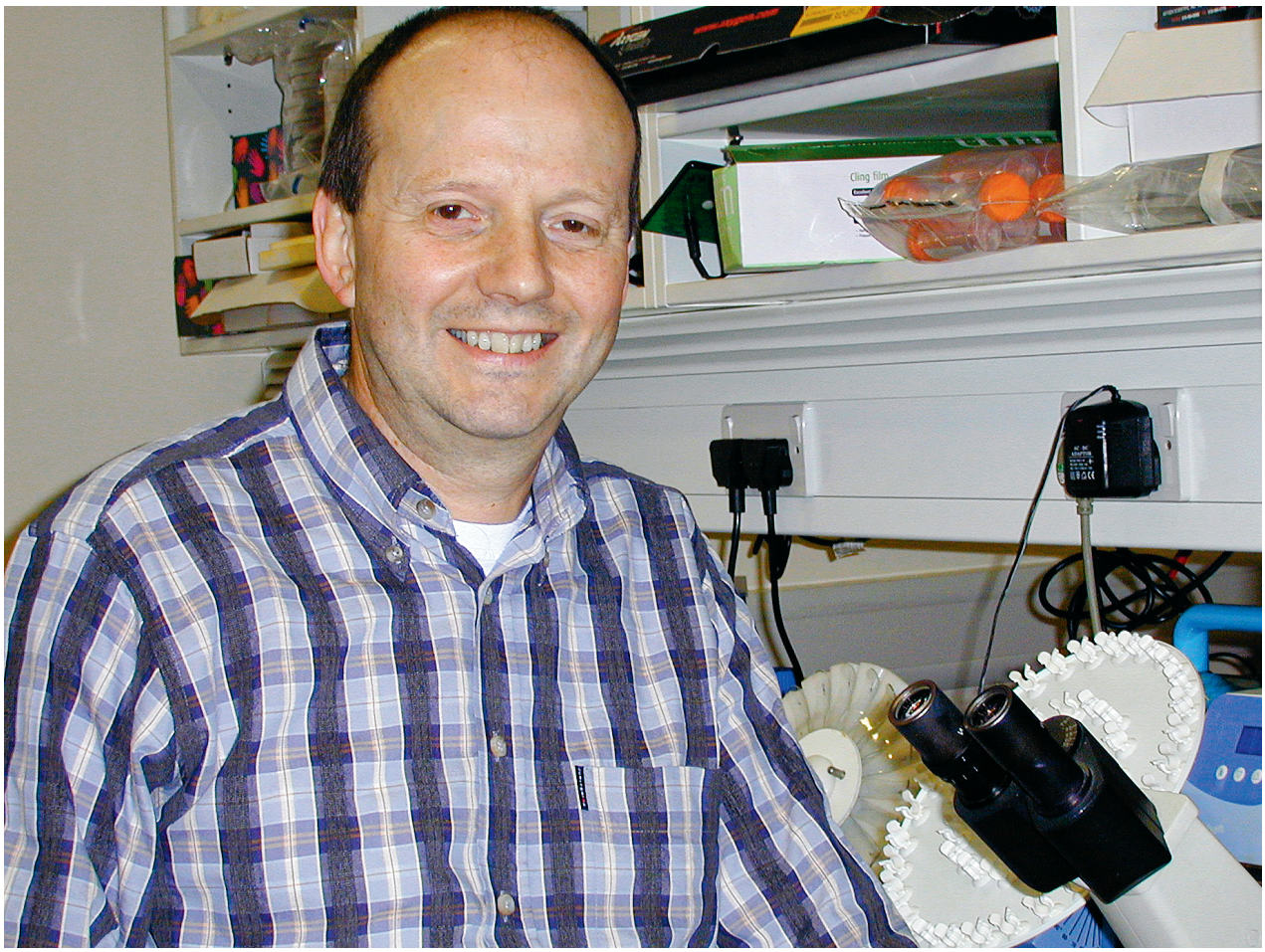
Ian Hickson in the laboratory.
Photo: Dr Ying Liu, University of Copenhagen.



