Danmarks
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Danish National
Research Foundation

TRANSFORMATIVE
RESEARCH

How, what and why?

Content

Navigation: You can click on the bar to the right, arrows and the table of contents. Click on this icon: ☰ to go to table of content.

Print: This document is configured for printing on a horizontal A4 sheet in full size without margins.

Transformative research **– how, what and why?**

There are several examples of research that has become transformative often through a series of unpredictable events and winding paths, but nevertheless undeniably transformative. Among the many examples of transformative discoveries are antibiotics, electromagnetism and transistors.

The numerous examples of research that has become transformative make evident that research has an enormous potential to impact on our lives. Research has – for good or bad – a built in potential to transform everything from the progress of science as a whole, over nation's ability to compete in tomorrow's global economy, to solutions to great societal challenges.

Although these examples showcase research that both changed the scientific field and the society there is also excellent research that primarily transforms one but not the other. The discovery of the Rosetta Stone led to the understanding of the hieroglyphs, and the redshift of light from distant stars led to the notion of the expanding universe and the big bang theory but has so far not changed our society remarkably. On the opposite, the scientific efforts behind the development of the combustion engine or the clinical diagnosis code system to be used in epidemiology research has significantly impacted our society without being highlighted as basic research.

From the political level to public and private funding bodies, there is a clear ambition globally to pursue what is conceptualized as transformative research. This is reflected in notions from various funding agencies as for example: "The ultimate goal of the ERC Synergy (SyG) scheme is to allow for a close and genuine collaborative interaction that will enable transformative research" (European Research Council), "Where To Submit Potentially Transformative Research Proposals" (National Science Foundation); "The ideas might hold the potential to respond to a long-standing question in science, bring a transformative understanding of a central topic, nurture a fruitful new research area or creative method/technology or the like" (Villum Foundation).

Political leaders have a special focus on research's potential to provide truly transformative solutions to societal challenges. Most researchers have a similar commitment, but it is also essential to them to be fully submerged in pushing the frontiers of what is known in specific fields without a view to the immediate impact on society.

Research funders and policy makers can play a key role in uniting the focus on research's potential to provide truly transformative solutions to societal challenges with the scientists' strengths in fundamental research; curiosity, originality and creativity.

Among others, Louis Pasteur has convincingly shown that research aimed at solving important societal problems can be transformative for the scientific field in question at an outstanding quality level. Recently, Danish politicians, the DNRF and four private Danish funders have joined forces in a program called the Pioneer Centers to fund excellent basic research within fields of key relevance to the future of our globe and if successful, it may be transformative in both terms.

In this booklet we aim to address the journey to transformational discoveries and how the best conditions for this to happen can be offered by the funding bodies, the academic world and the political system.

The central purpose of all research is to create *new knowledge.*
The word transformative is one word among others that are used to emphasize the research's degree of novelty or originality. Breakthrough research, frontline research, and, in a Danish context, excellence are other words that have also been used to delineate the borders of what we know.

The DNRF would like to gain a better understanding of a number of issues surrounding the use of the concept "transformative research."

- Overall it is an open question whether these different terms are equally well suited to bringing us closer to our aim: facilitating the best possible framework for researchers to do the best possible research.
- Is there added value to be gained from introducing the concept "transformative research," and, if so, in which context?
- Is focusing on transformative research a neutral focus across different fields?
- Is scientific work that has become transformative always excellent research?
- What are the optimal conditions for transformative research in different fields?

To bring us closer to a better understanding of these aspects surrounding the use of the concept "transformative research," the DNRF discussed the topic "Optimal working conditions for transformative discoveries" with the foundation's grantees during the fall of 2018 and spring of 2019. Further, we invited three scientists from each main area of research – the health sciences, the humanities, the natural, the social, and the technical sciences – to debate the topic in general and with emphasis on what is specific to their main fields of research.

The DNRF would like to thank all grantees for their insightful and well-prepared talks on the topic at follow-up meetings, and a special thanks to center leaders Anders Nykjær, Ian Hickson, Marja Jäättelä, Lars Boje Mortensen, Mette Birkedal Bruun, Rubina Raja, Carsten Rahbek, Jørgen Ellegaard Andersen, Lone Gram, Claus Thustrup Kreiner, Dorthe Berntsen, Mikael Rask Madsen, Anja Boisen, Jan Ardenkjær-Larsen, and Leif Katsou Oxenløwe for contributing their insights into issues specific to the health sciences, the humanities, the natural, the social, and the technical sciences respectively.

On the following pages you can read excerpts from our conversations with the center leaders who in groups of three generously took the time to give us their view on the topic of transformative research.

First, a little about the background for the concept of transformative research.

Professor Søren-Peter Olesen
CEO of the DNRF

Professor Jens Kehlet Nørskov
Chair of the board of the DNRF

> The DNRF would like to gain a better understanding of a number of issues surrounding the use of the concept “transformative research”.

Background for the concept of **transformative research** in the funding system

Soliciting, identifying and funding potentially transformative research
Interest in the transformative potential of research is not new. In 2004, the National Science Board (NSB) established a task force on transformative research in order to gain a better understanding of the National Science Foundation's (NSF) policies for soliciting, identifying and funding potentially transformative research.

The NSB defines transformative research as follows: "Transformative research involves ideas, discoveries, or tools that radically change our understanding of an important existing scientific or engineering concept or educational practice or leads to the creation of a new paradigm or field of science, engineering, or education. Such research challenges current understanding or provides pathways to new frontiers." (National Science Board: *Enhancing Support of Transformative Research at the National Science Foundation).*

The recommendation to the NSF that concluded the work of the task force was that the NSF develop a distinct, NSF-wide Transformative Research Initiative distinguishable by its potential impact on prevailing paradigms and by the potential to create new fields of science, develop new technologies, and open new frontiers.

As mentioned, other funding agencies have since joined the pursuit of transformative research.

Challenges surrounding the concept of transformative research
At first glance it sounds relatively simple. And from the many examples of how transformative research improves lives, boosts nations' competitive edge, and helps solve great societal challenges, it is logical to pursue more direct ways to solicit, identify and fund potentially transformative research.

The NSB itself pointed to the primary challenge: establishing an *operational* definition of transformative research is complicated by the fact that most examples of transformative research are identified as such only long after the work has been completed.

The researchers we have talked to also addressed this problem. To them, it is clear that research becomes transformative due to an unpredictable causal chain of events. Therefore, it is hard to predict what will become transformative.

The NSB's answer to the challenge was: "Although defining such breakthroughs a priori is difficult, attempts to do so are not in vain because history unequivocally records the essential benefits to mankind." The researchers we have talked to would be skeptical to this approach. To them, predicting at the proposal stage what will become transformative, in the sense of *radically* changing, is creating hypotheses about future causal chains of events.

Focusing on creating the right framework for truly excellent research is much more likely to yield the discoveries that, over time, can be of essential benefit to mankind.

Transformative research in different fields
In its report, the NSB notes that the lack of a clear definition has not stopped different NSF programs from using the term "transformative research." Interestingly, the NSB found that the Directorate for Computer and Information and the Directorate for Engineering was the most likely to use the term, and the Directorate for Biological Sciences and the Directorate for the Mathematical and Physical Sciences were much less likely to use it.

This indicates that there are differences in the various research fields in relation to the term "transformative research."

Still, the NSB concluded that it is "imperative for the Foundation to establish a single, uniform definition of transformative research to highlight its uniqueness and to alert the community that the Foundation invites and supports such research."

The DNRF thought that it would be interesting to investigate a bit further whether the concept transformative research is equally relevant across different fields.

Scientific progress
The uniqueness of transformative research is closely connected to the challenges it presents to prevailing scientific orthodoxies. In the NSB's report, this is linked to a conception of scientific progress as a process of often intertwined incremental and transformative steps.

The introduction to the report, Enhancing Support of Transformative Research at the National Science Foundation, states, "Science progresses in two fundamental and equally valuable ways." The vast majority progresses incrementally, and that progress is described by the NSB as "innovative" in that it extends or shifts paradigms over time. Much less frequently, science progresses in a transformative way, "because it 'transforms' science by overthrowing entrenched paradigms and generating new ones."

Although the NSB clearly states that the incremental and the transformative, often proceed hand-in-hand and overlap, it also states that the latter — transformative research — is the focus of the report, thereby making a distinction anyhow, which is in line with the ambition of gaining the knowledge that will enable the NSF to facilitate a more direct route to transformative discoveries.

Whether the NSB's distinction between incremental and transformative is the origin of the perception is not clear, but "incremental" today is not perceived to be as equally valuable as "transformative." We see this, for example, in peer reviews.

When we talked to our researchers, it was clear that they perceive that the distinction implies a ranking and a tendency to go only for the tip of the iceberg, which, to them, attests to a somewhat sensationalistic approach to research, rather than a deep understanding of how research actually takes place.

Researchers on the concept of transformative research
"For me as a researcher, it is hard to relate to the concept of transformative research. I wish we could chuck it from the funding system. It is impossible to predict what will become transformative at the time of the grant decision."

The quote above expresses the general opinion of the researchers we have talked to in connection with our investigation of the concept of transformative research. To researchers, transformative research is first and foremost a concept that can be

used to describe what *has* happened. As such, the concept is not very appealing to this group of people, who are generally driven by curiosity for *new* discoveries.

Accordingly, they prefer terms such as frontline research or breakthrough research. Further, they believe that the chances that research will yield transformative discoveries are better if we focus on creating the best possible conditions in all aspects of the ecosystem that make up research, from education, to funding, to university leadership, and everything in between.

Transformative research:
An implied ranking

Because of its evident impact, transformative research – that is research that "overthrows entrenched paradigms and generates new ones" tends to be described as inherently more valuable than research that has a less immediately obvious impact.

In its report, the NSB, emphasized that transformative research, defined as research that "overthrows entrenched paradigms and generates new ones," is a very rare phenomenon. The NSB also underlined the fact that the vast majority of scientific understanding advances incrementally, and that the incremental steps fuel transformative research. However, the focus on transformative research, which today is widespread in the funding community and in the political rhetoric, implies a ranking that favors the transformative, and perhaps forgets the reciprocal relationship between incremental and more radical steps toward new territory.

Transformative research:
How, what and why?

The way *how* science progresses differs from one discipline or field to another. If there is a widespread focus on transformative research, there is a tendency to look for a "switch-button", but maybe these types of progress do not exist in certain fields, or maybe in these fields an incremental breakthrough is as outstanding as it can get. If that is the case, then our researchers asked: what are we missing out on if we focus on transformative research? They believe that limiting our understanding of how science progresses to an interplay or a distinction between incremental and transformative can give rise to some unintended consequences.

What transformative discoveries are, can be many things on many levels, and there are different characteristics across fields. The following pages contain examples of transformative research or transformative discoveries from all of the five main areas of research: the health sciences, the humanities, the natural, the social, and the technical sciences. To mention just a few: The development of CRISPR/Cas9, the discovery of the Dead Sea Scrolls; determining that the cause of ulcers is the bacterium *Helicobacter pylori,* establishing the diagnosis and code types in the Diagnostic and Statistical Manual of Mental Disorders (DSM); and the invention of the optical amplifier. *Why* we want to pursue transformative research is quite obvious: it's not really a choice.

Forty years ago, we could not imagine the impact of the internet. Our culture, the use of technology, commerce, energy consumption, social networking, generation of data, etc., have changed in ways no one could have predicted. Changes of the same magnitude will probably happen over the next 40 years. Most likely, our planet will have changed in unforeseen ways too.

We are facing major societal challenges, and research-driven solutions are paramount to achieving long-term sustainability.

Research is a key to a successful transition to a future that, in some ways, is predictable and in other ways not. The internet was born from research, and in this way, research shapes or transforms our future. However, research is also sometimes shaped or *transformed* by societal movements or by politics. Are both considered transformative research? And is transformative research always excellent research?

Transformative research in **the health sciences**

... a conversation with center leaders Anders Nykjær, Ian Hickson and Marja Jäättelä.

Anders Nykjær

Center for Proteins in Memory (PROMEMO)

Ian Hickson

Center for Chromosome Stability (CSS)

Marja Jäättelä

Center for Autophagy, Recycling and Disease (CARD)

Among the five groups we talked to about transformative research, this group, Nykjær, Hickson and Jäättelä was the most skeptical.

They were in line with the other groups in their main reason for being reluctant about it, namely, that transformative discoveries can't be predicted. Further, to them, the concept, whether in the NSB's definition of transformative research or otherwise, is just not clear enough; it can mean too many different things.

