will be published on our website as well.

The conference website is www.science-impact.ac.at

Science Impact

OPENING STATEMENTS

Johannes Hahn, Federal Minister for Science and Research

Josef Broukal, Member of Parliament and Science Speaker of the Social Democrates, by proxy Werner Faymann, Federal Minister for Transport, Innovation and Technology

Günther Bonn, Vice President, Austrian Council for Research and Technology Development

Theory, practice and future of basic research and its promotion were the subject of the international conference on “Science Impact”.

Welcome & Opening
“The meeting is timely”

Gerhard Kratky, Managing Director of the FWF, who had undertaken to anchor the first morning, led to the three opening statements.
In order to ensure that the university and research sector would continue to be a “future area for continuing the success story of the Second Republic”, Austrian Federal Minister for Science and Research **Johannes Hahn,** in his opening statement, pointed out three necessary steps: first there was the need to create and preserve “free space”. Secondly, efforts needed to be made to give universities “radiant powers of attraction”. Thirdly, it would be highly welcome to “network innovative scientific research to those sectors of business [...] that generate new products or services and that are highly innovative in their focus”.

Gerhard Kratky, Managing Director of the FWF also took the role as anchorman of the morning of the first day.

Johannes Hahn, Federal Minister for Science and Research: “For the continuation of the success story we need to create and preserve free space.”

Josef Broukal, Member of Parliament and Speaker of the Social Democrates: “Spending this money is profitable, it is useful to give more money to this sector.”

Josef Broukal, Member of Parliament, similarly stated in his opening statement that today “the emphasis needs to be on the interfaces between basic and applied research”, explicitly referring to the COMET, Bridge and Translational Research programmes.
Broukal also pointed out the need “that we can meet the tax payers, our customers in our capacity of members of Parliament, and tell them: spending this money is profitable, it is useful to give more money to this sector!”

Same as the speakers before him, RFTE Vice President **Günther Bonn,** following the Council’s Strategy 2010, pointed out with “strong emphasis” the recommendations to “usefully link” basic research with applied research. In his estimate, basic research has the following role: “Basic research [...] is the point of departure for innovation in a country and an indicator of how politics perceives innovation, technology and progress in general.” A central point, according to Bonn, was that basic research was of economic utility, concluding: “I know that the FWF guarantees quality, and I know that we need to support this precious body which we have in Austria, and I can guarantee that the RFTE will continue to do so over the coming years.”

Günther Bonn, RFTE Vice President: “I know that the FWF guarantees quality, and I know that we need to support this precious body which we have in Austria.”

Science Impact

KEYNOTES

Karl Aiginger, Austrian Institute of Economic Research, AT
Haim Harari, Weizmann Institute of Science, IL
Sheila Jasanoff, Harvard University, USA

Basic Research:
Business, Science, Theory

The welcome and introductory statements were followed by the three keynote speeches of the first morning.
Karl Aiginger, Head of the Austrian Institute of Economic Research, looked into the impact of science from the economic vantage point. **Haim Harari** from the Israeli Weizmann Institute forwarded the scientific view, while the third keynote speech of the morning was used by **Sheila Jasanoff** (Harvard University, US) to furnish a state-of-the-art overview from the aspect of scientific theory.

Karl Aiginger, Haim Harari and Sheila Jasanoff during their keynote speeches.

Karl Aiginger, Director of the Austrian Insitute of Economic Research, about relations between business and basic research, the importance of social goals and future strategies for Europe.

Karl Aiginger
Basic research and the "greater importance of societal goals"

Aiginger described the European model, its successes and failures, sketching the situation of Austria within this environment, clarifying the role of science within it and, after asking "what do we know about the impact of basic research", concluding with an attempt to delineate recommendations for the future. He described the situation as follows: "Overall, competitiveness is insufficiently based on the future factors of research, training and new technologies." The structures "are better for the catching-up process and still insufficient for a top position". In order to achieve such a top position it was necessary to improve the "status in the research field". According to Aiginger, it was necessary for the expenditure on research and development to reach a level of 3.5 percent of GDP. He criticised that "foundations are used to save on taxes but not for social purposes" and that "businesses do secondary research". Basic research thus, according to Aiginger, had to "provide the base". As was clearly highlighted in the opening statements, Aiginger concurred: "SMEs need to have better relations to universities", in order to ensure that the saying "research is effective" will continue to be true in the future. Among his many impressive examples in his comprehensive and detailed description of relations between business and basic research he pointed out that a billion euros invested into research translates into 50,000 jobs. Accordingly Aiginger pointed out that "research is the most effective strategy to spend public funds". But research by itself was not effective; it was necessary to start at school in order to achieve long-term improvements, as the doyen among Austria's economists is convinced. "Research generates affluence" and leads to "integration". This affects "organisational, technical" as well as "social innovation". Aiginger diagnosed for Europe not just a "deficit in basic research [...] as well as a deficit in the speed of application". It was thus necessary to develop "concepts, competition and controls", where "the European model of basic research" can and must "look different from the American model". It can and should put greater emphasis on societal goals and attribute more importance to ecology, integration and social cohesion".

"The need for catching-up and change is considerable."

Summarising, Aiginger opined that the "need for catching-up and change" was considerable. In order to achieve the top position, it was necessary to rearrange activities. To this end it needed more than spending money, in order to get sustained success in Austria. Aiginger is convinced that the growth gap in Europe is linked "directly to the deficit in research and training". According to him, Europe is currently "making insufficient investments in the future", and he calls for a "top-level strategy" that supplements and combines the "small and large area". Returning to the societal goals, he concluded that the design of future "research focal points" would have to be "oriented along the priorities of the societal model".

Haim Harari, former head of the Weizmann Institute in Israel about the importance of basic research and its freedom, ten paths of converting "basic research" into "economic value" and the conclusion that basic research is in many ways like babies.

> "Basic research is like a baby. You don't know at which age the result is to come. You don't ask, why you feed the baby."

Haim Harari
Ten paths to impact

Under the heading of "From Basic Science to Economic Value: Necessary Path for a Modern Economy" Haim Harari made a strong witness for the importance of basic research. Referring to the discovery of the electron the benefit of which can meanwhile be described as: "More than 50 percent of the world economy is now based on our understanding and knowledge of the properties of the electron", and the unpredictability of this development ("none could have predicted it"), Harari launched on his "tour de force" of explaining the practice of scientists and their societal relevance and impact. He leaves no doubt that the creation of knowledge is the central growth sector: "Knowledge is the leading asset in the world." For an example he produced an amazing comparison between Singapore and Saudi Arabia: "The per capita GDP of Singapore is more the double the per capita GDP of Saudi Arabia." The astonishing strength of Singapore was in its strong investment in knowledge. For Harari, this example made it clear that the best investment was into the "creation of knowledge", chiefly in education and basic research. It was necessary to point out this fact again and again: "Drill it into the heads of every politician." Still, scientific freedom needed to be untouchable. Referring to his work as a scientist, he noted: "I have never done any piece of research which by itself leads to any commercial, practical consequences – I plead total innocence." In view of the great success, also in economic terms, of the Weizmann Institute which he had headed for a long term, his conviction was not only valiant but also well-founded, because the Weizmann Institute is "the leading institute [...] in deriving income from the fruit of intellectual property". After clarifying his standpoint, Harari pointed out ten possible paths for converting "basic research" into "economic value". These began with the traditional manner, a sort of "step-by-step" or linear approach to integrate and translate findings of basic research into products. The next was the so-called "lucky break", where, as the name already indicates, unpredictability of progress plays a key part. This path meant the need to carry out a large number of projects, trials and acceptance that projects may take an above-average time to completion. Here the unpredictability of development was the key to success. For his third path, Harari pointed at the "Cinderella story". These are fields where nobody had ever thought of an economic use and where suddenly something "totally useless" turned out to be a paradigmic clarifying basis: "Emerge as the queen." As an example he gave cryptographics which is of great importance today. His fourth path of an economically positive development for findings from basic research was the sudden emergence of a technological use, the development of an application that could not have been foreseen (such as Faraday, laser, polymers). Furthermore, basic research and its working (framework conditions such as international working structure, lab situation etc.) frequently require tools that do not yet exist and need to be "invented". The most prominent example is the World Wide Web, developed at CERN by Tim Berners-Lee. As the sixth path, there are findings of basic research that are of such epochal importance that the world needs to rearrange itself after they have been established. Harari listed the development of semi-

Sheila Jasanoff, professor at Harvard University, about the necessity of heterogenous networks and the assessment of the "impact".

conductors, the relevance of the discovery of DNA and the deciphering of the genome. As his seventh path, Harari pointed out the importance of scientific gravitation centres. The history of science was also a history of "scientific dynasties and industrial parks". Such centres were of great importance to business and industry, not just in a metaphorical meaning – Silicon Valley was the best example. With his eighth path, Harari once again referred to the freedom of basic research. He felt that it had to be possible to study scientific areas which may never lead to a result, such as the string theory, because quite possibly "the end of science is not in science". Following this point, Harari felt that "ethical issues [...] are crucial". As his last path, Haim Harari beautifully compared the use of basic research to the benefit of children. He said: "Basic research is like a baby. You don't know at which age the result is to come. You don't ask why you feed the baby." And he completed his keynote lecture with the pointed note that children made up 20 percent of the population but constituted 100 percent of the future, and that the same was true of basic research: it was only "a small fraction of R&D", but provided "100 percent of the economic future".