KAPITEL 23

Professor Ian Hickson

The Center for Chromosome Stability (CCS) is located in the Panum Building, a colossal and rather uninviting structure from the 1980s, located across from the university hospital in central Copenhagen and housing much of the Faculty of Health at the University of Copenhagen. Its internationally acclaimed architectural style is fittingly known as brutalism and features unpolished grey concrete and little else. One could be forgiven for thinking that these surroundings are not the most inspiring, but this is where the CCS, led by Professor Ian Hickson, has its very modern laboratories and carries out its textbook-changing research on human chromosome stability.

Ninety percent of what we do is basic biology

The overall aim of the CCS is to understand how cells maintain the stability of their chromosomes and what happens when those chromosomes become unstable (Facts box 1). Although the center often pursues this goal in the context of human diseases, the overarching aim is to understand the underlying biology, according to Ian Hickson.

> "Ninety percent of what we do is basic biology. We want to understand how cells keep their chromosomes stable, and everything else flows from that. We focus on the molecular causes of human disease; we don't really try to work on cures. Of course, if something comes up, we won't ignore it, but that is not really what we are about."

Although Hickson started his career working on the basic biology of chromosome stability in bacteria and yeast, he has long since shifted his focus to higher organisms and to questions related to human disease. This is mainly because he simply finds it more interesting and relevant, and a bit because of the practical aspect that it is much easier to fund studies on human cells. He is rightfully proud of several of his works, but one in particular stands out: a Nature paper from 2003 [85] on a gene, BLM, that, when mutated, causes a human disease called Bloom's syndrome, a disorder that predisposes those who have it to cancer (Facts box 1). This work has been cited more than 750 times and was instrumental in Hickson being made a fellow of the Royal Society in Britain in 2010; in addition, the findings have become textbook material. But listening to Hickson, it seems that what he finds so satisfying about this study is not so much the wide acclaim for it, but the process through which it was done. There are only two authors on this paper, himself as senior author and a post-doctoral fellow, Dr. Wu, to whom we will return later.

> "Based on the function of the BLM protein and the partners it works with, right from the beginning we conceived a model for how we thought its role in chromosome stability worked, and we developed a very complicated way to test that. And it all worked out better than we could have imagined. We identified a completely new pathway for how genome stability is maintained in human cells – and it was something that we did from the very beginning, from the basic biology. We sat down with pens and paper and said, "I wonder if it does this and this and this," and then we set out to test that model."

While this study has more than stood the test of time, it is not the only one for which the Hickson lab is widely known, nor the only one that has changed textbook perceptions. Another is the lab's discovery of so-called ultrafine anaphase bridges [86, 87], which are filamentous connections between the chromosomes of two dividing cells that had previously eluded discov-

ery because they do not stain with dyes normally used to label DNA (Facts box 2). In contrast to the Nature paper on the Bloom helicase, where Wu and Hickson developed a hypothesis and systematically tested it, this phenomenon was discovered completely by chance when an observant student noticed the bridges in the microscope while studying another phenomenon.

> "This was a total accident and, in fact, when he first showed us these things at lab meeting, I said: "It looks like a curiosity to me and I am not sure there's anything interesting in this." This irritated him so much that he went off and proved that I was wrong, which showed what a great scientist he is. We've now been working on this phenomenon, studying it in all kinds of detail, for 10 years."

Very recently, the CCS also made a new discovery that challenges an existing dogma. Whereas the processes of DNA replication and mitosis (the separation of a mother cell into two new daughter cells) in humans are generally thought of as two separate processes, the researchers from CCS showed that parts of the genome are still being replicated during mitosis, especially during times when replication is stressed, and that this likely serves to protect our cells against stress-induced chromosomal rearrangements and therefore cancer [88, 89] (Facts box 2). This work has already been frequently cited, and Hickson considers it the CCS's most important work so far. Yet, again, he suggests that they "sort of stumbled on" this discovery.

That they "essentially stumbled on it," or "it was completely by chance" is something I have heard many times from excellent researchers. I recall several Nobel prize winners, giving talks at conferences, using those terms. Are they really more prone to stumbling on things than the average person, or is this just a way of expressing humility? I asked Hickson to speculate a bit about what lies behind this phenomenon in general, and the apparently great capacity of his lab for "stumbling" onto things in particular.