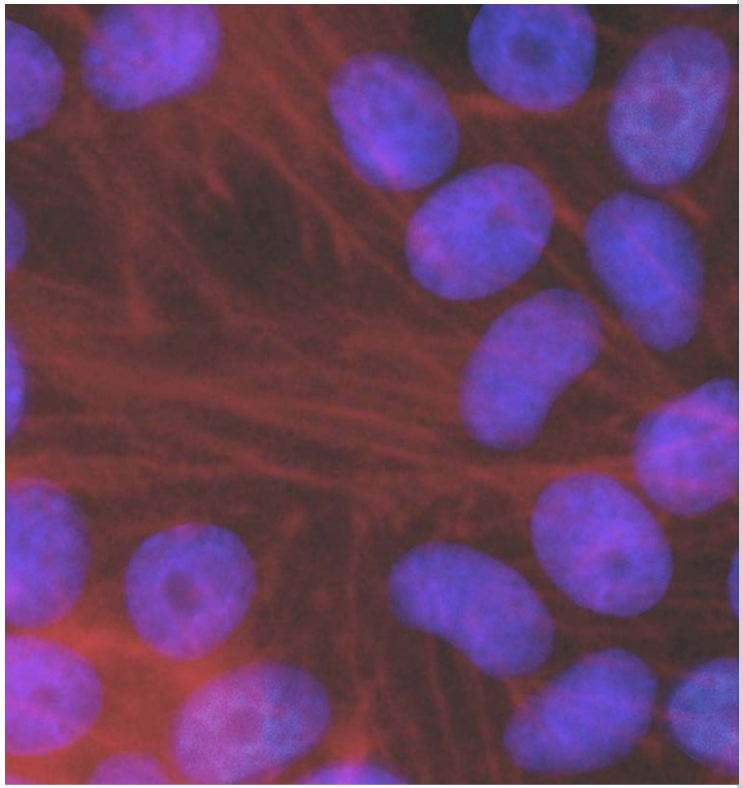
The distinction between incremental and transformative steps only added to blurring the understanding of how the health sciences progress. If we want to create the conditions for transformative discoveries, the group found that it would be much more fruitful to focus on the elements that really fuel originality and breakthroughs in the health sciences.

"In today's health sciences, progress in this field will always be incremental. Even the largest inventions have been incremental. In this way, maybe it is different than, e.g., in astronomy where you can discover water on some planet and suddenly it is conceptually understood that there may be life here, and everything has changed, but this is not how it works in in the biomedical sciences, because the complexity is very large."

In the health or fundamental biological sciences, infrastructure is extremely important. Sometimes in this science a hundred people are needed just to do the groundwork, keeping the technology at the top. Going straight for reaping something transformative would be ignoring everything but the tip of the iceberg, when it is really everything below the water's surface that gives us the part of the iceberg we can see. Those things are infrastructure, the maintenance of it, the right educational system, and the best, most daring, hard-working and creative people. These are the key elements we need to have a well-oiled base for the Danish biomedical sciences to be globally competitive, and maybe under that umbrella, transformative.

Having a concept such as transformative research can lead to neglecting the importance of doing the incremental steps. Too much focus on high-risk/high-gain projects could easily prove to be too much of a high-risk strategy, not only by way of not getting any results, but it could also make Danish biomedical groups less attractive for top researchers from abroad to collaborate with, visit, or join.

As an example of biomedical research that has become transformative in ways that wasn't predicted or even envisioned from what, in hindsight, can be seen as the beginning, the group pointed to CRISPR (Clustered Regularly Interspaced Short Palindromic Repeats). CRISPR is one of the most important present-day findings. Its development has taken place over 13 years, and according to the group: "CRISPR was at that time not the revolutionary finding that it turned out to be years later."



CRISPR/Cas9: **The genetic Swiss army knife**

The revolution of cloning and DNA manipulation has transformed how biological science happens, and it has led to several step changes in genetic technology. The most recent is the bio-technological tool CRISPR/Cas9 – a technology that represents the work of many scientists from around the world.

CRISPR/Cas9 has been named a transformative discovery of the beginning of the 21st century because it enables us to edit genes with enormous precision and in a much more efficient way than before.

The breakthrough came with the identification of a protein (Cas) in the bacteria *Streptococcus thermophilus*. The protein is controlled by a piece of RNA called guide RNA (gRNA), which is what gives the protein its ability to identify and cut DNA sequences in precisely "the right place." The great potential with Cas9 is that the protein can easily be reprogrammed to target other DNA sequences by replacing the gRNA that controls it, hence, the name *the genetic Swiss army knife*.

Before this discovery, researchers had to design whole proteins from scratch if they wanted to edit genes in this way. That process was both slow and expensive.

The incremental beginning of it all

The hypothesis that CRISPR is an adaptive immune system was brought forward in the 1990s by 28-year-old microbiologist Francisco Mojica, who studied DNA fragments of the microbe called *Haloferax mediterranei*. In these, Mojica found multiple copies of a near-perfect, roughly palindromic, repeated sequence of 30 bases, separated by spacers of roughly 36 bases, which was a curious structure that did not resemble any family of repeats known in microbes. By 2000, harvesting critical insight from bioinformatics, Mojica had found similar structures in 20 different microbes, and several scientists now wondered what the function of the CRISPR system was.

The incremental work continued. Mojica turned his focus from the repeats themselves to the spacers that separated them to search for similarity with any other known DNA sequence. Using DNA databases, he found that one of the spacers matched the sequence of a P1 phage that infected many E. coli strains. However, the particular strain carrying the spacer was known to be resistant to P1 infection. Slogging through another 4500 spacers, Mojica finally found grounds for believing that CRISPR encodes the instructions for an adaptive immune system that protected microbes against specific infections.

Then, in 2005, when Alexander Bolotin discovered that the Cas9 protein is an active component of the bacterial immune system, the path to CRISPR-Cas9's transformative potential was laid out.

Representation og CRISPR as the genetic Swiss army knife.
Credit: Pablo Alcón / University of Copenhagen

The basis for transformative discoveries in the biomedical sciences: Education and fruitful interdisciplinary collaboration

When the group highlighted CRISPR as an example of one of the most important present-day findings, they highlighted that it very much started out as incremental work, and that nobody had any idea as to how that could be translated in a way that would be beneficial for humans. CRISPR, in that sense, was for certain not a revolutionary finding to begin with.

"It's the individuals concerned that make the differences. The people who work on it and realize, aha, there is something more that we can do with this, and then they develop it. You need to have a receptive mind. There may have been a lot of people over the years who have been working on CRISPR-related things and never imagined that it could be used for editing human genomes, but some other people then suddenly could see that. I don't think they have breakthroughs; I think they have realizations of things that could then be a breakthrough or have an application. You can't plan to be transformative. You will realize it, if you were. No one planned CRISPR in the way, oh, I'm going to take advantage of this bacterial editing system to cure human disease."

The group could, however, see a relevance for the concept of transformative research if it is used in the context of our educational system. As they said, you need to have a receptive mind.

Therefore, we should be educated to think transformatively, to dare to go beyond the known. Problem solving should be a continuous way of carrying out education. The group all had experience with students who get results that don't fit the textbook and they react by throwing it away because they think they failed. Instead, we want them to keep repeating and discussing it with the supervisors and explore further. We want to educate the young people to be curious and openminded and to dare to go with their data or observations.

One element in creating a framework whereby transformative discoveries can take place is to teach people to analyze objectively and not only to pursue it in terms of the model that we already have. And if students get negative results, we should encourage them to share them with their peers and discuss them. Give students more freedom to go with data and make mistakes along the way. To the health sciences group, those are essential elements in fertilizing the grounds for transformative discoveries.

The Danish education system should be tailored as much as possible to getting people to think independently and think creatively, encouraging skepticism. Because that allows you to go into a whole series of walks of life and be successful – and who knows, maybe make transformative discoveries.

Optogenetics: **Receptive minds and interdisciplinarity at work**

Optogenetics is a technique that uses a combination of light and genetic engineering as a means to, for example, control the cells of the brain.

It all began some 18 years ago with scientists who studied algae. They discovered that the algae *Chlamydomonas Reinhardtii* contain a light-sensitive protein called channelrhodopsin, which triggers the algae to swim toward light, which the algae need to photosynthezise and survive. Other scientists then cloned the light-sensitive proteins and expressed them into synapses. Suddenly, they could control these by using light, and they could change the activity of these cells with the ion influx.

The steps from the algae to using the light-sensitive protein as an on/off switch with which to control the cells was taken yet another group of scientists experimented in using the technique to modulate function by putting the protein into a fly. Then when they put light on the fly, they could make it fly. Then the next group succeeded in putting the protein in a neuron, paving the way for the possibility of looking at genetically specified populations of neurons by which neuroscientists can elucidate the characteristics of normal and abnormal neural circuitry, which again can possibly offer new approaches to the treatment of brain disorders.

In 2013, the Lundbeck Foundation's Brain Prize was awarded to a total of six scientists with different scientific backgrounds for their invention and refinement of optogenetics.

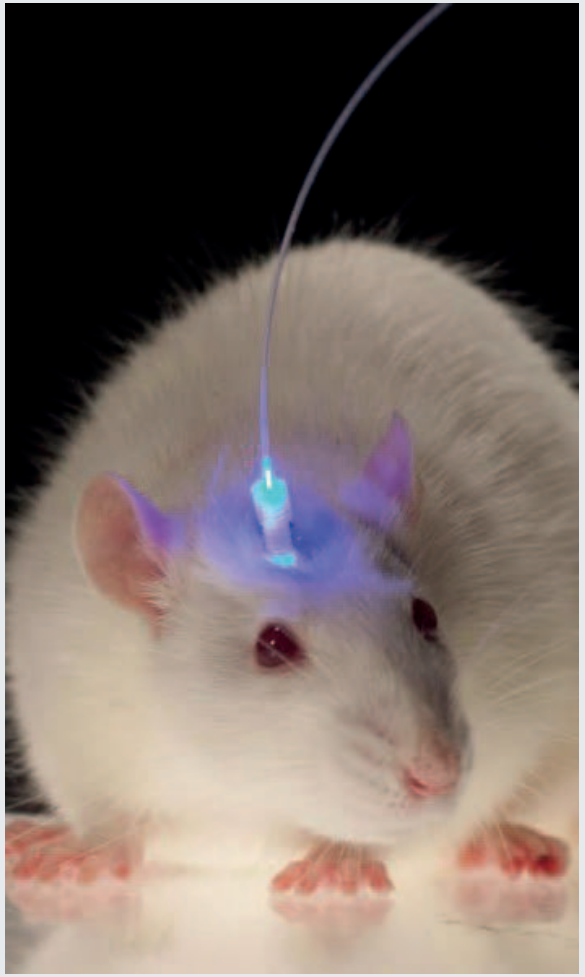
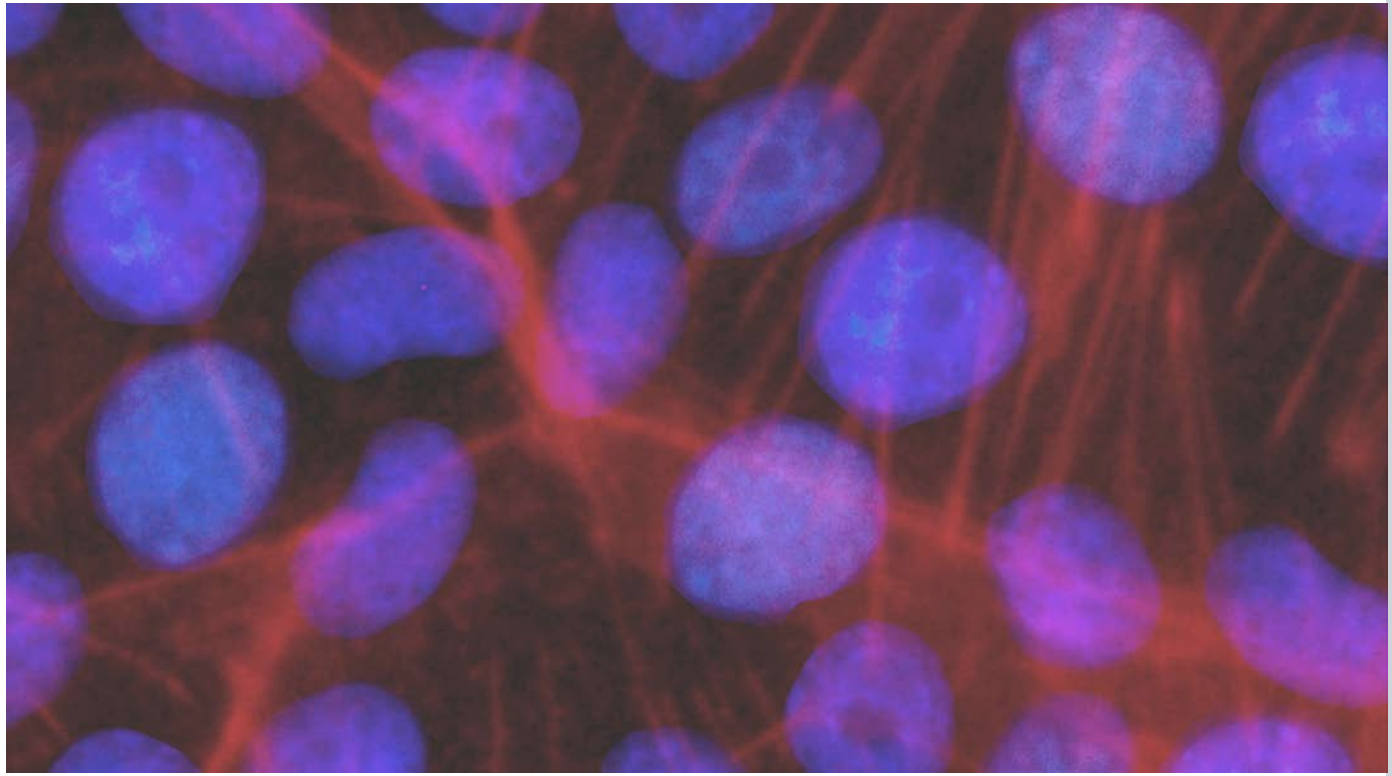


Photo: John P. Carnett/Popular Science/Getty Images

To the group, the example of optogenetics, described on the previous page, reflects the importance of receptive minds and interdisciplinary incremental work. The transformative impact hardly ever comes from one observation; rather, it's a series of observations that in the end yields the breakthrough. Optogenetics starts with the algae, understanding the structure, then taking a synapse, then a fly, and a neuron. Then you take an animal, and you use that to get an integrated understanding of the neuron system, and then you hope to explain something in humans.