Sheila Jasanoff
The desire for new competences

Taking up a "social point of view", Sheila Jasanoff devoted her lecture to the tension between "basic research and profitable outcome". For her it was important to make the "mundane work of the hands" visible, as she expressed it. According to Jasanoff, there was "a gap between the work of basic science and the visions that we have of how basic science is going to transform the world we live in". Referring to the preceding speech by Haim Harari, she added that "along each of the ten pathways" there were "points of friction". In her theoretical description of the work of basic researchers at the "frontier of dreams" she concentrated on the concrete working conditions. In her opinion it needed "heterogeneous networks" in order to successfully pursue the crucial development paths sketched by Haim Harari. But it was necessary to exercise caution because "languages are different". Thus, basic researchers talk about "experiments" while applied researchers talk about "progress". Linking science to society can fail, the connecting paths need not necessarily communicate. Thus it was necessary to have heterogeneous networks.

As an example, Sheila Jasanoff noted that the US National Science Foundation used two ways for evaluation. There was "merit" and there was "broader impact". She demanded significant frameworks for a definition of what made up an impact and who was to define

> "There is a gap between the network of basic science and the visions that we have of how basic science is going to transform the world we live in."

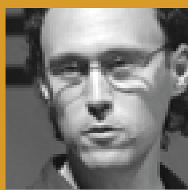
it. It was not just about business, the environment or health, but also about culture. Another problem was that risks should be handled as a rule. Sheila Jasanoff pointed out in her lecture enriched with plenty of graphics that "benefits" were an important factor and that yields therefore needed to be "assessed": "The benefits need assessment." She made it clear that since "Mode 2" an assessment of the "impact" should be a continual process. It needed intellectual competences, for which the following areas could be helpful: "social science competence, historical memory, distributive impact measures, new ethical discourses, cross cultural learning".

Science Impact

SESSION A
The Impact of Basic Research: Theory, History, Expectations








Herbert Gottweis, University of Vienna (Chair), AT
Andrew Webster, University of York, UK
Joel Mokyr, Northwestern University, USA
Luc Soete, UNU-MERIT, NL
Shaul Katzir, Bar Ilan University, IL
Laurens Hessels, Utrecht University, NL

"The Impact of Basic Research: Theory, History, Expectations" was the title of Session A. It was chaired by Herbert Gottweis, the speakers were Andrew Webster, Joel Mokyr, Luc Soete, Shaul Katzir, and Laurens Hessels.

Herbert Gottweis

Herbert Gottweis (University of Vienna, AT) introduced the session by briefly recalling the contributions from the previous key note lectures of the conference and drawing the conclusion that their common tenet is a warning that "single minded application fetishism" would be detrimental to science. He stressed that the discussion on the impact on basic research has several layers. The first session of the conference explores the issue from the perspectives of various disciplines which study the impact of basic research: social studies of science, history and economics.

Herbert Gottweis, University of Vienna and Vice President of the FWF chaired the first session.

Andrew Webster

Session A opened with a presentation by Andrew Webster (University of York, UK). The presentation was titled "Going beyond the lab: mobilising basic science through socio-technical networks". Andrew Webster believes that the traditional boundary lines between basic and applied research have become redundant. Instead he favours the concept of "basic innovation", which he defines as being a hybrid between basic research and its associated beneficial outcomes in society as a whole. The very concept of "Impact of Basic Science" implies a separate environment, in which basic science will have an impact: the economy, wider society etc. This "impact" is embedded in a wider framework, which goes beyond the lab where "basic research" takes place. For Webster, the impact of basic research depends therefore on the way, in which researchers in the laboratory anticipate how the results of basic research (in the form of knowledge, products, and processes) will be used.

In their endeavours to predict the pathways that discoveries will take from the laboratory to the wider "context of use", researchers have to cope with a great deal of uncertainties and contingencies both in their own research work and in the wider social framework in which their work will be applied. This means that in order to understand how science makes an impact, one needs to know how contingencies of basic research are linked to those of scientific application and context of use.

Andrew Webster, in his conribution, is "going beyond the lab".

For this reason, Andrew Webster suggests the concept of "Basic Innovation". This hybrid stresses once again that the boundary between basic, strategic and applied research is merely a framework to manage resources rather than to describe accurately the nature of the work the researchers are involved in. The term "Basic Innovation" encourages us to look at the way basic scientific developments and their uncertainties are increasingly embedded in innovation and are managed through

Andrew Webster, on the monitors of the control desk, explains to the audience the importance of socio-technical networks.

intersecting networks across labs, engineers, regulators etc.

An illustrative example of how contingencies in basic innovation are managed in socio-technical networks can be found in stem cell research.

The contingencies in this context fall mainly in two categories. On the one hand you have the uncertainties inherently related to biological variability among human embryonic stem cells. The differences here pertain to the origin of cell lines and to genetic differences arising over time. On the other hand there are uncertainties arising from the different laboratory practices on issues as varied as choice of methods, experimental setting etc.

In order to master those uncertainties, an "International Stem Cell Initiative" was developed. It brings together key players in the field of stem cell research to develop standards for the characterisation of human stems cells. Experts seek to secure agreed-upon criteria not through focusing on a specific tissue characteristic per se, but through a statistically robust set of measures to define what are good markers based on replicated testing of the same or closely similar sets of cell lines.

This example shows the most important characteristics of "Basic Innovation". Basic innovation involves an unprecedented level of reflexivity and collaboration, a collaboration no longer driven by "invisible colleges" but by visible networks. It is further characterised by greater power and higher levels of risk and provisionality, which can only be managed through distributing responsibility for it across a wide range of social, economic, and political actors and networks beyond the lab.

Joel Mokyr, focusing on the 18th century industrial revolution presents a historical analysis of the role of useful knowledge.

Joel Mokyr

The presentation by Joel Mokyr (Northwestern University, USA) "The Scientific Basis for Technological Progress: the Case of the 18th Century Industrial Revolution" followed. The Industrial Revolution, which occurred in Europe in the late eighteenth century, undoubtedly constituted a watershed in the economic history of the world. It marked the beginning of a process of sustainable economic growth that rested on the growth and dissemination of "useful knowledge".

In his presentation, Mokyr recalled that while technological progress derives from many sources, a necessary condition remains the "knowledge of natural phenomena and regularities", knowledge he defines as "useful knowledge".

To frame this concept of „useful knowledge", Mokyr suggested an epistemological model, which distinguishes between "Propositional Knowledge" (science) and "Prescriptive Knowledge" (techniques).

The relationship between these two types of knowledge is critical because every technique has an epistemic base in propositional knowledge. The wider the epistemic base relative to the complexity, the more likely it is for the technique to be amenable to cumulative improvements and adaptations, allowing sustained technological progress rather than one-off inventions.

Before the industrial revolution, most techniques used were supported by very narrow epistemic bases.

"[Before 1800] Europeans made high-quality steel without understanding the basic metallurgy of steel; brewed beer without understanding the modus operandi of yeasts; bred animals without understanding genetics; and mixed elements and compounds without understanding basic chemistry. They manipulated power without understanding thermodynamics and fertilized their fields without soil chemistry. New techniques emerged as a result of trial-and-error and serendipity, and at times people got it rather astonishingly wrong (especially in medical technology)."

Part of the explanation for the narrow epistemic basis of technology before the 1800s is the fact that most of highly educated people, who were at the frontiers of philosophy, were not con-

(from the left) Gerhard Kratky, Luc Soete, Joel Mokyr, Helga Nowotny, Shaul Katzir, and the audience as they listen to the contributions.

cerned with practical matters. Furthermore, there was too much commitment to the orthodoxy associated with great authorities such as Aristotle, Ptolemy, Avicenna, and Galen.

Mokyr explored the major changes which happened in and around the 1800s and which can be seen as driving factors of the technological innovation firmly embedded in science. The emergence of this "Baconian Programme" is characterized by four main elements:

- changed agenda of research;
- improved capabilities of natural philosophers;
- changes in "market for ideas";
- changes in diffusion mechanisms.

Following Bacon's guidance, many scientists turned their interests to pragmatic and applied problems. One typical example showing this changing research agenda is René Réaumur (1683–1757), a French scientist who made a seminal contribution in a wide array of fields as varied as iron and steel, porcelain and glazing, egg incubation, meteorology and temperature measurement …

Improved capabilities were also characteristic for this era as seen in the development of better scientific instruments and advances in experimental methods. During this period, advances in calculus and probability occurred and were used in solving concrete problems in engineering and public health.