> "My suspicion is that lots of people stumble on these things, but they ignore them, in many cases because it seems too risky to spend time on it. In my lab, if somebody mentions an exciting observation at a lab meeting, I nearly always say, "go for it," because taking those kinds of risks is what you should be doing. I don't want to "stamp collect" and I don't want to do things just because they are there. We take a lot of risks and we make a lot of mistakes and go off on tangents that turn out to be fruitless. So, there is an element of risk for the people in the lab, but when we do find things and characterize them, we hope that they are among the most interesting things in the field. That is what keeps me coming in at the weekends. I still have a kind of childlike enthusiasm, and I am still very excited about what we do. I think it is a privilege to do science."

Hickson's lab is decidedly driven by questions rather than methods. They do not constantly chase new technology, and many of the methods they apply have existed for a long time, at least in the fast-moving chronology of science. That does not mean that they don't employ new technology, and their work has been greatly aided by, for instance, new advances in microscopy, allowing visualization of the finest details of cellular structures both in intact and in live cells. But the group often introduces new technology through collaboration. They have several ongoing collaborative research programs, both in Denmark and abroad, allowing them to take advantage of methodologies such as optical tweezers (Chapter 17) and microfluidics. In this way, they can try to mimic complicated cellular events using instruments developed by physicists.

Challenging conventional wisdom

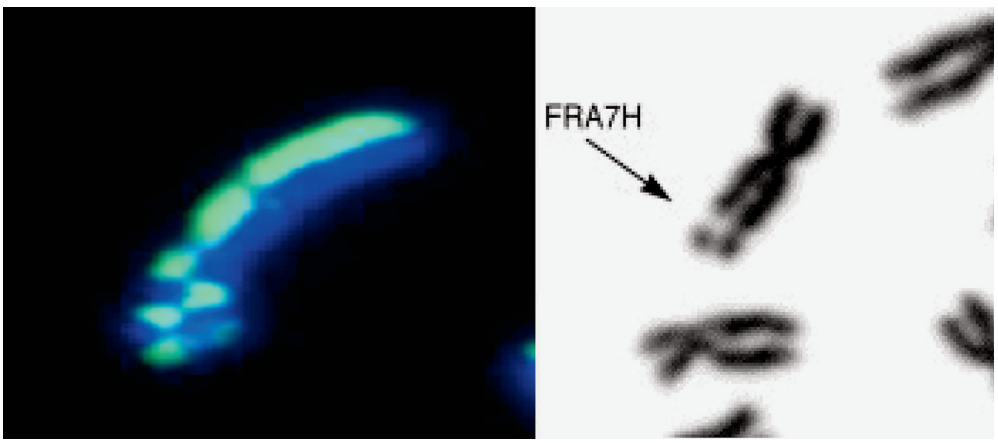
Consistent with his personal research focus on pushing our understanding of the big questions in biology rather than on pushing the boundaries of technology, Hickson's definition of excellent science has to do with challenging paradigms in the field.

The research at the Center for Chromosome Stability (CCS)

FACTS BOX 1

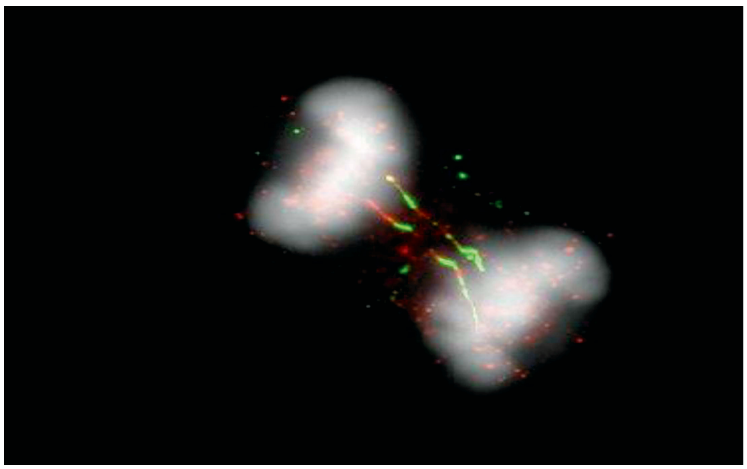
Our DNA – our genome – is packaged into 23 pairs of chromosomes, which are present in the nucleus of every single one of our cells. The chromosomes carry our genes, which become translated into our proteins, and which determine how our cells and hence our body looks and functions. The chromosomes are long thin threads of DNA, wrapped around proteins called histones. Every time a cell divides, the DNA has to be doubled and divided equally between the two daughter cells. If something goes wrong in this process, or if the DNA is otherwise damaged – for instance, by UV light or chemicals – mutations can arise that can cause cancer and other serious diseases. It is therefore essential that our chromosomes be kept stable, and our cells have numerous mechanisms to ensure this. The CCS conducts basic research with the overall aim of understanding in great detail how our cells can minimize the DNA damages that can cause our chromosomes to become unstable. More specifically, the main focus is on regions of our genomes that, due to their structure, are intrinsically unstable. These regions, which they term "the enemies within," include chromosomal fragile sites, repeated DNA sequences and telomeres – which are repetitive stretches at the end of each chromosome. One of the ways that increased instability of our chromosomes can be detected is by looking at the exchanges of DNA between two "sister chromatids" in a pair, as seen in the image on the left, where the two sister chromatids are labeled in green and blue, respectively. The image on the right shows a particularly fragile region in chromosome 7, called FRA7H, which appears as a gap on one sister chromatid, and is known to be particularly at risk for being broken during DNA division (image courtesy of Centre for Chromosome Stability, University of Copenhagen).