The transformative breakthrough involved cell biologists, molecular biologists, fiber optics people, who all got together based on curiosity, scientific overlap, and interests – and the exchange of post-docs. Meetings are tremendously important, but meetings are rarely with very different people; you meet with people who are already within your scope. If we want to stimulate transformative discoveries, we should encourage meetings between scientists from different fields.

That is also an element of education. It would be of great benefit if we encourage students during their general education to keep opening their minds to how many different ways



there are of thinking. Fruitful interdisciplinary collaborations don't suddenly sparkle. Creating sparks won't occur from meeting a nuclear physicist if you don't understand anything about that field of science. Students have to have been preparing themselves, and that's what maturing as a scientist means. The transformative edge lies in this process of maturing, keeping in mind, though, that you can't predict it.

The group concluded that in the end it depends on a general curiosity of putting things together.

Optimal conditions for transformative research in **the health sciences**

In the conversation with the three center leaders from the health sciences, a number of conditions for transformative research were discussed. We have chosen to highlight their thoughts on how the political level could help stimulate transformative research, and their suggestions directed at the funding system and the universities, respectively.

Policy change that would have an effect in stimulating transformative research
As mentioned, the group was not enthusiastic about the concept of transformative research. However, they did feel that it is possible to put in place policy change that would have an effect on stimulating research in that direction.

This should begin with a thoroughly thought-out strategy for how we should finance and fund research. How research is financed and funded has a tremendous effect on how people do research, and it would be possible to point to a few things in today's system that are counterproductive to stimulating transformative research.

At the political level, politicians and policy makers can try to lead scientists in that way. Not with top-down planning of science – the group clearly emphasized that that never works – but as top-down planning of how science is going to be supported.

For example, the DNRF is in place to support breakthrough research and the group see the CoE instrument as a model for a way to stimulate transformative research.

"The main thing is longer-term funding where you don't have to keep jumping through hoops every year to justify your research choices. Giving a two-year grant to somebody is never going to encourage them to do anything particularly risky or transformative. They are going to do something rather straightforward that gives them a paper."

At the political level, it should be considered how big a proportion of the research budget should go into longer-term funding in a strategy to further transformative research. That could be 15 or 25 %, and then parallel to this, there is a need for seed money for the really interesting but not yet mature projects. Not all funding should be given in big lumps.

The funding system and the way science is organized today puts people under pressure. If you have to attract funding to pay your own salary, that just does not stimulate very ambitious, more risky, or transformative research in the group's view.

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Suggestions aimed at the funding system:

- Grants specifically for interdisciplinary collaborations could stimulate breakthrough research. The truly fruitful interdisciplinary collaborations don't come overnight. You need endless testing, brainstorming, and failure to finally succeed

- The Wellcome Trust has interdisciplinary committees because it believes that that helps to improve the outcome of the decision-making process. There is more of a tendency to look at the person and consider if she or he is somebody they want to give the grant to, rather than a focus on the project. Also, they interview people, which is a good way to probe how well-consolidated the collaboration is. Some Danish funding agencies could benefit from similar models.

- Funding agencies should be a little less focused on how many papers the researchers have in the top-ranking journals. Often, researchers can use an extra year to get published in the top-ranking journals and often do it because they think that publishing in lower-ranking journals affects their chances of getting grants. And they are probably right. But the very top journals can have a bit of a barrier to publishing the papers that seem to be breaking all the rules.

  The citations confirm this; the median number of citations for papers published in the high-ranking journals is relatively low, whereas the really big papers that have gotten massive numbers of citations many times were published in lower-ranking journals because they had difficulties getting published. Then suddenly two or three years later, it was recognized how important the science was. The Nobel Laureates are often good examples of this. Again, it's a system that is counterproductive.

Suggestions aimed at the universities:

- The best institutions in terms of research breakthrough and innovation are the ones where there is a mixture of people working in the same place. People who work on something totally unrelated to your own field go to different conferences and they have different colleagues. This will let you get in touch with people you wouldn't get connected to otherwise. Danish universities should work toward establishing these types of institutions that have top researchers affiliated with them. Interdisciplinarity and multidisciplinarity are crucial in terms of breakthroughs, because it changes people's thinking.

- DNRF center leader retreats work well for establishing such networks. In the universities, the PIs of large grants could take a more inviting approach to the surrounding research environments, and invite other researchers to visit the centers. Now, researchers from outside centers often use the center's infrastructure, but they don't use the center staff in the same way. It's much easier to say: "Can I borrow your microscope" than to say "Can I sit down with you for half an hour and run this idea by you."

- Joint educations across universities could also facilitate meetings between researchers across Danish universities.

- It's massively wasteful to be duplicating expensive equipment. National core facilities, especially in the natural and health sciences where the equipment, labs, are very expensive, would be of benefit to Denmark as a whole. Also, it is easier to recruit top people to places with a high concentration of top facilities and top people.

- Big interdisciplinary Ph.D. schools could be an opportunity to facilitate diversity. It would be very productive for Ph.D.s to learn to defend their data or discoveries to a person from a different field, and exchange experiences and ideas.

Transformative research in **the humanities**

... a conversation with center leaders Lars Boje Mortensen, Mette Birkedal Bruun and Rubina Raja.

Lars Boje Mortensen

Centre for Medieval Literature (CML)

Mette Birkedal Bruun

Center for Privacy Studies (PRIVACY)

Rubina Raja

Center for Urban Network Evolutions (UrbNet)

Transformative impact specific to the humanities

The humanities are not one thing – as the plural form indicates. In Danish, the word is articulated in the singular, and whether that plays a part we don't know. But the field is often understood in the singular, as, for example, when an anthropology professor is asked to represent the humanities (humaniora) at a conference.

Across the different fields in the humanities, there is further a difference between the classical humanities and the more application-oriented, though such a distinction can be hard to draw, too.

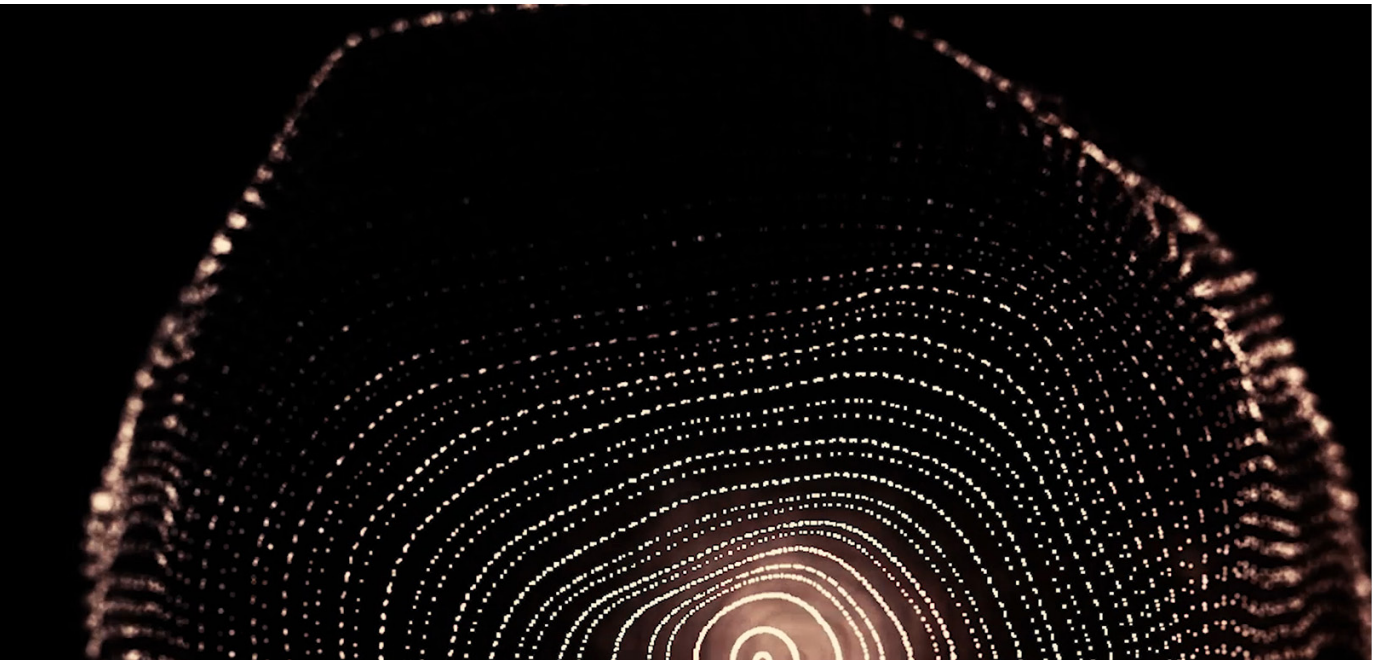
This is relevant to the issue of transformative research because the differences are widely reflected in the way research is carried out, which again creates differences in what constitutes a transformative impact and how the path toward it develops.

Bruun, Mortensen and Raja represent the classical humanities, and the examples of transformative research we will touch upon here are also born out of the classical humanities.

Almost always, transformative research in the classical humanities is characterized by "the long haul" in one way or the other. It evolves around the generation of new typologies, new categorizations, new chronologies etc., and at the macrolevel, the transformative impact specific to the classical humanities lies in the power of conceptualizations.

As we saw from the National Science board's work, the NSB found that the Directorate for Computer and Information and the Directorate for Engineering were the most likely to use the term 'transformative research', and the Directorates for Biological Sciences and Mathematical and Physical Sciences were much less likely to use it. The report didn't mention the humanities, but a guess informed by this conversation is, that a Directorate for the Arts or Humanities probably would not use the term at all.

That does not mean that the humanities don't produce transformative research. You could even argue that conceptualizations are "switch-buttons" for momentous transformations. They are just not as tactile as, for example, optical fibers, MRI scanners, or the laser.



Concepts are both the humanities' object of study and its result. This is one element of "the long haul." Aristotle's concepts have been thoroughly contemplated in the humanities for more than 2000 years, and many of his concepts are a natural part of our language today – even if we do not know that they originate from Aristotle's work. This is an example of how humanistic research leaks out and sinks in to society, and transforms our language and the way we understand the world and our part in it.

The works of the French philosopher Michel Foucault – his books, papers, conference talks, etc. – give us an example of a transformative conceptualization that has influenced both the humanities and, to an extent, the public's way of thinking in a transformative way.

Michel Foucault: **Discourse is knowledge and power**

Foucault was born in 1924 and died in 1984. His oeuvre has been transformative in the humanities because of its widespread influence on various disciplines within the humanities such as philosophy, psychoanalysis, the history of science, sociology, comparative literature, and history. His influence has spread to society too, mainly in that Foucault's methodology, the discourse analysis, and, connected to this, his concept of power as a structural force has been applied outside the realm of science.

Discourse simply means communication in speech or writing. An element in Foucault's discourse analysis was to make a distinction between what is said, that is, which ideas are expressed, and how those ideas are put into practice, meaning how certain ideas are laid out as true across a range of statements.

An example could be politicians' debate about homosexuals' right to adoption. In such a debate there will be a range of statements, and across this range of statements, a pattern can emerge, for example, a pattern that reveals a commonly accepted view of what constitutes a family. This "commonly accepted view" is expressed as knowledge or truth about the concept of family. A common understanding of family could be that of the title of a very common children's game: "dad, mom and kids." What interests Foucault in these patterns is how one concept excludes other types of – in this case – family structures, and how this dynamic function acts as a force of power that structures how we define and organize both ourselves and our social world.