The "market for ideas" changed also considerably as a result of the increasing importance of experimental evidence. The trend to challenge "classical authority" had already begun earlier during the renaissance but it is during this Baconian Programme that the commitment to seek and establish the truth by means of experiment (as expressed in the motto of the Royal Society: "nullius in verba") gained its full strength.

Arguably the most important feature of the Baconian Programme was the fourth factor, the changes in diffusion mechanisms, both horizontally (between scientists themselves) and vertically (between the spheres of science and technology). In the horizontal communication we see on the one hand the wide spread of peer-reviewed high-quality journals, the beginning of encyclopaedias, technical manuals and "mass edition" of cheaper books, to exchange codified knowledge and the growing importance of other means to exchange "tacit" knowledge (scientific societies and academies; lectures and demonstrations and personal correspondence and networking, travel and mobility). On the vertical communication dimension, the pioneering works of "dual career" persons, who applied their skills and insights from scientific endeavours to mundane problems of production like Benjamin Franklin, Humphry Davy, Joseph Black, Count Rumford and Antoine Lavoisier were influential in building bridges between science and industry.

The actual contribution of the Baconian Programme to the Industrial Revolution is not easy to assess.

If we define the Industrial Revolution in the narrow sense of the word, it can be said that intellectual ferment, whose foundation lay in the Baconian Programme, did not „cause" the Industrial Revolution. Most of the canonical inventions (e.g. in cotton spinning, stationary steam engines, cotton, and wrought iron) depended little on advances in science. With a few notable exceptions, most "promises" of mathematics, chemistry, mechanics, electricity, botany, medical science, and biology before 1800 had not materialized.

Yet the Baconian Programme created the environment, which made possible the so called second Industrial Revolution (1865–1900), which depended on "useful knowledge". Its tenet that investment in useful knowledge would pay off and translate in the improved welfare of mankind and related optimism was without doubt an essential precondition for the second Industrial Revolution.

Joel Mokyr concluded his presentation by reflecting on the motives of the individuals, who were engaged in those endeavours. He sees an analogy between these people who led science and technology but did not make their living from it and were suspicious of anyone who did, and the modern actors in the "open source software". They were mainly motivated by ambition [impress your peers]; altruism [help bring about human progress]; curiosity [science posed hard metaphysical questions but was also fun] and security ("tenure"). For Professor Mokyr a state of mind, which, to some extent, is still true in our time.

Luc Soete

Luc Soete (UNU-MERIT, NL) started his contribution "Recherche sans Frontières: When Science Turns Global" by making two main diagnoses of the post-war European research system.

With respect to the continuum between applied and basic research, the European research system of the post-war period has been characterised by what Soete calls the "Dutch knowledge disease", an analogy to the well

"Science without borders", Luc Soete investigates theories "when science turns global".

known economic concept which explains the deindustrialisation of a country, which follows the discovery and exploitation of natural resources.

Typical for the "Dutch knowledge disease" is the phenomenon to be observed in virtually all major European countries of the crowding-out of fundamental research in private R&D on the one hand and the crowding-out of applied research in university research on the other hand. Whereas multinationals like Philips, Unilever or Shell

used to perform basic research in their laboratories on a large scale, we see a gradual erosion of research investment in fundamental research, as indicated by a substantial decline of publications in scientific journals authored by corporate researchers in the last years. At the same time, we see technical universities in most countries becoming more and more "academised" partly due to the new evaluation frameworks, which assessed their performance in terms of academic pu-

THE US FEDERAL RESEARCH FUNDING

In the US, six R&D funding agencies are directly responsible for about 96 percent of the federal R&D budget. There are the Department of Defence (DOD), the Department of Health and Human Services (HHS), the National Aeronautics and Space Administration (NASA), the Department of Energy (DOE), the National Science Foundation (NSF) and the Department of Agriculture (USDA). Main recipients of federal funding are universities, the industry and federal institutions administered directly by federal funding agencies (referred to federal intramural R&D). In addition to those institutions federal research grants are also awarded to the so called Federally Funded Research and Development Centers and smaller grants to the highly specialized non-profit research institutions.

There are over 30 Federally Funded Research and Development Centers (FFRDC) with a dedicated mission to perform research for the US Government. They were established in World War II to do research on nuclear weapons for the DOD and DOE, but today they are engaged in both military and civilian research. Examples of FFRDC include the Los Alamos National Laboratory, the Fermi National Accelerator Laboratory (Fermilab), the Lawrence Livermore National Laboratory (LLNL), the National Cancer Institute at Frederick (NCI-Frederick). The FFRDC are sponsored by government agencies and administered by non-profit organisations, universities or industrial firms.

Examples of the smaller, highly specialised non-profit research institutes are the Cold Spring Harbor Laboratory, the Scripps Research Institute and the Woods Hole Oceanographic Institution.

Founded in 1890, the Cold Spring Harbor Laboratory is a private, non-profit basic research institution. Its research programmes focuses on cancer, neuroscience, plant genetics, genomics, and bioinformatics. It has a staff of about 300 scientists and an annual budget of about US-$ 100 Mio. The Cold Spring Harbor Laboratory prides itself on seven Nobel prize winners who have worked there.

The Scripps Research Institute, founded in 1961, is a private non-profit biomedical research facility. It carries out basic research in several fields: immunology, molecular biology, cell biology, chemistry, neurosciences, autoimmune diseases, cardiovascular disorders, and cancer research. It has about 250 faculty members, nearly 815 postdoctoral fellows, 235 graduate students, and over 1,500 technical and administrative support personnel.

The Woods Hole Oceanographic Institution is a private, independent, none-profit corporation dedicated to research and higher education in ocean science. It was founded in 1931. It is funded by a mix of grants from federal agencies, private contributions, and endowment income. It offers education programmes in joint programmes with the Massachusetts Institute of Technology (MIT) for master and doctoral programmes.

At the end of each Session, the floor was open for discussion, which was made use of extensively.

blications, thus changing their original dual structures as institution between the business sector and academia. This movement has led to an increasingly growing mismatch between the academic research following the rationale of peer assessment on the one hand and on the other hand the business-run research part responding to immediate business concerns and focusing on producing new designs, processes and products.

The second diagnosis pertains to the relatively low private investment in knowledge in Europe. The post-war science system relied heavily on the public funds to allow access to knowledge both for citizens acquiring higher education and for firms for which knowledge constituted the bedrock of innovation. This system was shaken to its core in the 1990s, as the reduction of tax burden for both businesses and high-income citizens considerably diminished the funds which sustained it. Data on investment in knowledge (as measured by expenditure in R&D and Higher Education) show that although the level of public knowledge investment in the US and Europe are on par, the private knowledge investment in the US is more than double the level of the European Union average.

From this background, Soete echoed the speakers before him in highlighting a key challenge Europe faces: how to create an environment conducive to increase R&D investments by the private sector?

With respect to the public R&D investment, Soete pointed out a major difference between the US and the European system. The university landscape in the US is dominated by 200 research universities which account for about 96 percent of R&D expenditures of the 3,600 US institutions of higher education. These research universities receive the biggest share of federal funding from the six major research funding agencies (see box on p. 24). However, in Europe research efforts and research funding are spread across a multitude of universities. In his view, the differentiation among the universities in Europe should be accepted as a feature upon which to build on rather than seen as a development to oppose in favour of a converging mode. A look at research outside the universities shows that Europe has large research organisations of high quality and both at the European and national level high quality research programmes. A major difference between the two systems can be seen not so much in large federally funded research and development centers (FFRDC) but in highly specialised institutions like Cold Spring Harbor, Scripps Institute or Woods Hole, which seem to have no equivalent in the European landscape (see box on p. 24).

These institutions, funded largely by federal agencies, are characterised by close links to universities (incl. strong graduate programmes) and strong technology transfer records. They tend to be concentrated in small areas and have a faculty of about 150 to 400 persons. Considering their crucial role in US research performance, the second challenge facing Europe is: how to create Cold Spring Harbors in Europe?

Soete devoted the second part of his presentation to the far-reaching effects exerted by the globalisation in science and technology realms. We have today not only an exchange of codified knowledge on a much higher scale, but also global scientific communities, where knowledge is shared. At the same time strong localisation of knowledge can be observed. The knowledge appears to be a "joint" production between its two main forms (tacit and codified) and thus strongly subject to different local increasing returns and global access.

The result of this trend is a significant increase in basic science knowledge hotspots characterised by the agglomeration effects in knowledge. From this perspective, Soete identifies three broad categories of policy challenges:

- For high-income countries, such as Japan or the EU, the policy challenge is one of the sustainability of Schumpeterian dynamism.
- For emerging economies, the policy challenge appears more linked towards the design of "technological competitiveness" policies.
- For developing countries, the policy challenge will have to focus on the disarticulated knowledge systems, including the design of pro-poor innovation policies within e.g. the context of agriculture and rural developments.