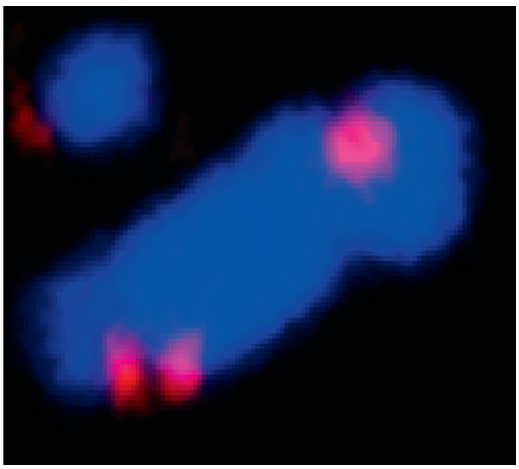
An important long term aim of the CCS is to understand how genome instability triggers age-associated disorders in humans, including cancer, neurodegeneration and impaired fertility. The center has identified several of the proteins that help cells maintain chromosome stability, and have shown that when these proteins are mutated, it can lead to life-threatening diseases. Another discovery from the center is that when chromosomes separate, they maintain connections that are now known as ultrafine anaphase bridges, or UFBs. These bridges, which lack histones and cannot be detected by staining cells for DNA, were discovered by an observant student in the lab, and are now one of the discoveries which have made the Hickson lab famous (see Facts box 2)



Ultrafine anaphase bridges and mitotic DNA replication

FACTS BOX 2





Ultra-fine bridges (UFBs) are thin threads of DNA that connect the two separating masses of DNA that will be inherited (one mass each) by the two daughter cells. The image on the left shows an example of these bridges, stained in green and red. They are only visible from the point where the masses of DNA start to visibly come apart during mitosis (called anaphase) as a prelude to the two daughter cells being formed. They don't stain with conventional DNA dyes and so they had remained undetected for the best part of 100 years, despite being present in just about every anaphase in a dividing human cell. They are detectable by using antibodies to specific proteins that bind to them, including the so-called Bloom's helicase (BLM). BLM is one of the proteins required for chromosomal stability, and mutations in BLM are the cause of the disease called Bloom's syndrome, which is associated with dwarfism and an increased risk of cancer.

Mitotic DNA synthesis, or MiDAS as the Hickson lab has named it, is a newly identified phenomenon where DNA replication (the copying of the genome), fails to be completed in the S-Phase of the cell cycle and instead is completed in the early stages of mitosis. The image on the right shows an example of the MiDAS phenomenon. The DNA that has already been replicated in S-phase is stained blue, but this chromosome shows three spots (red) where MiDAS has happened in early mitosis. The MiDAS sites were labeled with a method that distinguish them from all the rest of the DNA (image courtesy of Centre for Chromosome Stability, University of Copenhagen). The Hickson lab is still working out exactly how MiDAS occurs, but the current data suggest that it is not conventional replication occurring at the wrong time, but instead is a process that occurs only after replication has been halted and the DNA is broken – called break-induced replication. This is a process that has only been studied in detail in simple model organisms like yeast.