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Foucault called this structural force of power a dispositive. Discourse analysis is about uncovering what determines which statements will be accepted as meaningful or true in a given historical context, and about analyzing the derived structural functions of it.

Thus, in Foucault's oeuvre, discourse is a range of statements that provide a representation of knowledge about any given subject matter. The aim in discourse analysis is – in addition to identifying discourses – to show how the things we view as obvious or natural – what we view as meaningful knowledge about, for example, surveillance, punishment, insanity, family structure, etc. – are the result of a process where certain perceptions have become dominant to an extent whereby they become a structural force in how we define and organize both ourselves and our social world.

Foucault rejected the notion of absolute truth and meaning. To him, discourse instead constructs the topics. Analyzing how discourse installs the authority of being "within the true," will, in Foucault's theory, uncover omnipresent structures of power as a force that is exercised within discourses in the ways in which they constitute and govern individual subjects.

Since Foucault, we have had an intellectual tool which has been a "switch-button" in the sense that we can no longer overlook the existence of certain power structures.

This has had a spillover effect to the curriculum in high schools, for example. When high school students are introduced to discourse analysis as a way of analyzing political or public debate, for example, about homosexuals' right to adoption, a finding could be that political rhetoric about this topic was dominated by a heteronormative discourse at one point in history, which would open the students' view of the topic from just for-or-against to a more broad and critical understanding of the background for how viewpoints are constructed, and what the implications of this is in terms of how a society legislates, how people organize their lives, debate, etc. – thereby ultimately contributing to educating students to become critical citizens.

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The long haul toward transformative results is often characterized by cumulative steps

Bruun, Mortensen and Raja pointed out that scientists generally do not experience their findings as transformative while they are working on producing and substantiating them. This is connected to the specific feature of humanistic science: that concepts are both the humanities' object of study and its result.

Foucault was working with concepts that go back to ancient philosophy, and his transformative contribution, his renewal of these concepts, is an intellectual tool created by his unique way of thinking. This "tool," the discourse analysis, emerged from Foucault's oeuvre, which is an accumulation of two millennia of philosophical ideas entwined with the numerous observations he made on discourses about prisons, insanity, sexuality, hospitals, and so forth. The cumulative process then continued for example in psychoanalyst Jacques Lacan's and other scientists' reception of Foucault's ideas, in changes to curriculum in the education system and through this it leaked out into society.

To Bruun, Mortensen and Raja, the word "accumulative" is much better suited to describing scientific progress in the humanities than, for example, "incremental."

A continuous circle of ongoing critical thinking and building of intellectual tools – conceptualizations – is one key component of humanistic science.

In principle, nothing is left undisputed. This however creates a degree of inertia in terms of "great long-lasting transformative results" as a built-in characteristic of the humanities. Excellence here is the continuous reflection on and transformations of the concepts that are part of our language and the understanding of our cultures and societies.

Foucault based his conceptualizations on historical observations, but by the standards of the classical historical sciences, Foucault was not a particularly skilled historian. The transformative potential in his research lay in the way of thinking in which he spun his observations.

Classical historical sciences, cultural history, archeology, etc. excel by detailed, well-researched mapping of chronologies, typologies and cataloguing, and what constitutes a transformative impact and how the path toward it develops in these fields of the humanities are different from the case of Foucault. Sometimes it begins solely by a stroke of luck as in the case of the discovery of the Dead Sea Scrolls.

The Dead Sea scrolls **and cumulative transformation of our cultural history**

The Dead Sea scrolls are ancient, mostly Hebrew, manuscripts that were found on the northwestern shore of the Dead Sea over a decade, starting in 1947. The first manuscript was discovered by a shepherd boy, who found it by chance in a cave at Khirbat Qumran, where it had been preserved for nearly 2000 years. A search in caves nearby revealed a total of 15,000 fragments that represent the remains of 800 to 900 original manuscripts. About 100 of them are biblical texts, covering the entire Hebrew Bible except Esther.

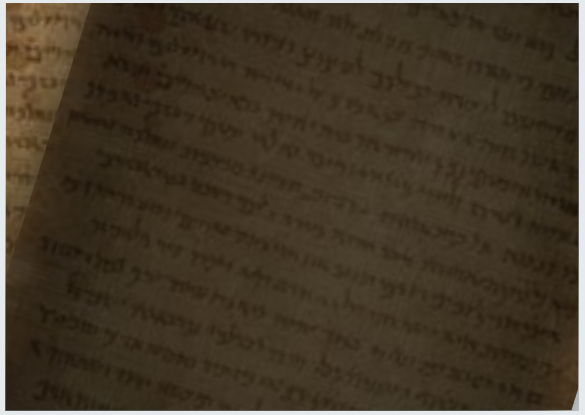
The discovery of the Dead Sea scrolls is among the most important finds in the history of modern archaeology. It has revolutionized textual criticism of the bible and prophecy, providing confidence that the Old Testament we read today is substantially the same as the manuscripts that existed before the birth of Jesus, or in other words, that the prophecy of Christ was written down before the New Testament writers were born.

Study of the scrolls and various dating methods have enabled scholars to push back the date of an authoritative Hebrew Bible to no later than 70 CE, to help reconstruct the history of Palestine from the 4th century BCE to 135 CE, and to cast new light on the emergence of Christianity and of rabbinic Judaism and on the relationship between early Christian and Jewish religious traditions. The scrolls themselves nearly all date from the 3rd to the 1st century BCE. (Source: Encyclopædia Britannica)

Since the first discovery, archeologists have searched through over 300 caves in the area, finding manuscripts in 11 of them. After this came the enormous task of dating and cataloguing the manuscripts, a process that was further complicated and delayed by political factors.

This example tells an entirely different story of what constitutes a transformative impact and how the path toward it develops in the humanities.

"The long haul" in this case refers to the arduous task of dating and cataloguing, which requires researchers involved to be highly skilled in their respective disciplines, meticulous and patient – the transformative impact of, for example, findings depends on many years of what can be called incremental, highly specialized scientific work.



Optimal conditions for transformative research in **the humanities**

In the conversation with the three center leaders from the humanities, a number of conditions for transformative research were discussed. We have chosen to highlight their thoughts on societal relevance, and their suggestions directed at the funding system and the universities, respectively.

Transformative research in the humanities and societal relevance

Bruun, Mortensen, and Raja emphasized the importance of a broad definition of societal relevance. The production of knowledge broadly speaking, conceptualizations, and the reception of and communication about new knowledge – that may transform the public's conceptual understanding of itself – takes a long time. Sometimes the full effect is only visible after decades.

Using the Annales School as an example, Bruun, Mortensen, and Raja stressed the importance of supporting the cumulative steps toward transformative discoveries instead of taking transformative discoveries as a starting point.

The Annales School was named after the journal *Annales: économies, sociétés, civilisations* in which a large number of the papers that constituted this new type of historical science were published. The "school" has been highly transformative in both the historical sciences and in how history books are written. Very briefly, the impact of the Annales School was a shift from studying kings and wars, that is, historical events, to studying social groups and mentalities. It was established in the first half of the 20th century, and as a result, curriculums shifted during the 1970s from studying a sequence of kings to learning about serfs' and fishermen's living conditions.

The Annales School was not planned as transformative research. It became transformative via cumulative steps, a process that is hard to predict, but that, in this case, became highly relevant by its contribution to our understanding of historical processes and shaping of societies. As a general feature to becoming transformative, Bruun, Mortensen, and Raja pointed to interplay between theoretically based humanistic research and its different types of application.

For a long time, societal relevance was understood to be something that had an immediate use in industry or business. But now, for example, with the 17 United Nations sustainable development goals (SDGs), Bruun, Mortensen, and Raja found that a broader view on societal relevance is gaining footing, and the relevance of knowledge about culture, consumption habits, behavior, etc. has gained renewed importance in achieving the 17 SDGs.

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Suggestions aimed at the funding system:

- Funding instruments with a two- to three-year funding horizon are likely to solicit projects that have almost formulated the results beforehand. It could be valuable to rethink these models in a way that would instead stimulate more open-ended proposals.
- Provide seed money as an instrument with a focus on talent development through wild and creative projects for researchers at the level of early to mid-career, and with a funding period of approximately three years.
- Medium-length grants for up to seven years, with 2.5 MDKK per year, and with research groups working on risky projects in a focused way would be a good way to mature humanistic research into larger center constructions that might, over 20-30 years, yield transformative research.
- Funding agencies should perceive interdisciplinarity more broadly than as the humanities working with, for example, the natural sciences. Interdisciplinarity within the humanities is equally important. Collaborations between historical disciplines such as comparative literature, history, architectural history, art history, legal history, intellectual history, church history etc. can yield highly original and interdisciplinary research, even as two archeologists with different profiles can do when working together.

Suggestions aimed at the universities:

- Tenure would increase the quality of research at the universities. We see a lot of good people with bad nerves. We lose an enormous amount of talent when researchers have to spend a large part of their energy chasing funding every two or three years with the stress and bad quality of life this entails. Also, it wastes the universities' and the foundations' money.
- The universities could develop clearer strategies for how they want to apply for funding by analyzing what type of research is a better fit for which types of funding programs, and under this, develop strategies for what research should be financed with the universities' government endowment.
- The humanities are not and cannot always be as internationalized as most other overall research areas because the objects of study are often strongly bound to specific places and specific languages. Basic research within Danish literature is obviously best conducted in Danish and within European studies in one of the main European languages. It is therefore important that the humanities have a strategy that allows for a national, a regional as well as a global level.
- Danish universities could benefit from incentives that would stimulate collaboration across universities.

Transformative research in **the natural sciences**

... a conversation with center leaders Carsten Rahbek, Jørgen Ellegaard Andersen and Lone Gram.

Carsten Rahbek

Center for Macroecology, Evolution and Climate (CMEC)

Jørgen Ellegaard Andersen

Centre for Quantum Geometry of Moduli Spaces (QGM)

Lone Gram

Center for Microbial Secondary Metabolites (CeMiSt)

Transformative impact differs across the natural sciences

Just like the other main fields, the natural sciences are not one thing. In our conversation the group pointed to both transformative research that comes from the field's major theories founded by some of the grand old men of the natural sciences, such as Issac Newton and Albert Einstein, and to discoveries that have transformed how we understand and treat specific diseases.

The natural sciences are both strong monodisciplinary cultures driven by exceptional individuals, as, for example, in mathematics, and large interdisciplinary groups that are dependent on a lot of infrastructure. Both have a transformative potential in different ways.

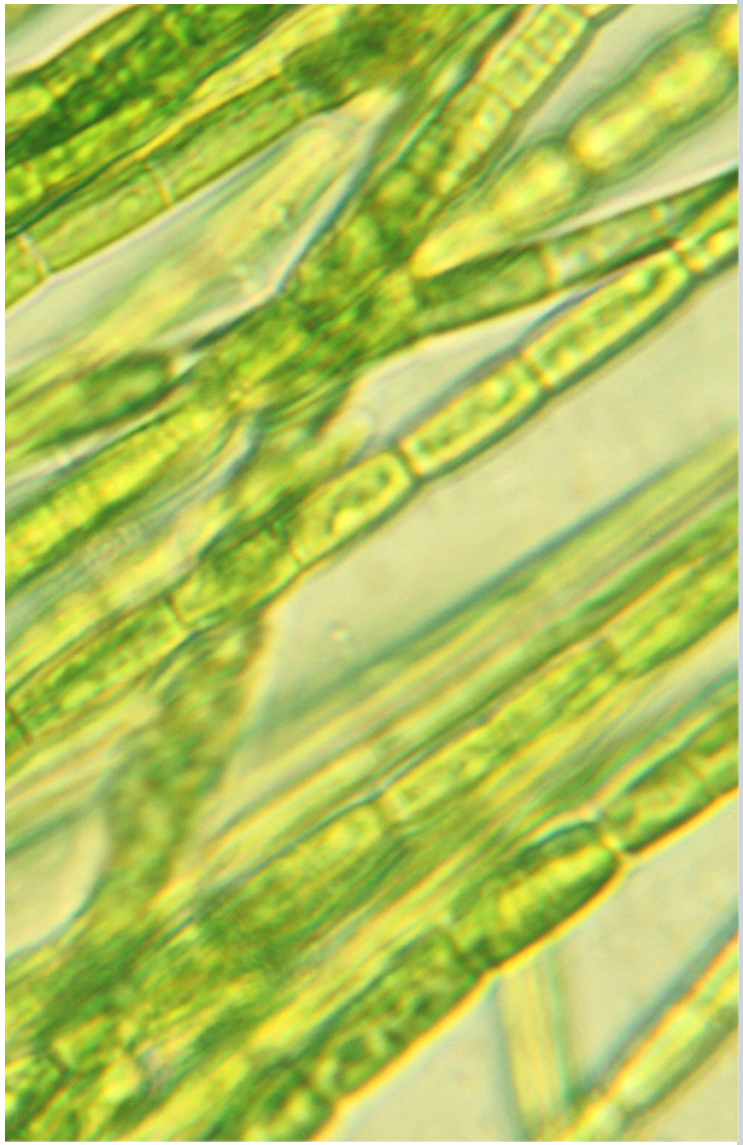
These differences imply that the optimal conditions for transformative research differ across the natural sciences. One type of framework that is essential in one area might be completely irrelevant in other areas. The potentially transformative impact of the different areas' research results can also be very different things, adding to the difficulties in predicting what will become transformative.