These challenges share one common feature, which can be seen as decisive in shaping the S&T policy of the decades to come: the issue of access to knowledge.
On the global dimension, "collaborative innovation" can go hand in hand with the huge concentration of R&D efforts in the US, Japan and the EU with the emerging countries catching up. The access involves not only access to the required knowledge but also to the tools necessary to replicate and improve upon knowledge.
Local sharing of knowledge is also emerging as a valuable source of innovation, as shown in recent efforts of large foreign companies to engage in new forms of strategic alliances with public and NGOs to identify and address "pro-poor innovations" (development through profits).
The new source of innovation is therefore likely to emerge from those two sources: both the global and the local sharing of knowledge.
In this context the local public sector will play an important role in setting the fences of the commons in nature, but also in innovation.

Shaul Katzir

The Linear Model of Innovation is a theoretical framework, which is widely used to describe the impact of basic research. The basic tenet of the model is that the path from basic research to innovation is a linear process. The development of new products or processes is a result of applied research, which in its turn is based on discoveries made by basic research.
In his presentation "Basic, Applied Research and Technology through the Lens of Crystal Frequency Control" Shaul Katzir (Bar Ilan University, IL) closely examined this model on the example of the field of piezoelectricity.
The piezoelectric effect – the fact that certain materials, especially crystals, generate electric charge when submitted to temperature change – was discovered by the brothers Pierre and Jacques Curie in 1880. This phenomenon was studied by a growing scientific community over 30 years for purely scientific curiosity without any reference to potential technological applications. The first application came in 1915, when the military requested tools for underwater detection of submarines. Paul Langevin, a French physicist who knew well the study of piezoelectricity, suggested its use for ultrasonic detection in the prototype of the Sonar. This first use of piezoelectricity could be seen as an example, which validates the linear model: a direct flow of knowledge from basic to application.
Yet a closer examination of this and subsequent applications show a rather complex pattern. This can be illustrated by the development of the ultrasonic transducer devices. Those devices convert the ultrasonic sound waves in electrical current and vice-versa. Scientists and engineers, who developed piezoelectric transducers, could not simply implement pre-existent unchanged scientific knowledge by applying it to the problems of design at hand. Although piezoelectricity was firmly grounded in accepted theory and empirical knowledge, neither theory nor experiment described or examined high-frequency oscillations needed for the ultrasonic device. The development of the ultrasonic transducer required therefore an

Shaul Katzir gives a closer look at basic and applied research on the example of the field of piezoelectricity.

DEFINITION OF RESEARCH IN THE FRASCATI MANUAL

The OECD Frascati Manual proposes standards (methodological and classificatory) in collecting and using science and technology related statistics. The 2002 edition defines research as follows:
Research and experimental development (R&D) comprise creative work undertaken on a systematic basis in order to increase the stock of knowledge, including knowledge of man, culture and society, and the use of this stock of knowledge to devise new applications.
The Manual distinguishes between basic research, applied research and experimental development and defines them as follows:
Basic research is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundation of phenomena and observable facts, without any particular application or use in view.
Applied research is also original investigation undertaken in order to acquire new knowledge. It is, however, directed primarily towards a specific practical aim or objective.
Experimental development is systematic work, drawing on existing knowledge gained from research and/or practical experience, which is directed to producing new materials, products or devices, to installing new processes, systems and services, or to improving substantially those already produced or installed.

extension of the "basic" knowledge about the behaviour of piezoelectric crystals in this domain. This shows that the influence of basic research on applied research is not unidirectional and that technology affects "pure" science as well, even if the initial direction is from science to technology.
Today piezoelectricity finds a wide use also in quartz clocks and watches as well as in frequency control, wave filtering and light modulators. Those applications were made possible mainly by further study of the oscillations of crystals.
Studying the research notebook of Walter Cady, who made a seminal contribution in the field of piezoelectricity, Shaul Katzir found ample evidence that their discovery followed from a more general interest in the oscillations of crystals and their properties beyond their known utility.
The examples of application of piezoelectrity call for a questioning of the division between basic and applied research as popularized by the "Pasteur's quadrant". This concept, suggested by Donald E. Stokes, considers the research on two dimensions in respect to the main goals pursued by researchers: quest for fundamental understanding on the one hand and consideration for use on the other hand. Superposing those dimensions, Stokes identifies three main types of research: pure basic research (the Bohr Quadrant); pure applied research (the Edison Quadrant) and use-inspired basic research (the Pasteur Quadrant).
The study of application of piezoelectricity shows that knowledge and problems flow in two directions: from "pure" to "applied" science and back; the demarcation line between the two is not sharp. Katzir sees that important research is performed in a segment which can be called "theoretically oriented" science. Apparently, technological orientation to different degrees characterises great parts of modern science, as it occupies a wide share of the continuum between "pure" science and research aimed at technological design. This perhaps makes it too wide. Still, it may help understand the way knowledge about nature transfers into practical means for design.

Laurens Hessels and Harro van Lente

In 1994 Michael Gibbons and his colleagues published the book "The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies" which was to influence profoundly the discussions on the contemporary scientific practice. In their book they coined their concept of "Mode 2" as a way to what they saw as an emerging (and new), socially distributed mode of knowledge production.
In their presentation "Re-thinking New Knowledge Production: a Review of 12 Years of Mode 2" Laurens Hessels and Harro van Lente (Utrecht University, NL)undertook a review of the reception of this concept in the science policy studies and reflected implication.
Since its publication, "The New Production of Knowledge" (NPK) seems to have influenced the policy in science, technology and innovation discussions. With over 1,000 references in scientific articles, it is obiously very popular. In the same time, however, scholars have written numerous critical papers on the claims and the use of the "Mode 2" concept.
In the first part of his presentation, Laurens Hessels presented a systematic analysis of the reception of NPK. He identified seven (recurring) objections and critics to the "Mode 2" concept:

- The lack of empirical evidence for the claims around "Mode 2".
- The universality of the claims: in contrast with the generality of NPK, scholars expect the dynamics to be different in different national contexts, different scientific disciplines and different institutions.
- The necessary coherence of the concept: there might be a lot of multidisciplinary, application-oriented research that does not show organisational diversity nor novel types of quality control.
- The long-term historical perspective: the view of "Mode 1" as the original type of knowledge production is contested.
- Potential normative or political components.
- Lack of theoretical underpinning of the claims and references to sociological theory.
- Lack of a proper future outlook.

Laurens Hessels rethinks new knowledge production by giving a review of 12 years of "Mode 2".

The second part of the presentation compared the "Mode 2" diagnosis with a number of alternative accounts of current changes in scientific practice like the "Academic capitalism", Post-normal science", "Triple Helix", "Enterprise university", "Post-academic science", "Strategic research", "Finalization science".
It was shown that the NPK is by far the most popular concept in the literature. There seems to be a consensus that the model describes accurately some of the changes occurring in the research system like the changes in the choice of research agenda, the interac-

Joel Mokyr on the relation of basic and applied research: "I do not know when the night ends and when the day begins, but I could distinguish one from another".

tion with other societal "spheres" and the map of disciplines. Yet some of its diagnoses are severely disputed (changes in research methods, labour ethic and epistemology for example).
In his conclusion Laurens Hessels suggested to view NPK as a manifest rather than a scientifically sound theory of science dynamics. Although its claims seemed to point in the right direction, their underpinning (both theoretically and empirically) was insufficient and ignored the heterogeneity of scientific practice. In spite of these limitations however, NPK still provides a fruitful starting point for designing research about the changing nature of knowledge production. He concluded by listing the claims of "Mode 2" that called for further investigation.
(i) Transdisciplinarity: what is the degree of integration of scientific disciplines in collaborative research? And: is there a practice of flexible teamwork emerging? (ii) Reflexivity: can we speak of a new labour ethic of researchers; are they increasingly aware of the societal impact of their work? (iii) Quality control: taking into account the contradictory signs with regard to the accountability of scientists (strengthening of traditional academic norms and call for additional criteria), is there a demonstrable change in quality assurance of scientific research?

Discussion

In the discussions and reactions to the presentations in this first session of the conference a range of issues was addressed by both participants and speakers.
A participant commented on the role of military sponsored research. As important, applications of basic research are also made in response to military needs. He suggested taking into account moral and ethical implications when discussing the impact of basic research.
A second participant raised the issue of the innovation-friendly environment, which seems to be a precondition for a successful translation of basic research discoveries into economic and social valuable innovations. Innovation, he suggested, tends to take place in a specific environment in which elements such as industrial entrepreneurs and risk-taking venture capital play an important role. Discussions on this comment touched upon the shared role of the state and the private sector in innovation. Historical examples show that the state was rather passive in the industrial revolution. On the other hand we see today that the expectations on the state and public sector in innovation are growing. This can be witnessed in the growing involvement of the European Union in research matters. A comment was made that if the European Treaties were to be written, research would most likely be put in the European Community's goals.
A third participant questioned the viability of the distinction between basic and applied research as postulated by the OECD Frascati manual (see box on p. 26). If, as most speakers seemed to agree, basic and applied research were intertwined and not clearly distinct, why not abandon the concept of basic versus applied research altogether, the participant asked.
Answers reiterated that although the distinction between the two types of research was blurred and the two were indeed not easy to distinguish, nevertheless a distinction was appropriate in some cases. Using the metaphor of day and night, a speaker suggested seeing both types of research in continuum. "I do not know when the night ends and when the day begins, but I could distinguish one from another."
A distinction between basic and applied research can also be a useful concept in the allocation and management of resources. Research funding agencies use this concept in deciding on the criteria to assess research proposals. Some projects are assessed solely on the basis of their "scientific merit" (i.e how good they are in scientific terms as compared to others) and other projects have also to fulfil other criteria such as the likelihood of developing new products and processes in prototype as well as the size of the expected return on investment.