"There is a sort of herd mentality in science, as in many other things. People rapidly accept a concept, until somebody comes along and says: "This clearly cannot be the case, because of this new observation." That is the kind of research I like to see, where people are willing to take the risk of challenging conventional wisdom, busting myths, and coming up with new ideas. It is not just what they do experimentally, it's the thinking behind it. It's when someone can say, "You've

seen this in the textbooks for the last 20 years but actually it's wrong, and this is how we showed that.""

Hickson thinks that, very often, these kinds of breakthroughs are more dependent on somebody thinking that the paradigm of how a certain process works doesn't make sense, than on new technology. He likes to think of problems from the perspective of: if one were designing a cell from scratch, how one would do so? But he feels that, surprisingly often, people are happy to ignore the fact that something does not make a great deal of sense. I asked what else might set him apart from people who do less groundbreaking science.

"Nothing really. I think the biggest difference between myself and many of my colleagues is simply in commitment and interest in science. I probably spend 20 hours a week longer than most of my colleagues, maybe 30 or 40 more than some of them, thinking about and doing science, to the extent that many of my colleagues think I'm crazy or endangering my health. You know: Why is this idiot sitting here on a Sunday when he should be doing something more interesting? But I have done it all of my life, and I don't even notice it really. I love doing science, and I still have the drive and the excitement. I love sitting down with people in the lab and finding out what they've discovered recently. Not everybody maintains that drive throughout life."

This raises the question of whether work habits in Danish universities are different from [read: less committed than] those in Britain, where Hickson is from. A little bit, he concedes, although he does not think it is universally so.

"I think in Britain there was a greater proportion of colleagues who were very strongly committed. Here, there tends to be a little less commitment, but most of all it is the notion that science shouldn't dominate your life. It should be your job, and at the weekend, you go and spend time with friends or family and do something else. Obviously, if you've got a young family, then you have to, but in the end, it comes down to what excites you."

No great ambition to do research

Hickson does not come from a scientific background; in fact, most of his family didn't go to university. He was always interested in science as a boy, and for that reason, he went to university, but he did not really know what to do with his education.

"I certainly had no great ambition to do research, and to be perfectly honest, doing a Ph.D. was mostly a question of putting the decisions about what I wanted to do with my life off for a few years. My Ph.D. was unusual for those days in that it was sponsored by industry and I spent quite a bit of it working in a company, but on really whacky projects that eventually all fell apart. So, towards the end of my Ph.D., I had to make some big decisions. I started working on something totally different and I stopped going to the company. Perhaps what I have done throughout my career is that when things are going badly, I work extremely hard. In that time, I was in the lab every day of the week for months and eventually came up with something that was very nice and that became a Nature paper [90]. So, it appeared on paper that my Ph.D. was highly successful. But actually, it was very close to being a catastrophe, and I was very close to failing. But I turned it around. That experience was what triggered me to think: "Oh, I really quite enjoy this", and to decide to stay in science."

After that, Hickson did what everyone warns ambitious young researchers not to do. He stayed at the same university, Newcastle University, after his Ph.D., first as a post-doc and later as a faculty member, a lecturer. Then something happened that turned his career around: his Head of Department was offered a job at the Weatherall Institute of Molecular Medicine in Oxford, and asked him to move with him.

"I did, and stayed at the Weatherall Institute for 20 years. That move transformed my career from something where I was moderately successful to where we really started to blossom into a major group. There was so much luck involved again. Both the Ph.D. and the move to Oxford happened more or less by accident, and even the later move to Denmark was accidental in some ways. I'd been offered a job in Germany as head of an institute, and I was seriously considering going. But then some negative things started happening there, and the building that I was supposed to move into was not going to be finished on time. So, in the end I decided not to go. And I happened to mention this to Vilhelm Bohr, of the famous Bohr family, while chatting at a conference."

Moving to Denmark

Vilhelm Bohr (who runs a laboratory for molecular gerontology at the National Institute on Aging in the US) mentioned to Hickson that a Center for Healthy Aging would be starting in Copenhagen. Soon after, Ulla Wever, the Dean of the Faculty of Health at the University of Copenhagen, called him and asked him to come and have a look at a leading job there.