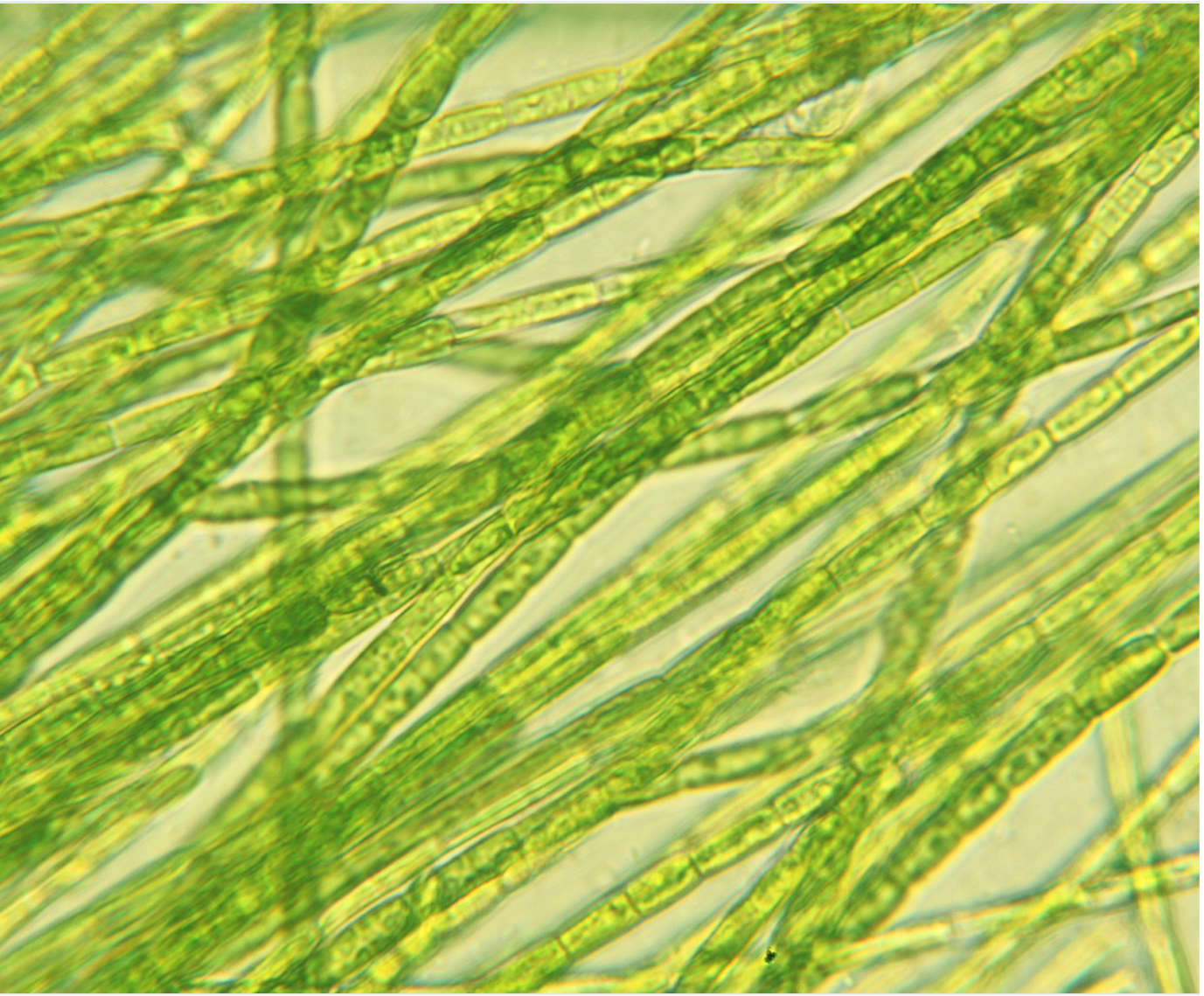
In general, instantly transformative discoveries are very rare. Mostly, transformative discoveries in the natural sciences are a consequence or the result of an enormous amount of incremental research that eventually interconnects into something that can be described as transformative over different lengths of time.

As an example of this, the group mentioned the genome-sequence-protein-analyses revolution that was enabled by methodological advances that made it possible to study a kilo of bacteria from the gut without having to take them out one by one; instead, it is now possible to study the interactions.

From these revolutionary methodological advances, it now looks like that the development of Alzheimer's, different types of cancer, etc., is contingent on the gut's microorganisms and not the body's cells. In 10 years, we will likely look back at this and say: that was transformative. We went from believing that diseases and health are solely based on our cells to also seeing them as contingent on our microbiology.

This will not come from individuals, as we will see in the next example, but from research groups led by George M. Church, Morten Sommer, and Jo Handelsman, groups from China, etc., a conglomerate that draws on a technological development that enables a paradigm shift.





Is that incremental research? Not really. It is large groups in this area that work in parallel on common new discoveries involving maybe 5-6 new technologies and a lot of infrastructure, involving physics, computer science, and bioinformatics – and maybe, in 20 years' time, we will be able to see this as a transformative process. But it is not one project or one researcher who can say: this was my idea! Accumulative research is a better word for the ongoing process.

The highlighted examples on the following pages of this paragraph were chosen because they show variation in what transformative impact can look like within the natural sciences.

Many of the examples of transformative research from the natural sciences, similar to examples from other disciplines, have had a difficult path to eventually becoming transformative, as we will see from the case of Barry Marshall and Robin Warren's struggle with *Helicobacter pylori* and the scientific establishment.

Barry Marshall and Robin Warren: **Do you believe us now?**

For many years peptic ulcers were thought to be caused by stress and increased acid production in the stomach, that is, caused by a bodily reaction. Then in the 1980s, Professor of Clinical Microbiology Barry Marshall and pathologist Robin Warren hypothesized that peptic ulcers were caused by bacteria. In 1982, they began to study ulcer biopsies, and in the microscope, they saw something they believed to be a bacterium. The bacterium was, moreover, only discovered by chance in a sample that the lab technician hadn't had time to throw away on day two as was customary.

Claiming that this gastric disorder was an infectious disease was a major challenge to the prevailing view that peptic ulcer had a physiological basis. In 1983, when Marshall and Warren submitted their findings to the Gastroenterological Society of Australia, the reviewers turned their paper down, rating it in the bottom 10% of those they received that year. Marshall and Warren were ridiculed by the scientific establishment, which just did not believe that any bacteria could live in the acidic environment of the stomach, and Marshall and Warren had no luck in substantiating their finding.

Long story short, Marshall – in a somewhat desperate attempt to prove the theory – ended up ingesting a cup containing *helicobacter*, as the bacterium is called, whereby he developed peptic ulcer, which he treated with antibiotics.

In 2005, Marshall and Warren were awarded the Nobel Prize in Physiology or Medicine for their discovery of the bacterium *Helicobacter pylori* and its role in gastritis and peptic ulcer disease.

Illustration of the presence of the bacteria Helicobacter pylori in the human stomach.

Transformative research and fundamental science: The risk of bias

In the group's opinion it is more or less impossible to target transformative research from a funding perspective. This is of course dependent on the definition of transformative research. In saying this, the group referred back to a research director from one of Denmark's large research-driven companies who in a talk had said that 85% of the company's research was supposed to fail. If the number was lower, they were not really at the edge of things, where the potential for high yields often lie.

Would that be an acceptable scenario for the public funding system or for the researchers at universities? Probably not, just as "fail" probably doesn't mean the same in the two realms.

Fundamental science, also known as basic research, is different than corporate research. Executive vice president and chief science officer at Novo Nordisk A/S, Mads Krogsgaard, has illustrated the difference by saying: "We have never made a fundamental discovery." Fundamental discoveries, or simply creating new knowledge, are the raison d'être of basic research, whereas "high yield" in corporate research is more closely connected to increasing competitiveness.

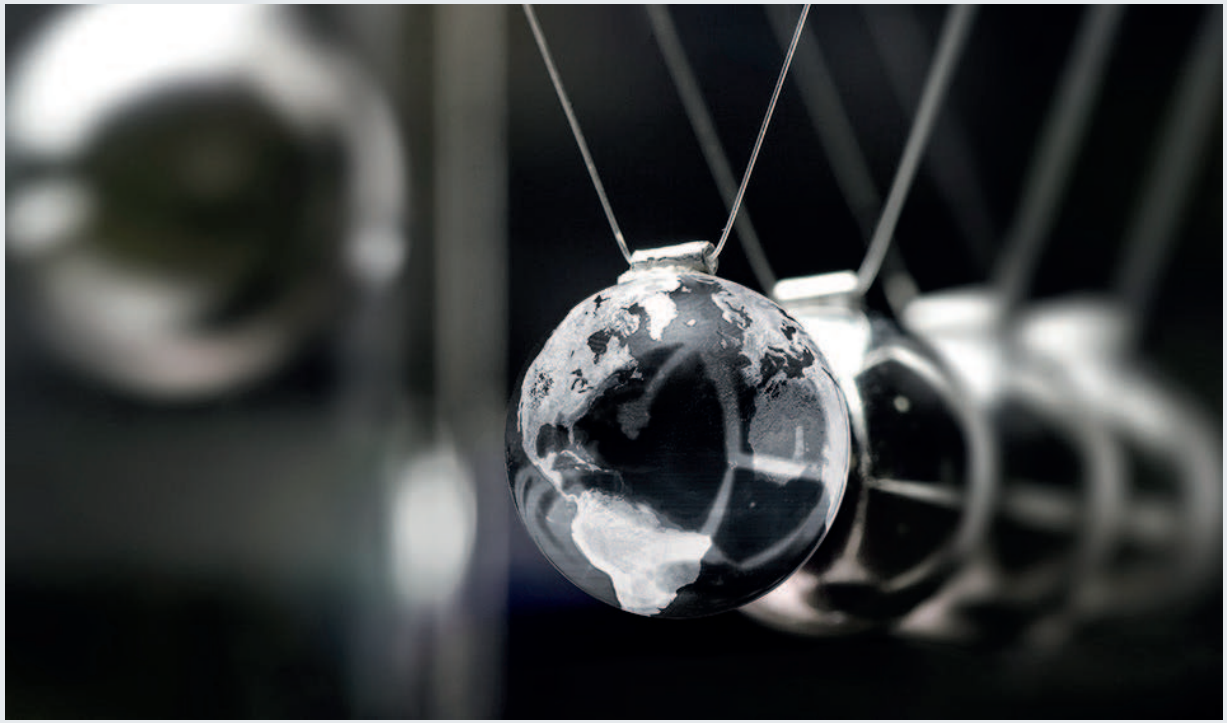
In fundamental research, especially at the proposal stage, the term "transformative research" can be problematic. According to the group, a tendency to become biased toward techniques and methods have emerged in connection with the term in the U.S.

Techniques and methods are also areas where it is easier to say: "If we can develop this type of doohickey, which can do this and that, it will become transformative."

"Then researcher from the purely theoretical branches of science will not receive the same kind of funding as researchers who can produce engineering-doohickeys or chemistry-doohickeys" and write in their proposals that their changes to photosynthesis will enable a 10% increase in productivity. Stuff like that is easy to write and label as transformative. If you do the kind of research that has synthesized thinking as its result, you cannot label it "transformative" as easily, and that can cause a tendency to give lower priority to specific types of research if there is much focus on "potentially transformative research."

As said in the introductory remarks to this booklet, how science progresses is different from one discipline or field to another. If there is a massive focus on transformative research, there is a tendency to look for a "switch-button" and maybe, in fundamental science, the most outstanding progress is the incremental or accumulative research. And if we stimulate this accumulative, fundamental research in the best possible way, it may – over time – lead to transformative breakthroughs, or, as we will see from the next example, lead to enabling something as historic as the Industrial Revolution.

Newton's sledgehammer



When asked about a definition of transformative research, Jørgen Ellegaard Andersen said that he had an intuitive sense of what it is if he thought of examples.