Science Impact

SESSION B
Funding Models and their Influence on the Impact of Basic Research






Erik Arnold, Technopolis (Chair), UK
Ian Halliday, European Science Foundation, FR
Susan Cozzens, Georgia Institute of Technology, USA
Chris Mombers, Technology Foundation STW, NL
Thomas Bechtold, University of Innsbruck, AT
Sam Wong, University of Leeds, UK

This session was the part of the conference dealing with the most "practical aspects" of the research funding business. Facing the change of the very nature of scientific research and the increasing complexity of science governance, the session aimed at discussing best practices in funding models in terms of maximisation of the social and economic impact of basic research. It was chaired by Erik Arnold, the speakers were Ian Halliday, Susan Cozzens, Chris Mombers, Thomas Bechtold, and Sam Wong.

Erik Arnold

The session was chaired by Erik Arnold (Technopolis, UK), who underlined the growing importance of new approaches that aim at a productive divide between science and society and set new framework conditions – both project-based and institutional – for the work of scientists at the interface between traditional structures. Terms like "innovation", "system", "(inter)national" etc. will have to be at least reconsidered (if not redefined) in the light of current developments, both in Europe and worldwide. The session should shed some light on future challenges and best practice models.

Erik Arnold chaired the first session of the second day, stressing the growing importance of new approaches that aim at a productive divide between science and society and set new framework conditions for the work of scientists.

Ian Halliday

Ian Halliday (ESF & SUPA, UK) focussed his presentation on current expectations of science change and the related challenges for funding agencies. Numbers of proposals and budget requirements are increasing dramatically, scales of money and/or time are increasingly divergent: many scientific areas require big investments in infrastructure in order to enable the work of large groups of researchers and ensure their success in the international competition. Furthermore, there is an increasing pressure on science to add to economy. Funding agencies are crucial partners of science, they will have to develop explicit strategies in order to find a balance between the various tensions, e.g. balance large, medium and small proposals (where to draw the boundary?), balance high-risk vs. "safe" research, support emerging areas vs. "entrenched" ones, decide about priorities (enforcing vs. diminishing areas, closing facilities etc.). In Europe, an additional challenge is the balance between national funding agencies and new instruments on the European level, i.e. the ERC. Funding of innovation is even more complex than funding of science: The borderline between research and "innovation" depends very much on the science area, interaction between both sectors has to be organised. Some lessons can be learned from successful models (e.g. DOE, DARPA in the US). The essential message is: Funding of science and innovation is seriously important and should be performed by experts. Pick the winners, get experts in and politics out, pay and fund people to carry out innovation. Funding agencies have to identify clear goals, clearly articulated strategies with long-term perspectives (about 15 years) and review mechanisms to ensure a constantly high quality. Governments and politics will then be more open to provide more money.

Ian Halliday: "Funding of science and innovation is seriously important and should be performed by experts."

Susan Cozzens

Susan Cozzens (Georgia Institute of Technology, USA) addressed the tension between economic growth and socially stable and sustainable societies and the problems connected with maximizing (and identifying) the social impact of science. If research is going to be oriented increasingly towards commercialisation, research policies are challenged to preserve social oriented mechanisms and develop new ones. Cozzens suggested a new "mo-

del" for the understanding of "basic research": based on D. Stokes' model of the "four quadrants", her version emphazises the interaction between the quest for basic knowledge and the consideration of its use. When new "pure knowledge" is created, the "quality of life returns" should also be taken into account. This approach would have a significant impact on how programmes are designed and run, how the performance and success of programmes are analysed and judged. Bringing people back in would free us from thinking about targeting research and focus more on building capacities. A three-dimensional model should connect knowledge, practice and education. Outcomes tracing analysis should examine both how and what goals are

Susan Cozzens: "Redistributive policies should ensure that social aspects are integrated in research and innovation."

achieved, use a mix of indicators to track change in desired outcome areas, identify the portions of change that can be attributed to research by expert judgement, clear timing of change and causal connections. The "Cozzen's Thesis" states that indicators in relation to public goals for research are numerous, but there is a lack of logics that connects research and innovation to outcome indicators. Examples for analysing the social impact of research were presented from Biomedical Research and Agricultural Biotechnology in the US: A comprehensive "logic model" considers the interaction between reseach and commercialisation in relation to public and private goals and strategies, as well as public and private benefits and costs; strategy is crucial. Redistributive policies should ensure that social aspects are integrated in research and innovation. Such policies should have three main components: pro-poor policies (reduce poverty or alleviate its conditions), egalitarian policies (reduce vertical inequalities), policies to ensure fairness (eliminate horizontal inequalities). Examples for such policies and types of intervention as well as design principles were presented.

Chris Mombers

Chris Mombers (Technology Foundation STW, NL) gave a comprehensive presentation and discussion for a best practice model for stimulating, organising and funding knowledge transfer from public research organisations to private enterprises; i.e. the "Open Technology Programme" of STW. The Programme is supported by NWO and the Dutch Ministry of Economic Affairs and gives an impressive demonstration of a good co-existence of basic and applied research. The programme funds academic research projects with potential utility with a volume of EUR 30 mio p.a. The programme is thematically open (no thematic areas defined) with no submission deadline. At present, nine General Universities, three Technical Universities and one Agricultural University are involved. By 2004, ten diffe-

Chris Mombers explains a best practice model for stimulating, organising and funding knowledge transfer from public research organisations to private enterprises.

rent scientific disciplines were involved, including life sciences, medical sciences, earth sciences and a variety

(from the left) Susan Cozzens, Erik Arnold and Chris Mombers in the morning session of the second conference day.

of natural and technical sciences. Projects have to be multidisciplinary and include a utilisation plan; programme management with user committees is required. The objectives of these user committees are knowledge transfer, building up new social networks between researchers and users, focus academic projects on applications, exchange knowledge about the state-of-the-art technology, providing a platform for establishing IPR and patent exploitation (rule of discipline: publication of results only after approval of user's committee and STW). The programme is extremely sucessful in terms of networking between academia and users: Types of users are big companies, SMEs, the government, social groups (e.g. nature, health, environment), scientists from other disciplines, start-up companies, the community at large (e.g. open source & internet) etc. A variety of interesting success stories was presented. With its well-defined knowledge trade policy, the programme also successfully co-ordinates opposing values and common interests of the various partners involved, e.g., requests for academic freedom and open discourse on the university side and the need of confidentiality and limited public discourse on the industry side. The interest in the use and commercialisation of new and useful technologies is the "connecting tissue" between both parts. Living the "cyclic innovation model" and applying a demand-apply model for valorisation of scientific results are central to the programmes' success story. A recent evaluation of STW (2006) clearly shows, that open programmes work at least as good as focussed programmes and provide an excellent tool for increasing the impact of (basic) research.

Thomas Bechtold

Thomas Bechtold (Research Institute for Textile Chemistry and Textile Physics, University of Innsbruck) presented the success story of the Christian Doppler (CD) Laboratory for "Textile and Fibre Chemistry in Cellulosis". A CD Lab is supported by the Christian Doppler Research Association (CDG). CDG is a non-profit association that aims at promoting development in the areas of natural sciences, technology and economy as well as for their economic implementation and utilisation. It enables talented scientists in renowned research centers to achieve high-quality research and knowledge transfer in line with the demands and to the advantage of the CDG member companies. The backbone of a CD Lab is a consensus between academic and industrial partners about budget and work plan. The budget of a CD Lab is between EUR 110.000 and EUR 500.000 p.a. (50 percent public funding, 50 percent industry, foreign companies may be partners) for up to seven years; the size of the research groups involved is 5–10 scientists. The given CD Lab "Textile and Fibre Chemistry in Cellulosis" represents competence, "critical mass" and synergy in all relevant steps from wood (fibre formation) to consumer products. It succeeded in building up a dense network of national and international academic and industrial partners. Academic success (publications, career

Thomas Bechtold, presenting the success story of a Christian Doppler Laboratory.

development) is as important as applied results (knowledge transfer, patents). The Lab lives up to the principle of Christian Doppler (Austrian Physicist, 1803–1853): "The most rewarding research is the one that delights the thinker and at the same time is beneficial to humankind".