"I came to Denmark to discuss a particular job, but I soon came to the conclusion that it wasn't what I wanted. It wasn't a good fit for me, and there were too many research areas outside of what I understood or wanted to do. But then Ulla said: "Why don't we create something for you if you're willing to come." So I came, apparently to a much lesser job than the original idea, but I was ready for the change and they gave me very generous core support and allowed me to bring people from the UK. So, it was just an accidental conversation at a conference and from that everything else followed."

Don't waste your time with trivial stuff

We will return to the lab in Denmark later, but first, I wanted to hear something about who inspired Hickson in his approach to science. He immediately pointed to David Weatherall, the renowned director of the Weatherall Institute.

"Weatherall was a practicing clinician at the same time that he was running the institute and a research lab. He had a strong personality and had a big influence on most people at the institute. As far as he was concerned, there were only two types of science: good science and science that you don't even bother with. His view was: "Don't mess around trying to do trivial things; you've got to have big questions and big ambitions." Weatherall's own work was on trying to understand how certain disease gene mutations are maintained in human populations, such as how, in areas where malaria is prevalent, the population often has heterozygosity for other conditions that protect them against the most severe malaria. But when they're homozygous for that, of course they get that disease. In the case of malaria, it was blood diseases like thalassaemia. He wanted to know why the thalassaemias are concentrated in certain areas of the world, and the idea is that this is usually driven by infectious disease. He really brought me to thinking about bigger questions of human disease and trying to understand the molecular basis of that, rather than studying lower organisms."

Weatherall was also a pioneer of molecular biology in medicine, at a time when many medical schools considered molecular biology irrelevant to treating diseases. And he was, as Hickson says, rather critical with the university when it did not maintain a focus on molecular research.

"His attitude was, "if you're not going to help me, at least get out of my way." Even though he was a practicing clinician, he loved research and he wanted research to be seen as important in the university. And if he supported you, he supported you completely. You could knock on his door with an issue, and he'd pick up the phone and

call up somebody from another university and say, I've got this great guy who wants to do whatever. He had a very big influence on me."

It is not hard to see why David Weatherall was a key role model for Ian Hickson, as he seems to have a similar personality. He too burns for research, and that has not changed throughout his career. Like Weatherall, Hickson also sets very high standards for his work and that of the lab, and the excitement about science and the drive to do well push him to carry on. He is mildly surprised that so many of his colleagues seem to be frustrated by their job or even hate it. But he acknowledges that because he does very little formalized teaching, he has the privilege of being absolutely focused on his research – a general issue that we will return to in the last part of our conversation.

Intelligence doesn't equal brilliance

Talking about Weatherall leads to a discussion of what constitutes real scientific genius. Trying to define the characteristics of such people is not easy, and they are also not necessarily the same, Hickson stresses. However, one key trait is a fascination with science and discovery.

"Throughout my career, I can count on the fingers of one hand the number of genuinely brilliant colleagues I've had. The person who developed the Nature paper about the Bloom's helicase was one of them. Very sadly he isn't doing science now. He was a lateral thinker; he had insight into things that the rest of us simply don't have in that way. He had no papers for quite some time in my lab, and it got to the point where people started saying: "What on earth are you carrying on with this guy for? He's a total failure." To which I said: "He is going to make it big. He is trying to do things that other people don't dare." In the end he turned things around and suddenly published four or five major papers and then people had a different attitude. Intelligence doesn't equal brilliance. You have to have an inquiring mind, to be the sort of person who's happy just to sit and talk about science for the sake of understanding things. And if you have that kind of mind, then of course you also see things that other people don't see. Some people just have this insight to say, "I shouldn't ignore that; it could be really important." And that's what distinguishes people really."

Nurturing both the good and the brilliant

How does one find these geniuses then? When Hickson recruits new people to his group, he, of course, looks at their track record, from the viewpoint that past success is a good indicator of future success. But personal traits are a more important quality for him in many ways.

"I look for people who are clearly excited by science. I think there's a very big pool of people who can do practical science and among those, there's a very small pool of people who are sufficiently excited by, and committed to, science that they will make a success of it. I am looking for that kind of fire in the belly. I try to engage them in genuine scientific discussions during interviews, trying to gauge how they interact with me and how they look at science."

It is not surprising, therefore, that many of his lab members are very committed. Hickson also expects that. And, in return, they have his full support.