One of his examples was Isaac Newton's *Philosophiæ Naturalis Principia Mathematica* (1687), which introduced three physical laws, the laws of motion, that together laid the foundation for classical mechanics. The basis for Newtonian mechanics was Kepler's laws of planetary motion, which further were based on Kepler's analysis of Tycho Brahe's astronomical observations. Those observations came from very incremental processes of developing the astronomical instruments and measuring and fixing the positions of stars very accurately.

The example with Newton illustrates that the incremental steps – over a long period of years – led to large-scale transformative breakthroughs. "Newton didn't find some small doohickey that could work as some kind of switch-button – he found a sledgehammer that could do almost anything! Take for an example the English Industrial Revolution – one of the truly transformative events in world history – it is built on Newton's mechanics! You see, a couple of hundred years later, Newton's research forms the basis for a huge revolution. Today, Newton could easily have received a big 0 in funding, if the funding agencies were looking for a small doohickey that could give us an edge for the next 10 years. Newton's research didn't have that, but it had some ramifications 100-150 years later that were magnitudes bigger!"

Optimal conditions for transformative research in **the natural sciences:**

In the conversation with the three center leaders from the natural sciences, a number of conditions for transformative research were discussed. We have chosen to highlight their thoughts on strategic academic leadership, and their suggestions directed at the funding system and the universities, respectively.

University leadership should be ensured strategic and flexible room to maneuver

The best ways to stimulate fundamental transformative research in the natural sciences differ from one field to the other. In biology, the ways to stimulate the potential for transformative research is to infuse massive funding to selected wide areas and large interdisciplinary collaborations. Some of it will not lead directly to new discoveries, but it optimizes the likelihood with a strategic focus on wide areas backed with sufficient and long-term funding.

In mathematics it is the individual that counts. Stimulating transformative research in this field is all about brain gain, about getting the best people to come to Denmark and giving them a lifelong funding horizon.

For both ways to work well, university leadership should be ensured strategic and flexible room to maneuver.

In 1982 Marshall and Warren got funding for one year to test out their risky hypothesis. That kind of easily accessible money at the department level would be of extremely high value. Smooth and fast recruitment processes would be another very important factor in stimulating transformative research at the universities.

Managerial decisions must be made on the basis of strategies for ensuring outstanding research. With the restrictions on the universities' economy that result from recent cost reductions, the strategic, flexible room to maneuver at the level of both the departments and the faculties has disappeared to an extent.

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Suggestions aimed at the funding system:

- Parallel to infusing massive funding to strategically chosen wide areas, it is important that the funding system as a whole be geared to catch the talented researchers whose focus is slightly off target compared to the scope of the large, well-funded areas.
- Especially younger researchers who are slightly off target compared to the scope of the large, well-funded areas can have a hard time attracting funding.
- The funding systems as a whole pull in the direction of larger grants. It is important that there be diversity in the funding instruments.
- For "transformative research" as an aim, long-term grants of 10-15 years with market freedom are essential. Lean evaluations every five years would be optimal to ensure impetus.

Suggestions aimed at the universities:

- Prioritize tenure track and start-up packages with full commitment financially and practical support
- Denmark, being a small country, is challenged by elite universities around the world. We could increase Danish research's international competitiveness by increasing focus on connecting Danish researchers' positions of strength in focused collaborations between Danish universities.
- A divide-and-conquer approach between the universities could be relevant so that all universities don't have to cover everything. Maybe an increased focus on joint education programs would be good.

Transformative research in **the social sciences**

...a conversation with center leaders Claus Thustrup Kreiner, Dorthe Berntsen and Mikael Rask Madsen.

Dorthe Berntsen

Center on Autobiographical Memory Research (CON AMORE)

Claus Thustrup Kreiner

Center for Economic Behavior and Inequality (CEBI)

Mikael Rask Madsen

Center for International Courts (iCourts)

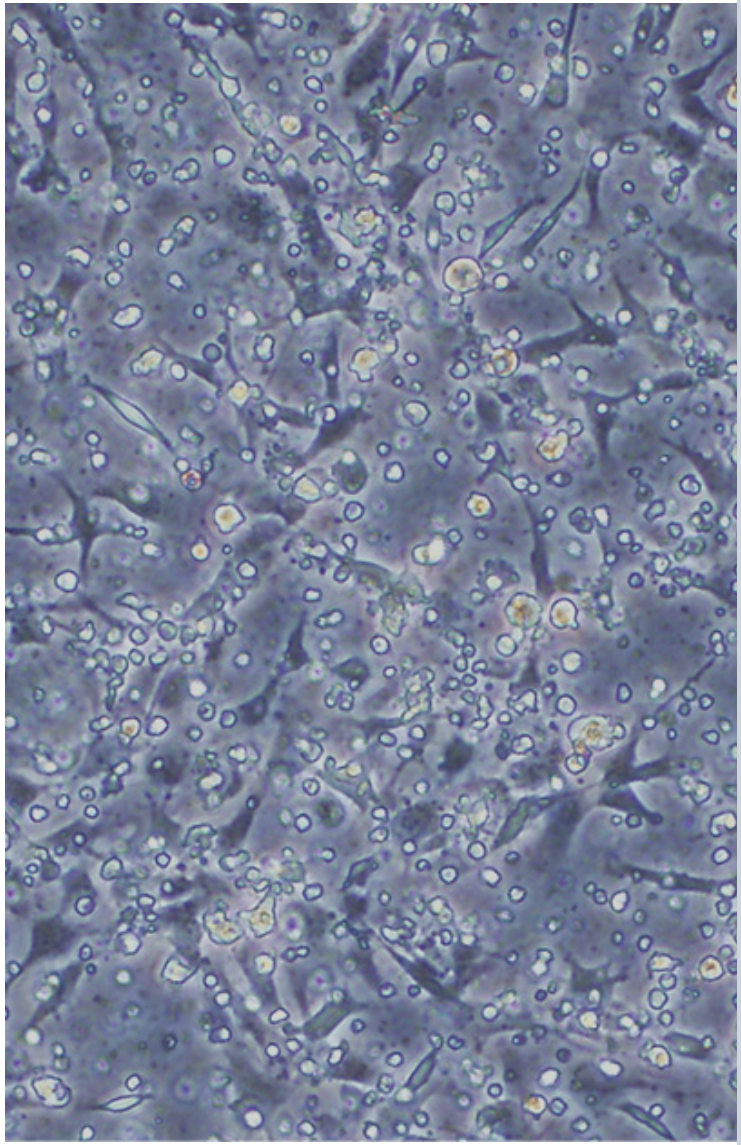
Transformative impact specific to the social sciences

The greatest transformative potential in the social sciences lies on the conceptual level, on impacting people's "Weltanschauen," or worldview or change their behavior. If research has that impact, it can be radically transformative. But that does not come from one scientist's one paper; it arises from one scientist's entire oeuvre *and* the society's reception of it as, for example, with Karl Marx and the concept of the class-divided society, or Sigmund Freud's concept of the unconscious – both concepts that for many years have shaped how we understand society and humans.

This type of transformative research in the social sciences often has complex origins. Marxist theory does not come from something that Marx made up in his head. It comes from historical events, from how people were treated in the 19th century, and how Marx articulated that into a theory that later became one of the most influential political movements of the 20th century, a significant school of thought in the social sciences, and transformative to a degree where it has become a kind of "common property" and a natural part of the language of everybody from hairdressers, to dentists, and physics professors. Personality is also an aspect on the path to a transformative impact: Marx was a skilled agitator, and in particular, he knew how to involve people who were also skilled agitators for his theory.

The time frame for this type of transformative research is, not surprisingly, quite long and definitely unpredictable. In terms of describing how this type of science progresses, the words incremental and revolutionary are not the most relevant. Rather, the impact is the result of a causal chain that depends on, for example, historical conditions with which the scientist's theories or experiments interact in forming new ways of thinking, new models, or new concepts. These new items may be developed for a small selected area, but may then be applied to other areas where they help us understand and structure a whole other area of our economy, the law, human psychology, and so on.

Center leaders Kreiner, Berntsen, and Madsen noted several examples of research that can be described as transformative in the field of social sciences. The three highlighted here – Marx and Freud, George Akerlof, and establishing the DSM diagnosis and code types – were chosen because they show a variation in the path to becoming transformative in the social sciences.



George Akerlof's lemons: **The process of becoming transformative**

George Akerlof's paper, *The Market for Lemons: Quality Uncertainty and the Market Mechanism*, describes the risk of degradation of product quality due to asymmetric information in markets, using as an example the secondhand car market in which the seller is the only one that truly has complete information about the quality of the car.

The paper, for which he was awarded the Nobel Prize in 2001, was rejected three times. The first two times it was rejected on grounds of triviality; the third time the hypothesis was believed to be incorrect. Akerlof himself has stated that:

""Lemons" was much less of a break with the economics of the time than might otherwise be interpreted. It was the natural extension of the on-going intellectual activity at MIT."

As the center leaders described it, when Akerlof, in his first years as an assistant professor at the University of California-Berkeley, wrote "Lemons," he wasn't concerned with transforming economics; his work was "a natural extension of on-going work", and getting the right idea at the right time.

"I happened to be in the right place at the right time, and therefore was extraordinarily lucky to have been able to write the first theoretical paper on [...] how asymmetric information affects markets."

As the history of rejections shows, the paper certainly didn't become transformative overnight. The center leaders have described the process of becoming transformative as an empirical phenomenon. Research, a paper, a discovery, becomes transformative if recipients make it transformative. In Akerlof's case, he describes it as follows:

"Many people were tremendously generous at all stages, in the writing, editing, and refereeing of this paper, and also later, in exploring its further implications – thus illustrating the extraordinary commitment of academics in general, and of economists in particular, to seek truth and to advance knowledge."

To sum up, the birth of Akerlof's prize-winning, transformative paper was a combination of incremental work, luck, and peers' commitment to seeking truth and advancing knowledge.

The full account of "Lemons" leading to Akerlof's Nobel Prize can be read here.

> “ The terminology [transformative research] is potentially dangerous. We prefer ‘excellence’ or frontline research, which refers to the novelty criteria, which is the most relevant criteria in research. If it then becomes transformative? Well, time will tell; that depends on a causal reaction – it cannot be a criterion.

The risks connected to focusing on transformative research
When asked about the ambition to pursue transformative research, one of the center leaders responded that everybody would say that it sounds smart. He then added a bit cautiously: "But that's because they don't quite understand research."

The path to eventually becoming transformative is not a straight line. Shifting a field or paradigms can only be done on the basis of many years of incremental research that builds a solid basis for the breakthrough that eventually and in hindsight can be pinpointed as the event that transformed the field.

If we pursue a more direct way to these transformative "events," there is a great risk that we will see even more replication crises than what we have already seen. We should be careful not to accidentally encourage the production of flashy results in emphasizing the concept of "transformative research."

"The terminology [transformative research] is potentially dangerous. We prefer 'excellence' or frontline research, which refers to the novelty criteria, which is the most relevant criteria in research. If it then becomes transformative? Well, time will tell; that depends on a causal reaction – it cannot be a criterion. In that case, it would be a hypothesis about how a research proposal, or a research idea is going to have this and that causal effect on research, on society or what not; that is, a hypothesis about a hypothesis."

A form of orchestrating things to look transformative can quickly occur with the introduction of the concept of transformative research to the funding system, and this is a real concern. It pushes everybody to be ultra-original all the time, which calls to mind the illusion of the Age of Enlightenment, that is, the idolization of the genius.

If the focus shifts in the direction of front-page news, this presents a risk in relation to research. It is a somewhat populist notion of research whereby researchers can almost feel pressure to be rock stars.

Going in that direction risks not focusing on the elements essential to creating what we are aiming for: excellent research that can potentially transform our lives for the better.

Societal influence that transforms research: **Development of the DSM**

The Diagnostic and Statistical Manual of Mental Disorders (DSM) was first published in 1952 by the American Psychiatric Association (APA) as DSM-I. The manual describes and offers standard criteria for the classification of mental disorders.

Prior to World War II, understanding and treating mental disorders, to a large extent, belonged in the realm of mental hospitals. But around the time of World War II, the need for knowledge about mental functions and disorders that could be useful to the military in the selection, processing, assessment, and treatment of soldiers changed this.

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Psychiatrist and Brigadier General William C. Menninger was part of a committee set up by the United States Secretary of Defense to develop new concepts of mental disturbance. These new concepts were adopted by the armed forces; later, the structure and conceptual framework influenced the DSM-1 to a large extent, whereby the principles that were developed in a military context were further introduced into clinics and hospitals.

However, both DSM I and the ensuing DSM II were criticized for poor reliability. This posed a problem for providing reliable data for psychopharmacological research as well as in relation to insurance-based reimbursement. DSM III was introduced in 1980 as a response to these problems and presented a radical revision of the diagnostic system. The ambition was to create a purely descriptive and evidence-based diagnostic system for mental disorders. This, however, does not mean that the DSM III diagnostic categories were uninfluenced by societal factors. Quite the opposite. The eight members of the task force, consisting of psychiatrists, psychologists, and one statistician, had a great influence on the ensuing classification system and many decisions were consensus-based rather than based on empirical evidence.