Sam Wong

Sam Wong (University of Leeds, UK) talked about "Maximising Impact of Interdisciplinary Research on the Ground – constraints and results". The starting point of his presentation was that scientists are using public money and, consequently, they should be obliged to maximise the impact of their research on society and economy. His case study was an EPSRC-funded project with a volume of £ 7 mio, involving seven universities and eleven industrial partners as well as a variety of scienti-

Sam Wong talked about "Maximising Impact of Interdisciplinary Research on the Ground – constraints and results".

fic disciplines (engineering, psychology, environmental sciences). Strong research collaborations between UK research councils, involvement of industry and NGOs set an environment that enables an exchange of ideas and experiences, interdisciplinarity and co-cooperations across institutions. Main challenges are time restrictions for discussing assumptions and expectations of collaboration due to calls for grant proposals, over-preference of certain disciplines (social sciences feel marginalised) and the often unclear role of industry, which may result in conflicts. Solutions could be: 1) modifying criteria for the success of proposals, i.e.: not number of packages, but quality of collaboration, 2) jointing research councils in order to break artificial boundaries.

Discussion

One point of the general discussion was how to design and perform peer review for interdisciplinary projects. It was stated that there are probably no "interdisciplinary problems". But there are problems that require interdisciplinary research teams. Vertical splitting of research councils, as it is the case in the UK, is certainly a problem for organising (funding) interdisciplinary research teams. It was admitted that peer review of interdisciplinary projects is a problem. Peer review groups or "Laymen Jurys" that do comparative review of proposals, could be a solution in some cases. Research funding institutions are well advised to treasure peers with experience in interdisciplinary work. Basic research results are unpredictable to some extent (otherwise research would not be necessary), so developing evaluation criteria is not easy. If review should be done by experts, it is necessary to define the required expertise.

Another point addressed the impact of basic research: If pure bottom-up models are preferred, how could it be ensured that basic research would actually lead to better models for, e.g., pro-poor, egalitarian and fairness policies? The discussion emphasised the importance of time scales, when impact and applications of research are considered. The time lag between basic scientific knowledge and applications is in the average still in the order of 50 years. For politics it is almost impossible to judge on the consequences of basic research, when they are forced to take decisions on the distribution of funds. The challenge is the "transformation process" of science, how science could achieve a broader basis in society. Social impact can hardly be measured, but a variety of parameters could be identified that indicate such an impact. Major societal changes are usually reflected by the kind of research work that is performed. In this way, the effect of research can be linked to programmes. On the other hand, persuasion could often have greater impact than research. Looking for impact, emphasis should be put on analysing the mechanisms for the distribution of information. In general it was agreed that a great variety of mutually dependent and interacting parameters have an influence on the impact of basic research. Thus, the identification and, even more so, quantification of basic research impact should be handled with great care.

The central importance of the role of basic research was then stressed in the discussion about the question of how innovation could be "done". Is it that simple like "give me 50 years and money"? It was admitted that the time lag between a scientific breakthrough and a useful product is about 50 years. Innovation rarely takes that long. Usually, already available information and scientific knowledge is used for innovative developments. The paramount importance of basic research is to nurture and constantly extend this pool of knowledge. Planet earth is a closed system, but science and innovation are not. This should be brought to the attention of politics.

Science Impact

SESSION C
Scope, Limits and Role of Impact Evaluation






Helga Nowotny, European Research Council (Chair), B
Ben Martin, University of Sussex, UK
Benoît Godin, INRS (Montreal), CDN
Wolfgang Polt, Joanneum Research, AT
Claire Donovan, Australian National University, AUS
Soile Kuitunen, Net Effect Ltd., FIN

The final session was titled "Scope, Limits and Role of Impact Evaluation". It was chaired by Helga Nowotny. She also substituted Benoît Godin who was unable to be present. Further speeches were delivered by Ben Martin, Wolfgang Polt, Claire Donovan, and Soile Kutunen.

Helga Nowotny

In her brief introduction, Helga Nowotny (ERC, B) considered the reasons for measuring the impact of science. Scientists and those who work with them are naturally under pressure to justify their work, but this is not the only reason. Increasingly there is a wish to understand technological visions, risks etc. – in short, to grasp the larger picture. And finally, scientists and the insti-

Helga Nowotny, Vice-President of the ERC, chaired the final session of the second day.

tutions where they work are motivated by their own curiosity. All of us are part of the transformation processes induced by scientific research and institutions are attempting to reflect this in their understanding of their function. Impact is not just something that has happened but something that is happening now. We need to look at the potential consequences of the work we are doing. The higher the knowledge content of the scientific work being carried out, the greater the requirement for sociological understanding of the work's potential.

Ben Martin

Ben Martin (SPRU, the Science and Technology Policy Research Unit at the University of Sussex, UK) asked how the impact of basic research could be assessed. Is it the aim of the conference to convince the participants that there was one true "faith" with regard to improving the prospects for humanity, in parallel with the Jesuit priests who had debated similar issues in the same room several centuries earlier? The task of assessing the impact of basic research was little easier than the parallel task faced by the priests; the benefits come in a variety of forms and thus their assessment is far from trivial. There have been numerous debates on whether governments need to fund research and, if so, at what level. A significant proportion of government spending now goes on research, generally 2–3 percent of GDP in most developed countries. It is understandable that governments expect some degree of ac-

Ben Martin, the long-time director of SPRU, asked how the impact of basic research could be assessed.

countability for this money. It is generally easier to make a case for spending money on health or education than on science. In recent years, SPRU has been trying to prepare more convincing arguments for spending significant sums of public money on research.
The underlying question of "What are the benefits from science?" implies a linear model, in which those benefits are compared with the original inputs: But can the expenditure on science be simply related to the outputs? Scientific discoveries in the early 20th century and during World War II gave rise to the linear model, which has since been used to justify increased spending on

research. But the relationship of science to technology is not linear; furthermore, it is hard to link benefits to particular scientific work. It is likewise hard to determine what proportion of subsequent technological developments or innovations should be attributed to research, as opposed to other factors. Finally, the importance of long-term benefits may be underestimated, especially if measurements of impact are made too soon. The fundamental point is that, in most attempts at assessment, the main output of research is assumed to be useful, codified knowledge; tacit knowledge (as discussed in a previous session) is not taken into account.

Martin described several different approaches for measuring the impact of science, noting the necessity for making sometimes unjustified assumptions in their application, the problems caused by sample bias and the difficulties of generalising from the results. Econometric studies have generally indicated a high rate of return from science (20 to 50 percent), but they have a number of intrinsic limitations, the effects of which are hard to gauge.

Another method used have been surveys of industrial R&D managers, asking about their dependence on university research. The method also faces a number of substantial problems, but the results, around 28 percent in one prominent study by Mansfield, have proved useful in securing funding for research. Studies of citations in patent applications and case studies have also been used with some success in other approaches to assessing the changing impact of science.

SPRU has attempted to develop a more comprehensive approach to assessing research impact. SPRU distinguishes seven types of "exploitation channels", some of which are more easily measurable than others. It is difficult, for example, to quantify the impact of training skilled graduates, despite the widespread acceptance of the importance of this as a goal of science. Likewise, "invisible colleges" or worldwide networks are a valuable resource and their formation is often stimulated by research.

Unfortunately, the benefits of many of these exploitation channels are extremely difficult to quantify. Nevertheless, Martin stressed the importance of considering all of these channels in any attempt to assess the impact of scientific research. Focussing on just one or two of the channels was potentially dangerous, in that the results were likely to give rise to inappropriate policies.

THE SEVEN PATHWAYS OF THE IMPACT OF THE BASIC RESEARCH

The Science and Technology Policy Research Unit (SPRU) at the University of Sussex regularly undertakes systematic and comprehensive review of existing studies on the economic and societal benefits of publicly funded research. The studies undertaken to assess the benefits of basic research can be divided into three broad categories: econometric studies in which large databases are analyzed to estimate the rate of return to research; the survey of industrial R & D managers to assess for a example to assess for example how different industries utilise research results from different fields and case studies to trace historical input to innovations. As Ben Marin stressed in the conference, any of those approaches has its limitations, however taken together they help to uncover the various forms in which basic research makes economic and societal impact. Based on those studies, SPRU developed a seven dimensional typology to classify the benefits of basic research. According to this model, seven, relatively distinct "exploitation channels" can be identified:

- Increasing the stock of useful knowledge both codified (publications) and tacit (skills, know-how and experience),
- Training skilled graduates on whom firms heavily depend on to make innovations,
- Development of new scientific instrumentation and methodologies which are often also useful in context beyond which they have been developed,
- Forming networks and stimulating social interaction. Researchers are part of the "invisible college" of peers, which provides a platform for researchers from the industry and a forum from which firms access expertise.
- Increasing the capacity for technological problem-solving on which firms rely to solve complex technological challenges which require often combining technologies.
- Creating new firms as spin-offs to exploit research results from basic research.
- Provision of social knowledge for example in cases of social sciences providing basis for public policy or the impact of the humanities on "creative industries".