"I convey to people a standard and a level of commitment that I want to see. It's a bit like keeping a pot boiling. I think that those people who want to be successful in science, if you give them the resources and the facilities to do that, many of them will be successful. So, I back people financially as much as I possibly can. Apart from motivating people, I see it as my primary job to make sure that nobody in the lab is held back by lack of money."

In some high-profile labs around the world, the only way to get the attention of the boss is to be one of the two or three best students in the lab. This is not Hick-

son's approach. Those students who perform best are allowed to essentially run their own projects, whereas those who need more support get much more of his attention and time. He feels that it is his responsibility to try to bring everybody up to a high level. He is also very aware that most of his students will have their careers in industry rather than academia, and he would like to help them get to the point where they can get a top position. On the other hand, he thinks that the very best students may be best left alone to explore.

> "The very best people sometimes succeed most in a completely hands-off environment where they don't feel under pressure and can follow their own instincts and leads. You do have to give freedom to the people who have that ability. I obviously try to do that in a subtle way, so people don't necessarily notice that I'm treating them differently, but in practice I am."

The big questions and the currency of science

For better or worse, publications are the objective assessment system for deciding whether a researcher has been successful or not. So Hickson sees it as his responsibility that people in his lab all get papers published.

> "You can be the most brilliant person in the world, and if you haven't published, then when your CV lands on somebody's desk, they are not going to be very interested in you. Publications are the currency of science. So, of course, we're driven by wanting everyone to publish."

Balancing that with the desire to only address big questions can be challenging and also has to be tailored to the personality of the people in the lab. To Hickson, some people are willing to invest years in developing a big story without having papers, whereas others would be frightened to death, but are happy to "fill in the gaps," for instance, if someone has made a big discovery and resources are needed to finish it. Once such a major discovery has been published, the balance between going for new big questions and filling in the gaps is likely to change for a while, because then Hickson is likely to be moving in the direction of a new question.

> "I would love it if everything we did was big questions, but we have to accept that a pretty substantial proportion of what we do is fill in the gaps. But we do try, wherever possible, to move from question to question. Throughout my career, I have tended to stop working in an area rather soon after we have made a major discovery, to switch emphasis to something else. Both because after that, it becomes dotting the i's and crossing the t's, as they say, and because all the opportunists may jump on to it and suddenly everybody's trying to do the same things. So, either we drop something and move on, or we say ok, we're going to take a new approach and it's going to take another three years to develop that, by which time these guys will have gone anyway."

Academia versus industry

In the early 1980s, when Hickson did his Ph.D., having such a degree was still something reserved for the privileged few, and a Ph.D. degree would almost automatically open up numerous opportunities in and outside of academia. Today, there are many more Ph.D.'s and opportunities have diminished correspondingly. The very best people still succeed, and in Denmark, there are many jobs for Ph.D.s in industry as well. But for those wanting to stay in academia, he feels, things have become extremely tough. Faculty positions in universities are few and far between, and at the moment, an academic career is restricted to a small group of people.

> "I am certainly not one of those people who say to a student, "I'm not interested in you if you don't want to stay in research science." I think that's a very stupid attitude. In fact, I think most people who go through the lab shouldn't stay in research science, not in academia anyway. If I look at the people we have hired in our center

and our department, these are the ones who are getting ERC Starting Grants and this kind of thing. If you are just a little bit below that standard, you fall off the end of a cliff. The very top people can choose where they want to be, but the rest are desperately looking around in academia, and many times end up staying in dead-end positions which ultimately force them out of science. Unless you have a fantastic track record, you will be struggling all the time, and then it's a miserable job. If I have a career chat with someone and they say to me almost apologetically, that they've decided they don't want to stay in academia, then I normally say, that I think that's a very sensible thing to do. And it turns out that most people who did go into non-academic careers are really enjoying it and cannot understand why they didn't think about that earlier on."

Who is going to teach in the future?

In a scenario where the only people who can get a position in academia are those who get the most prestigious recognition for their research – which is only possible if you "eat and sleep science" – how likely is it that the university professors of tomorrow will want to spend a substantial part of their time teaching? And how can universities handle this?

"Yes, this is difficult. We actually had a meeting this very day about that here in the department. The simple fact is that the only recruits we've had in the last few years have been brilliant scientists who want to do research, and a lot of them teach only a small amount. And so now, we've arrived at a crisis point, we simply don't have enough teachers. Who is going to teach in our department in the future?"