One illustration of the impact of societal factors was the introduction of a new and now highly prominent diagnostic category, Posttraumatic Stress Disorder (PTSD). Multiple historical accounts have documented that this happened in response to strong pressure from Vietnam war veterans and their supporters. Before the introduction of the PTSD diagnosis, mental distress caused by traumatic events was viewed as a transient phenomenon that in part depended on the patient's ability to adapt. After DSM-III, posttraumatic stress reactions were viewed as a general stress response that could develop in anyone who encountered a sufficiently powerful environmental stressor. The theoretical arguments for this view in large part stemmed from one highly influential psychiatry professor, Mardi Jon Horowitz. His book, Stress Response Syndromes, published in the mid-seventies, provided a timely theoretical motivation for the PTSD diagnosis. Despite substantial empirically based criticism, the description of the PTSD etiology and its key symptoms are still reflective of his transformative theoretical view.

The DSM has had a massive societal impact, and it has been termed a revolution or transformation in psychiatry. For example, by the National Science Board's definition, it has been truly transformative.

However, since the first edition of the DSM, the accuracy of the categories and even the efficacy of psychiatric diagnosis in general have also been fiercely disputed. The main criticism is that the categories don't always fit when the symptoms of mental disorder are investigated systematically and empirically.

This of course raises the question: Is the transformative impact always the same as the best research?

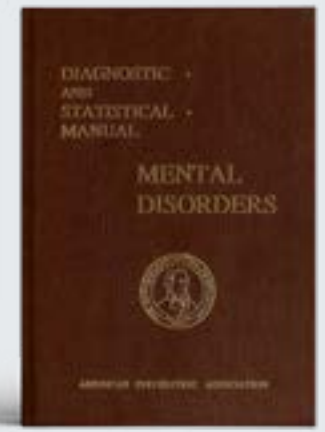


Illustration shows the first edition of the *Diagnostic and Statistical Manual.*

Optimal conditions for transformative research in **the social sciences**

In the conversation with the three center leaders from the social sciences, a number of conditions for transformative research were discussed. We have chosen to highlight their thoughts on mobility and internationalization, and their suggestions directed at the funding system and the universities, respectively.

Mobility and internationalization
If we want optimal conditions for transformative research in the social sciences, the group finds it timely to look into a modernization of how we look upon mobility and internationalization.

"Compared to industry, academia's eagerness to travel is second to none. We have a 20th century conception of how we should internationalize. The conception is rooted in the classical educational journey to the 'unknown,' but today there is nothing unknown about what the groups in Paris or Boston are doing – it's all on their websites! Sometimes you would think that Skype wasn't invented."

For years we have known about the problems related to the mobility requirements for young researchers. It skews the gender balance, and it favors resilience over talent.

It is essential to experience different research environments, but this can happen in many different ways. Shorter stays with well-planned activities in different places or with stays at the same place more times might well prove to have a greater internationalizing effect than moving your permanent address at three-year intervals.

The group also feels that Danish research misses out on a lot of recruiting opportunities. Sometimes, natives can be highly international with shorter visits, conferences, and Skype meetings, and by being part of, for example, a center leader's international network, even though they have "set up their tents" in Denmark with families and kids at school.

Further, the group experience a number of obstacles when recruiting non-Danish employees. Some research environments are characterized by a notable provincialism that manifests in reluctance to having meetings or even emailing in English, and this provincialism further impacts non-Danish people's chances of getting permanent positions in Denmark.

Finally, the mobility requirements also hamper academia's opportunities to compete with the other career opportunities available to the best talents.

Continue on next page ⟶

Suggestions aimed at the funding system:

- The funding system should be infused with a bit more risk-willing and easily-accessible capital. Especially for the young researchers, it takes too long to obtain funding. Often, there is only one application deadline a year. Today, resilience is favored over talent, which is paradoxical when we know that what really pushes research forward are ideas and talent.

- Broadness should be ensured in terms of different funding strategies and instruments. In the social sciences, the breakthroughs often originate from smaller groups.

- In terms of international competitiveness, Denmark, being a small country, cannot do everything. In making strategies for Danish research it is essential to uphold and guard the bottom-up research freedom.

Suggestions aimed at the universities:

- There must be money in the system at different levels. At the department level it is essential to have financial room to maneuver to be able to run with the best ideas or candidates when the chance arises. There should also be financial room to maneuver at the rector's or faculty level, also here keeping in mind that in making strategies it is essential to uphold and guard the bottom-up research freedom.

- Increased collaboration across the Danish universities could also optimize Danish research and education. It's better to have 500 students at very well-prepared lectures; it ensures critical mass and limits "repetition-education and research."

- In the social sciences, the revenue from teaching (STÅ) is relatively smaller compared to, for example, the natural sciences. The implication of this is less time for research for the individual researcher. It is crucial that the revenue from teaching (STÅ) is at a level that is more comparable to the one in natural sciences and health sciences in order to ensure competitive research in the social sciences.

Transformative research in **the technical sciences**

... a conversation with center leaders Anja Boisen, Jan Ardenkjær-Larsen and Leif Katsou Oxenløwe.

Anja Boisen
Center for Intelligent Oral Drug Delivery Using Nano And Micro-fabricated Containers (IDUN)

Jan Ardenkjær-Larsen
Center for Hyperpolarization in Magnetic Resonance (HYPERMAG)

Leif Katsou Oxenløwe
Center for Silicon Photonics for Optical Communications (SPOC)

Transformative impact specific to the technical sciences

The most distinct transformative potential of the technical sciences probably lies in the development of what can broadly be categorized as "'tools." Research results in the form of new tools that shed light on, for example, a whole new physical dimension and enables us to study this dimension. That result can be described as transformative in the sense that we suddenly understand and study nature in a radically different way than we did before the invention of such a tool.

Touching on whether technical science progresses as an interplay of incremental and transformative steps, the group of technical scientists pointed to the work done over time to achieve higher and higher time resolution. Developing the tools that enable measurement of incredibly short timescale could be described as incremental steps, but at the same time, it involves huge new physical principles each time that researchers have succeeded in going down a factor ten in time resolution. In that sense, the steps can also be described as transformative. To the group, this just tells us that with the words incremental and transformative, we run into the same difficulties in describing the progress of the technical sciences as we do when using applied and fundamental. The words rarely fit precisely, and it is usually not a case of either/or.

The development of tools sometimes has the potential for different types of application, but in fundamental or basic technical sciences, the development processes involve and require a deep understanding of the fundamentals of physics, chemistry, and other related scientific disciplines. The impact of such new tools or instruments is that they can potentially transform our understanding of the physical world – which is transformative, in itself – even if there is no immediate application for it.

"You don't make an impact in the technical sciences, if you can't make it so that people can use it." What technical science is about can be described as short as that, as it, in fact, was by the group. Use, or societal use, however, should be understood very broadly, since the transformative potential can apply to different areas.

In the development of the scanning tunneling microscope (STM), we will see an example of a transformative technical achievement that changed the way we look at our physical world, created a whole new field of science, and which was later the basis for a variety of products that have been introduced to the market. As such, it exemplifies the broad "usefulness" or, in some cases, the transformative potential of fundamental technical science.

The invention of the STM: **The elements of a technical paradigm shift that makes it to market product**

"Physicists Gerd Binnig and Heinrich Rohrer were working on superconductors and not at all in the field of surface science, and how they, out of the blue, got the idea to piece together bits and parts in the lab and make the first STM (scanning tunneling microscope) that can detect surfaces at the atomic level.. I just don't know," Anja Boisen said when asked about the driving factors behind a technological paradigm shift. "They had extensive freedom and IMB was a fantastic playground at that time."

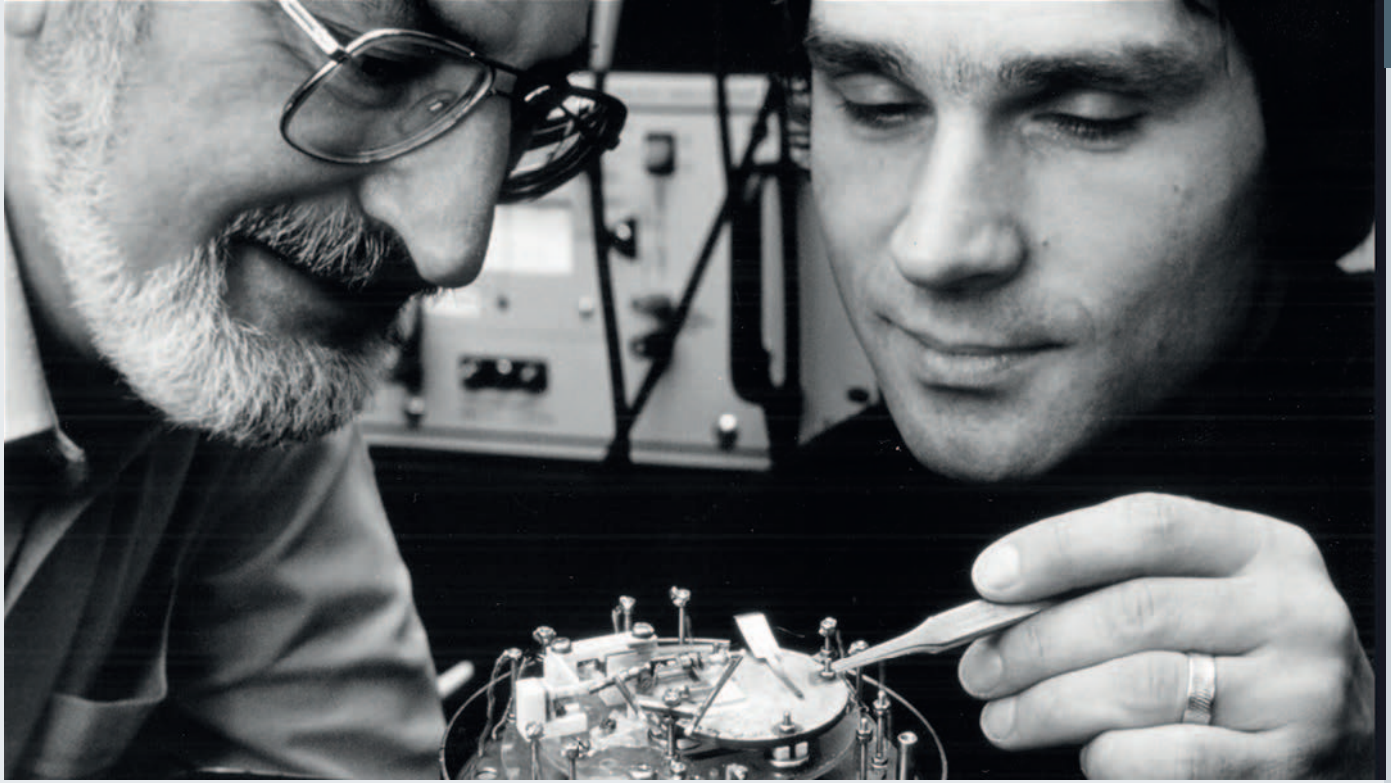
Gerd Binnig and Heinrich Rohrer were awarded the Nobel Prize in Physics in

Continue on next page ⟶

1986. In the press release it was stated that: "The scanning tunneling microscope is completely new, and we have so far seen only the beginning of its development. It is, however, clear that entirely new fields are opening up for the study of the structure of matter. Binnig's and Rohrer's great achievement is that, starting from earlier work and ideas they have succeeded in mastering the enormous experimental difficulties involved in building an instrument of the precision and stability required."

The development of the STM is a classic example of the distinct transformative potential of the technical sciences: the development of "tools" or, as in this case, instruments. The physical principles on which the STM was based were already known before the development of the STM, but Binnig and Rohrer were the first to solve the significant experimental challenges involved in putting it into effect.

Binnig and Rohrer's achievement was the beginning of the entire field surrounding nanotechnology because suddenly it was possible to image atoms and move them around. It has had vast derived effects, and it also resulted in massive funding for the area. On the other hand, nanotechnology stayed in the lab for years before other types of researchers began looking into translation, that is, ways to make technologies cheaper and turn them into market products.



Heinrich Rohrer and Gerd Binnig pose with the core of their scanning tunneling microscope, the underlying technological accomplishment of their 1986 Nobel Prize in Physics, received with electron microscope inventor Ernst Ruska. Courtesy IBM Archives.

That part takes time, in this case around 30 years. Now it is spreading. For instance, there is a Danish company, Heliac, that produces a polymer foil lens that mimics the shape of a traditional Fresnel lens with nanostructures for a fraction of the cost and sells it in Africa as a solar cooker. Twenty years ago, no one imagined that this would be possible.