Benoît Godin

Owing to illness, the next speaker, Benoît Godin (INRS in Montreal, CDN) was unfortunately unable to be present. His presentation was delivered by Helga Nowotny. The full text of his talk was made available to the participants and its contents are summarised only briefly here. Godin noted that people had been trying to measure the impact of science and technology for over 100 years but the underlying problems have still not been solved. He traced society's interest in technology's social impacts to the end of the 18th century and the start of the 19th century. The problems of the early 20th century, such as World War I and the later "technological" depression also focussed sociologists' interest on science and technology. We owed an enormous debt to sociologists of the early 20th century, especially to the American so-

Ben Martin, Wolfgang Polt, Claire Donovan and Soile Kuitunen during their Q&A session.

ciologist William Ogburn (b. 1896), who devoted much of his energies to an examination of science and technology and its effects on social change. Godin devoted a large amount of his talk to a discussion of Ogburn's work. The measurement of "effects" (as opposed to "outputs", such as publications) in

Benoît Godin submitted his conference contribution but could unfortunately not make it to Vienna.

Ogburn's time was detailed. Introduction of the concept of "effects" was very valuable to the field of science and technology, as it enabled forecasts to be made, although these often relied on correlations and extrapolations. Subsequent to Ogburn, sociologists moved away from the study of impacts, leaving it instead to economists. There is still no theory linking science and technology to society and a conceptual framework is still lacking. As Godin put it so eloquently, "The source does not know the ultimate user."

Wolfgang Polt

The next speaker was Wolfgang Polt (Joanneum Research, AT). Governments and funding agencies were increasingly asking for information on the monetary outcome of research. Polt did not have an easy solution to the question of how to provide such information, giving the audience instead an indication of what could not be expected from any such study. The perennial question, "Are we spending enough on basic research?" remained as hard to address as ever, although many organizations held strong views on it. For example, the EC had stated recently (2003) that nearly all technologies that have led to economic and commercial success were based on basic research, a strong and important statement.

Few studies have focused on the impacts of research. Most have looked instead at R&D, a much broader concept. And the comparability of different studies has been limited by their different focuses: on university or public research, on strategic research, on publicly funded research etc. Comparatively little is known on the impacts of basic research. Economic impacts have been examined at the micro, the meso and the macro level; Polt gave illustrations of each of these. His "macro" illustration showed an apparently inverse relation between countries' GDPs and the proportion of the GDP they spent on basic research – this was presented as warning against such simplistic analytical methods and showed that funding agencies should not be pressured into looking for spurious cor-

Wolfgang Polt: "The return on investment in university research is very high".

relations. Case studies at the meso and the micro levels consistently showed high spillovers from spending on basic research into increased profitability. Polt cited a study by Bessette (2003), who had calculated the return on investment from universities and several different companies; his calculation was based on very short-time periods but suggested that the return on investment in university research was very high. It should be noted, however, that his model was more of a monitoring tool and looked only at very short-term returns. And some examples with very high economic payoffs had not resulted in products but had merely lowered the costs of experimentation, for example by reducing the number of experimental animals needed for tests.

Claire Donovan feels that meaningful impact assessment should strive to measure the wider public value of research.

Polt finally turned to the issue of what could be done with the results of such analyses. He felt that the results of studies could potentially be used to justify expenditure on research, although much care should be taken in their interpretation. There had been considerable advances in methods to quantify the effects of R&D, but fewer on quantifying the effects of "basic research", a term that is not always understood in the same way. It remained difficult to perform a cost-benefit analysis, when neither costs nor benefits were precisely known. The scientific community should do its best to fend off questions of this kind, because the tools available to address them were not available.

Claire Donovan

After the break, Claire Donovan (Australian National University, AUS) continued the discussion of methods to measure the impact of research. The findings of her thesis were that economic indicators represent low-level impacts and favoured private over public interests. She felt that meaningful impact assessments should strive to measure the wider public value of research in terms of the "triple bottom line" of social and environmental as well as economic impacts; these were elusive to quantitative analysis, but were nevertheless extremely important. She conceded that the concept of "triple-bottom line accounting" had been called into question when approached in purely quantitative terms. In Australia, a national research evaluation exercise is underway, which attempts to evaluate social, economic and environmental benefits (and a fourth criterion of cultural impact) on a five-point scale. This is essentially a qualitative exercise, although impact claims may be supported by suitable metrics.
Donovan proposed that future efforts should be focussed on best-practice in assessing the returns of medical research; these qualitative approaches to impact evaluation could be applied to other disciplines. She presented several examples to illustrate the approach. The approach was very different from more economically based models, and measured the public value of research, which eludes quantitative approaches.

According to Soile Kuitunen it is important to identify the beneficiaries of developments.

Qualitative measurement therefore had an important part to play in evaluating the impact of scientific research.

Soile Kuitunen

Soile Kuitunen (Net Effect Ltd., FIN) gave the next brief presentation, providing an overview of a model she had been developing (with Katri Haila) to evaluate the societal relevance and impacts of science. She summarised the need for evaluating the impact of science in a more comprehensive manner than is possible using earlier methods. The majority of studies has not looked at the final outcomes of research, the real impacts, but have focused on the results and the outputs. It was important to identify the beneficiaries of developments – what strategic purposes are served by public R&D&I? It was important to look into the future: How is the development likely to proceed? More complex models, such as the multi-dimensional model presented, should give better results that would represent sources of opportunity and innovation for the R&D&I community.

Discussion

All four speakers then took their seats at the front to participate in the final discussion. It was noted that there was some light on the horizon: the development of methods was difficult but there was current work that appeared to be promising. However, the idea of comparing combined criteria in assessment (the question related in particular to Donovan's presentation) seemed very difficult. Donovan agreed that many scientists seemed to prefer quantitative indicators but efforts to raise the acceptability of robust qualitative approaches were underway. The use of the word "adoption" was also queried: Adoption of a model seemed to imply a leap of faith that should not be required to accept a scientifically based methodology. And starting at a point where reinterpretation of the model for the humanities, arts and social sciences was required could hardly be a good idea!

A further comment from the audience was that many researchers gained a large proportion of their funding from industry: This represented a form of impact and could be included in Martin's analysis. Martin felt that the point was already covered, although he conceded that the taxonomy of exploitation channels could be further refined. Indeed, a fresh "exploitation channel" had recently been incorporated to reflect the impacts of social science.

A further participant noted that societal impact was by definition a long-term process. But there was a political and financial necessity to balance income with expenditure. This idea had been well summarised in Godin's presentation of the cultural lag, but it is often neglected in analyses of the impacts of society. Martin agreed that the issue of time-scale was extremely important. Case studies had taken particular examples and looked at the scientific and technological developments that had been required for them. They yield very different results depending on the length of time involved (the longer the time period, the greater the importance of basic research). But assessments based on shorter time-frames are necessarily more easily applied to policies than longer ones.

There was a question on the distinction between "useful" and "useless" knowledge (that had been made in a previous session), if such a thing existed. It was also noted that all discoveries could potentially be used both for good and for evil. Polt responded that the term "useful" had been applied in a particular context as meaning "giving rise to an application". Nowotny added that the distinction between "good" and "evil" research was not always clear and depended on the context.

A participant wondered whether the discussion was purely academic or whether it was being applied: Were public funding bodies requesting scientists to provide information on different aspects of the impact of their research? Polt replied that the EU asked applicants about any potential economic or societal impact of their research; Nowotny added that the European Research Advisory Board had noted that the usefulness of such information was highly limited. Researchers tended to tick boxes and evaluators simply discarded the answers. Polt added that there appeared to be a gap between the people performing the research and those who could potentially apply it.

Eric Arnold wondered whether we might be posing the wrong questions. The simplistic model of the "national innovation system" was not leading very far: it might be time to ask more fundamental questions. There seems to have been little real progress in the past few years. Martin agreed: we seemed to be dealing with an intrinsically intractable problem. He considered the economic benefits of the buildings along the Ring in Vienna. Had economists been asked on the sense of building these at the start of the 20th century, would they have been in favour? There was a grave danger that the long-term benefits would have been neglected and the money would have been spent differently. Polt agreed and felt that economics should be less focussed on questions of impact but should return to its roots as a social and behavioural science, considering the influence of expenditure on society and behaviour.

Kuitunen agreed that less emphasis should be placed on identifying all possible types of impact; instead, the effects on society should be looked at more globally. Donovan agreed; attention had detracted from the main point, which was the trend away from technometrics and sociometrics towards qualitative triple bottom-line assessments. She felt that these had a very bright future.

Nowotny closed the session by thanking all speakers, including Godin, for their contributions.

Science Impact

CONFERENCE SUMMARY

Stefan Kuhlmann, University of Twente, NL

Stefan Kuhlmann of the University of Twente had the task of summarising the meeting. He noted that the conference had not only focused on measuring the impact of science, but wanted to address whether there was a positive impact of science. The debates over the past two days had shown clearly that there was such an impact.