The discussion about the balance between teaching and research has come some ten years later in Denmark than in the UK, where many universities are trying to solve this by hiring full-time teachers and teaching coordinators who are promoted on the basis of the quality of their teaching. In Denmark, there is much resistance to such a model, on the grounds that university teaching should be "research-based" – a given topic should ideally be taught by someone doing research in this area. In Hickson's view, much of the current problem has to do with the institutions' attitude to teaching.

"It is very clear that there is a two-tier system: if you're a top researcher you get promoted and you get lots of goodies, you might say, and if you're a top teacher, nobody ever thanks you. They want you to take teaching seriously, but the institution doesn't take rewarding teachers seriously enough. Teaching is viewed as a secondary job. As far as I'm concerned, you should be promoted on whether you're doing a good job, not what you're doing. But nearly all the people in our department who do most of the teaching have not been promoted, and that sends a very strong message. As a result, people say, "Look, I can't afford to spend time on teaching." For the moment, universities still think that they'll get their teaching done by just kind of leaning on people to teach a bit here and a bit there, but I think it is necessary to rethink how the system is organized. There is a very active debate about this at the moment and things might be about to change."

If we are moving toward a system where a lot of university faculty members, who are hired to do research about half of their time, don't have the funding to do any research at all, it seems to me that is not very good use of their skills or of the taxpayer's money. How do you view this development?

"I don't think it is sustainable at all. Historically, in Denmark there has been almost a fear of announcing that X is brilliant. But in the last years the total number of people being supported in research in Denmark has declined and those who are funded get proportionally more. So now you have a system very similar to the Anglo-Saxon system where an elite group of scientists are funded at an internationally competitive level and a large group of people get little or no fund-

ing. If the pot of money stays the same, there are only two possible strategies. The first is to support a small number of people to be internationally competitive. The argument for that is that we have to support our elite scientists at the level that our competitors are doing it; otherwise we're wasting our time and resources. The other view is that you're wasting enormous resources if you don't support all researchers in the university to do research. Maybe Danish universities need to look at having fewer core staff than they do now. Many British and US universities have a much smaller number of academic staff and a very large number of research fellows who are affiliated with the university but funded externally. It's a very unstable structure, but that's how it always was in Oxford, for instance, and that's how the Weatherall Institute ran – most people were essentially totally independently funded. And if the meritocracy – this elite funding system that we are seeing now – continues, it will happen here too."

Being funded at the elite level, getting big center grants for instance, generally requires that people be quite senior. But few people tend to get better at coming up with groundbreaking ideas with age. Doesn't the system of fewer and larger grants come with an increased danger of favoring the predictable and conservative rather than the paradigm-shifting? And therefore potentially losing that young post-doc with a brilliant idea who doesn't fit into anybody's big center?

"I would love to see a system where the government has a fund that permits risky but potentially hugely exciting research by a junior person. I think that would be a great way to put some money into the system but, currently, the system is being driven down a road where the resources are in the big groupings, like our center. So, as a young researcher, to avoid a miserable existence, I think you have to look at these structures and be flexible enough to fit into one of them. The junior people who've joined our center, for instance, join an infrastructure that immediately makes them much more competitive than they would be if they were on their own. But it does mean that if you're outside of these structures, you're struggling to be as competitive. That can be divisive in a system and you have to be careful. Having a very large overhead associated with the center grants helps the department and center become integrated and drawn into a partnership rather than becoming locked ivory towers. In my view, that is very important."

Professor Ian Hickson

Born 1957, United Kingdom

Education: Ph.D. (1982) Universities of Newcastle and Leicester /ICI Laboratories

Professor of Molecular Aging, Department of Cellular and Molecular Medicine, University of Copenhagen

Director of Center for Chromosome Stability (CCS), started 2015

Important previous posts: Principal Scientist, Weatherall Institute of Molecular Medicine, John Radcliffe Hospital, Oxford; Deputy Director, Cancer Research UK Oxford Cancer Centre, University of Oxford

Elected member of the Royal Society UK (2010) and the Academy of Medical Sciences UK (2010); elected member of European Molecular Biology Organization (EMBO) (2011); ERC Advanced Grant (2012); numerous international invited talks