The elements involved in this are first of all a lot of different people. It begins with the two physicists, who stepped out of their comfort zone and built this instrument. That opened up an entirely new field: nanotechnology.

Then scientists started to look at what happens when they nanostructure materials. The next steps involved in making Heliac's product possible was to use techniques that have, so far, been used in microelectronics, and repurpose them into making these other types of nano-microstructures. Finally, it involves people who know plastics and foil manufacturing.

To get all these different competencies to play together, you need knowledge transfer and someone who can visualize the connection points that make up this entire pipeline.

Potentially transformative technical science and collaboration with industry
There are several examples of collaboration between technical scientists and industry that has led to transformative discoveries. This can play out in many ways, for example, with industry involved from the beginning, or with industry adding the necessary financial muscle at a later point to, for example, achieve a higher level of technology readiness that can lead to transformation, for example, in the form of societal impact.

The development history of the optical fiber is an example of the latter. The optical fiber was developed with a clear application in view, but it was far from obvious that it would be scientifically feasible. The development required the merger of a lot of chemistry, materials physics, and knowledge about glass. This interdisciplinary merger took place at the research center of Standard Telephones and Cables at Standard Telecommunication Laboratories (STL) in Harlow, England, where physicist and electrical engineer Charles Kao developed the theoretical and experimental basis for the optical fiber, for which he was awarded the Nobel Prize in Chemistry in 2009.

A few years later, big companies, such as Corning Glass Works, took over and developed it further, so that in 1972, it was finally possible to produce fibers with a low enough loss of energy to make it feasible to send optical signals through the fiber, which is today the backbone of for the massive transmission of data on the internet.

In this case, industry was paramount to further advancing a research discovery into becoming transformative for society.

In other cases – as we will see from the example of the MRI where the instruments go from using frequency domain to time domain – transformative technical research can cause industries to die more or less overnight.

When does collaboration with industry work?
The group highlighted two sides to having industry on board.

On the one hand it can limit the freedom of researchers to follow and explore fundamental questions that arise if it doesn't look like something that will increase the involved industry's earnings or market shares, or, even worse, if it threatens the industry's existence. In such cases industry has been known to work against changes that could become transformative.

On the other hand, if industry is fully committed to a certain development driven by technical science, then the financial power it will be willing to invest often far exceeds what universities can match. One point of view here would be that there is no need for universities to invest in the research topic, since it will be developed anyhow.

Then there is the middle ground, where the research has large potential, but is a still at a low technology readiness level (TRL), and where industry and academia find it beneficial to collaborate on bringing the research to a higher TRL. For example, the Centre for Silicon Photonics for Optical Communications (SPOC) conducts research with no strings attached to industry, and for the most part, at a low TRL. However, through the large funding schemes of the Innovation Fund Denmark, some of the research topics in SPOC can be brought to higher TRLs in a very intense and dynamic collaboration with many industrial partners.

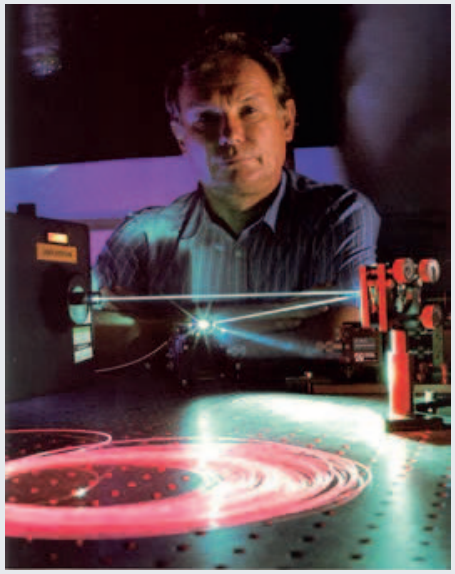
Timing is often essential for a potential to actually become transformative to society. Sometimes it lies in the linkage between an acute need in the market and the birth of a technological paradigm shift that revolutionizes the market, as, for example, with the invention of the optical amplifier.

The optical amplifier

The optical amplifier is an example of transformative technical research that, over a very short period of time, developed into a product that has gained massive application. The scientific development took about five years, and it was only another five years from the first paper in 1987 until the final product was put on the ground and into use.

The invention was truly a technological paradigm shift developed by David N. Payne, the principal investigator (PI) of a group from the University of Southampton and a team at Bell Laboratories led by Emmanuel Desurvire and Randy Giles. Before the optical amplifier, a long optical fiber transmission line required a complicated optical-to-electrical (O-E) and electrical-to-optical (E-O) converter for signal regeneration. For the most part, the industry was satisfied with this and therefore was not pushing a transformation, except for Bell Labs, which was a part of the revolution.

The optical amplifier completely eliminated the need for O-E and E-O conversion, which truly caused a paradigm shift in telecommunications.



Professor of photonics Sir David Payne.
Photo: University of Southampton.

“Research is, by nature, an endeavor where we embark on new territory, and we cannot predict the outcome. Of course, we consider the relevance of our research, the potential impact and societal relevance. In some cases, it could lead to transformative discoveries, but we have no way of knowing.

Nuclear magnetic resonance spectroscopy **(NMR)**

Fundamental science, such as the field of super conductivity and Fourier transformation, forms the basis for today's NMR spectroscopy and medical imaging, but the two companies Bruker and Oxford Instruments were integral in enabling the transformative advances we see in the field today.

In the 1950s and 1960s, the instrument for NMR spectroscopy was made from an electromagnet, a radio transmitter, and a receiver. That type of magnet has a limited field, which, in turn, limited the spectroscopy's degree of detail. Oxford Instruments pioneered the instrument by manufacturing superconducting magnets, which revolutionized the development of nuclear magnetic resonance (NMR) and magnetic resonance imaging (MRI).

Around the same time, Bruker introduced Fourier transformation (FT) into the NMR spectroscopy system, which it then merged with the advances that Oxford Instruments had made. In the 1970s Bruker was the first to commercialize a superconducting FT-NMR.

Industry, in this case, took two genuine breakthroughs of fundamental sciences – Fourier transformation, which refers to the 18th century mathematician Jean-Baptiste Joseph Fourier, and superconductivity – and applied it to the manufacturing of instruments for NMR spectroscopy. This transformed the entire field of medical imaging and NMR spectroscopy.
It has since been followed up continuously with incremental steps toward increasing magnetic field strengths.

Optimal conditions for transformative research in **the technical sciences**

In the conversation with the three center leaders from the technical sciences, a number of conditions for transformative research were discussed. We have chosen to highlight their thoughts on the concept of transformative research in relation to optimal setups, and their suggestions directed at the funding system and the universities, respectively.

The concept of transformative research in relation to optimal setups

"Research is, by nature, an endeavor where we embark on new territory, and we cannot predict the outcome. Of course, we consider the relevance of our research, the potential impact and societal relevance. In some cases, it could lead to transformative discoveries, but we have no way of knowing."

The group from the technical sciences was equally as reluctant to discuss the concept of transformative research as the other groups we spoke to, and they felt that it is important not to impose it as a requirement for all funding instruments or on research in academia in general.

Transformative research, as defined by the NSB as paradigm shifts or steps that radically overthrow something already in existence, will always be rare. If we want to pursue it, the group pointed to set-ups such as Bell Labs in its golden days, to IBM in Zürich in the 1980s, and to departments they were part of in the beginning of their research careers, departments such as the Center for Communications, Optics and Materials (COM), and the Center for Microelectronics at the Technical University of Denmark (DTU). These centers worked as departments, but different from most departments today, there was wide-ranging freedom, common goals, different competencies, extensive collaboration and, importantly, financial room to maneuver because they were established on public grants: in the case of the COM center on a 5+3 year grant from the Public Research Council for Technology and Production Sciences (Statens Teknisk Videnskabelige Forskningsråd).

Common to the set-ups was the agility that comes from having financial room to maneuver, room for pursuing the more wacky ideas, and financial power to act fast to attract the best people from the global research community. Such a framework has a spiraling effect; it attracts ambitious and highly talented people, and is big enough to encompass several people with different expertise. That combination is essential to stimulating originality.

The right people are equally essential. If the aim is to combine the puzzles in fundamentally new ways within these large set-ups, it can be meaningful to have some "creative satellites," certain personality types that can really fuel changes, but who often need a managerial effort in order for them to fit in. These challenges were addressed in 2018 by the DNRF in the booklet *Diversity and Excellence in Recruitment and Career Development*.

IMB in Zürich employed approximately 150 people from all over the world. A company with a size and a diversified environment like that, combined with the best researchers and a very broad overall research scope, pursuing interesting ideas within that broad scope, might yield transformative results 20-40 years from now.

Continue on next page ⟶

Suggestions aimed at the funding system:

- In terms of designing the funding system to help increase the likelihood of transformative discoveries in Danish research as a whole, the group addressed peer review and grant decisions. In peer review, the more wacky ideas have a hard time being approved, especially if the PI doesn't have a strong track record, for example, if he/she is young. Their assessment was that it becomes even harder in panels where consensus is required.

- In the grant decisions, the group felt that the sleek proposals where nothing sticks out are more likely to receive grants than the ones that allow for wild ideas.

- Some of the same elements hold true for peer review of papers. There is definitely inertia in terms of getting the transformative ideas out of the lab, which is also documented in many of the discoveries that later ended up getting Nobel Prizes.

Suggestions aimed at the universities:

- In terms of designing the universities to help increase the likelihood of transformative discoveries, the group believed that universities must be good at creating trust and in showing the private foundations what overhead funds are being used for and hopefully making the foundations open to paying the indirect costs of the projects they fund.

- Also, it was considered important that the Danish universities expand their collaborations. This, combined with the above-mentioned point, would also increase politicians' trust in the universities.

- Danish research is truly excellent, globally speaking. It would be prudent to ride this wave, by way of increased efforts to show how the universities jointly lift research and education for the benefit of Denmark as a whole.

- There are already some good examples of this: the joint bachelor's programs between the University of Copenhagen and DTU for STEM students. The universities have an important job in making the politicians aware of the important role they play for our young people and mutual future.

Concluding remarks

Interestingly, when researchers from one of the main fields explained, how in their field the incremental steps were absolutely essential to something becoming transformative further down the line, they often said, that in other fields the transformative discoveries might occur much more suddenly.

The group from the humanities suggested that in mathematics, finding a new proof could have this more sudden character. However, a mathematician would say that finding a new proof would require incremental steps that can take a lifetime. The biomedical scientists suggested that astronomists could find life at an exo-planet and that would be an example of a sudden, transformative discovery. While the astronomists would say, that finding life outside our galaxy would require an infinite number of incremental steps; building the telescopes, knowing how to point them in the right direction, making the calculations, developing the materials etc., and then in the end, the perceived transformative discovery of extraterrestrial life would probably 'just' be a statistical argument.

This led us to the notion that in many areas it does not add to our understanding of the important elements of scientific progress to divide it into incremental and/or transformative steps.

Transformative research – how, what and why?

The DNRF has since the foundation was established continuously focused on funding Centers of Excellence that are based on ambitious and original research ideas. To be awarded a grant for a Center of Excellence, the board must be convinced that the combination of these elements: the ambitious and original research idea, the person who will lead the center, the core-members and affiliated scientists, and finally, the focus, structure and size of it – together, hold the potential for real scientific breakthroughs.

The CoE's research has led to numerous new scientific discoveries, opening of new research fields, rewriting of text books, patents and spin-out companies. The Centers of Excellence are at the level of the best research in the world in creating new knowledge or breakthroughs. They have fundamentally changed the way we understand the peopling of the earth, international law, bioinformatics and much more.

The most relevant conditions for creating this type of world class research have not changed over time and they include: research freedom, long-term flexible funding, recruitment of the best talents globally, infrastructure, and the ability to create a fruitful scientific environment and diversity in research groups.

From our conversations about the optimal conditions for transformative research at the DNRFs follow-up meetings with the centers last year it became clear, that transformative research is dependent on the above-mentioned framework conditions. It has also become clear that there are fundamental differences across different fields of research. First and foremost, we cannot predict *what* will become transformative, and we cannot predict *how* something will become transformative. The concept transformative research is more of a guiding principle, in a way that is similar to when Kennedy set the aim of putting a man on the moon.

We are facing major societal challenges that make the *why* self-evident. The DNRF believe that the research funding community can do something along the lines of Kennedy's highly ambitious strategical goal to the benefit of society.

The DNRF have learned a lot from discussing the topic with center leaders and center affiliated researchers during the past year.

We look forward to further discussions at our annual meeting.

Other DNRF **publications**

DIVERSITY AND EXCELLENCE IN RECRUITMENT AND CAREER DEVELOPMENT

OPEN ACCESS TO DATA – IT'S NOT THAT SIMPLE

THE POST-DOC CHALLENGE

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