Kuhlmann felt that it would be scarcely possible to summarise the entire conference. Nevertheless, some key points and conclusions could be derived. It was important to consider whether "basic" research formed the focus of the discussions; Kuhlmann proposed the use of the relatively new term "frontier research", which means cutting-edge, high-risk research. It did not show clear borders between disciplines and transcended national borders. The identification of "impacts" was also controversial: Impacts were essentially defined by those who study the question and their definition assumed an implicit model of input:output relations of cause and effect. The term "impact" was little more than a rational construction. Various speakers in the conference had presented various classifications of impacts, and there seemed to be little consensus on what should be taken into account, nor on what should be considered as a "positive" or a "negative" impact. The impacts of science were very broad and it was not possible to cover them all.

Stefan Kuhlmann summarised the conference briefly and took the part of moderating the final discussion.

Frontier research was a creative enterprise and its impacts on society were elusive. Forcing it to have an impact would undermine the foundations of science. But organised, sustainable science could not exist (would not be funded) without impacts. The present conference had provided a number of insights:

- The relevance of science and frontier research for modern society was confirmed. It is highly beneficial provided that there is sufficient funding and freedom and that there is sufficient interaction between science and society/the economy. There also needs to be a reasonable discourse to overcome the divide between the "dreams" and the "fears".
- Theoretical explanations were debated but there was the general agreement that scientific research is historically a need for a better understanding of the dynamic interrelationship of science, research, technology, knowledge and their impact.
- Funding models must be tailored to achieve optimum impacts. Different tensions are faced by different fields; we need to better understand the dynamics of knowledge in different fields.
- A broader view of impact evaluation was required to reveal all the potential benefits of science. It is now time to come up with a definition of "impact", which has remained elusive despite much effort. Impact assessment is still unable to capture the full range of the benefits of frontier research, although it is clear that these come in a number of "exploitation channels".

Finally, Kuhlmann drew four broad conclusions from the meeting:

- Economics has shown us that investing in science gives rich returns and that a "knowledge-based economy" is gradually taking command. The history and sociology of science have revealed that research is a special sphere of modern society, interwoven with different but overlapping social practices and values. "Social shaping" gives rise to a "desirable impact" of science and frontier research.

At the concluding "round table" speakers and chairs from the three sessions reentered the stage for a final discussion.

- Communication between frontier research and potential users is becoming increasingly important, even if such communication is sometimes controversial.

- Public policy should be encouraged to treat science and knowledge production intelligently to maximise potential returns and impacts. It was important to avoid general policies and concepts that did not pay sufficient attention to the dynamics of individual situations and particular disciplines. This point was complex and would require a further conference to develop fully.

- The allocation of research funds was generally based on the results of peer review procedures. These should be extended to take potential impacts into account. To facilitate this, there was a need for the development of "post-national", problem-oriented theories on the analysis of science and its impacts.

Round Table

All speakers expressed their thanks to the organisers for preparing so comprehensive a programme, which had included scientists and representatives of funding bodies. All participants would leave the meeting with a better appreciation of the complexity of the field. As a result of the conference, the speakers were persuaded that "frontier research" was highly important and did pay off in a large number of ways, although these ways were not very well understood. The conference should be seen in the framework of the increasing dialogue between funding agencies and policy makers: Scientists and funding agencies should not be afraid of stating that politicians sometimes had unrealistic expectations. It would be important to avoid general policies that may not be appropriate in all countries: The EU, in particular, would face considerable difficulties if it tried to adopt a "one size fits all" approach to research policy. The interactions between local, international and global policies would need careful monitoring.

It was noted that policy makers question the impact of research on society because they want it to have an impact. It is important to address the impact of novel technology, which is highest when the technology is embedded in local technology, although it is often understated. When technologies become less visible, their impact is often underestimated. We are presently living in a golden age of science funding in Europe. This cannot be expected to last forever, and when the bubble will burst, it will be important to come up with good arguments to fund science. And quantitative arguments might carry far more weight than non-quantitative ones, especially to non-sociologists. The importance of the civil service should not be underestimated: Scientists should make more efforts to understand the constraints under which civil servants have to operate. It was important that policy makers trust people who perform research: Scientists should help restore and foster this element of trust to help the perception of science as a public good in which investment is required. Such trust must be earned on a daily basis and gaining it cannot be delegated to public relations officers brandishing glossy brochures.

Science Impact

CLOSING EVENT

It did not take long for Gerhard Widmer and Erika Chun to convince the audience of the "beauty of basic research".

COMPUTERS AND MUSIC

"Years ago, when I started to work in the field of computers and music, which by the way wasn't much of a field then, most people considered the combination to be really strange or rather useless. Now, of course, this perception is changing rapidly ..." In his opening statement, Professor Gerhard Widmer of the University of Linz made clear – not without pride – what he has accomplished in the past few years. We now know that a number of applications have emerged in Widmer's field. "However," Widmer continued, "tonight I should like to invite you to forget about the application aspects of this kind of research and enjoy the luxury of pure basic research, which carries no constraints related to applications."

To help the listeners enjoy "the beauty of basic research", which was the title of his speech, Widmer went on with one of his favourite quotations: "The beauty of basic research is that it goes where no man or woman has gone before, the uncharted frontier of science". This explains why Widmer feels so passionately about his work. "It's the fascination of studying something that nobody has ever studied before".

Before the computers had their turn, the conference participants were treated to a live performance of Wolfgang Amadeus Mozart's second piano sonata, performed by Erika Chun on a Bösendorfer computer grand piano named "Zeus". Widmer then posed the question that was in the minds of all the conference participants. "Isn't it wonderful to hear what musicians can make of a piece of music, a dead piece of paper? How do they bring it to life?"

A musician brings music to life by providing a so-called "expressive performance". Each musician plays the same notes in a different, individual style. What Widmer has achieved, is to represent this individuality on paper, or more precisely on the computer. Based on different performances of the same piece of music by different musicians he showed, how individual playing styles can be visualized and transformed into a graphic analysis. Classical musicians do not play music exactly as it is written – which would sound mechanical – but rather continually vary various parameters. The most important parameters in musical performance are tempo and timing, i.e. speeding up, slowing down or delaying notes; dynamics, i.e. changes in volume; and articulation, which essentially means the way notes are connected to one another. Most of these points are not stated explicitly in written music but left to the musical understanding of the performers.

Such an analysis would not be possible without data. Obtaining such data is currently the most difficult part of Widmer's work. Precise details of dynamics, articulation, timing etc. are required. In other words, it is necessary to know exactly when each note is played, how it is played, how loudly and for how long. The data can only be "captured" by the use of special musical instruments like "Zeus", which is packed with extremely delicate sensors that precisely measure every action of the performer, every movement of the keys, the hammers inside and the pedals and store the information in computer-readable form. Based on the information, it is possible to reconstruct

Gerhard Widmer, using the "Theremin" to play the piano as if played by the invisible hands of a ghost.

every detail and every nuance of the performance.

Widmer can visualize musical expressivity using the "worm", a computer programme that his collaborators have developed. The results are a two-dimensional animated graph based on the tempo and loudness of different pieces of music. Widmer used this analytical method to display the different styles of famous pianists performing the same piece of music. The audience was truly fascinated to see the differences among famous pianists and it became clear why each has favourite pianists, although each of them play the same piece of music at a very high musical level.

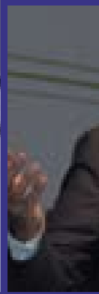
Is it possible that a computer might be able to learn to recognize an artist based on characteristics of their "manner" of performance? According to Widmer, the answer is yes. His computer first "listens" to musicians and then "learns" to understand their way of playing certain pieces. Afterwards, the computer is again exposed to a piece of music but this time the machine is not informed who is playing. And with a "hit rate" of on average 80 to 95 percent, the computer can correctly identify the performer, based solely on information relating to tempo and changes of volume from beat to beat. No further details are required.

Widmer also addressed the issue of whether a machine could itself "learn" to play music expressively by simply listening to musicians and analyzing their recordings. Widmer had the computer play a piece of Mozart to which it had never previously been exposed. The machine only "knew" how to play Mozart from other pieces. And the result? Well, as Widmer admitted with a smile, "we even won a prize at an international computer performance contest in Japan but the answer to the question is a definite 'No'. Computers will never get close to the performance standard of human musicians."

Finally, Widmer moved from the computerised visualisation and analysis of data to the active control of performance. To do so, he had the computer play notes mechanically while he controlled the Worm (tempo and loudness) by moving his hands above a "Theremin", which consisted of two antennas that sensed the position of his hands. It was a thrill to see Widmer (and, of course, his computer) play Chopin merely by moving his hands over some antennas – and via the computer connection, "Zeus" appeared to be playing by itself. As if played by the invisible hands of a ghost, the piano keys were moving and Widmer – as master of this mysterious scene – had convinced even the most sceptical listeners in the audience of the beauty of basic research.

IMPRESSIONS OF THE CONFERENCE



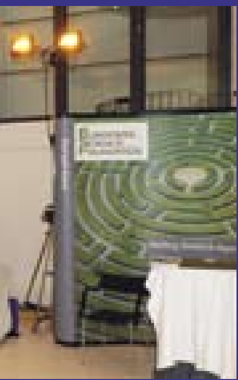









Science Impact

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