Chapter 1 of Science 3.0. Real Science, Real Knowledge
by Frank Miedema. Amsterdam University Press, 2012.

1 The players and the game

A brief introduction to science

The invisible hand

It doesn't happen often, but if in an unguarded moment we scientific researchers allow ourselves to reflect on our chosen profession, it occurs to most of us from time to time that our job is indeed a very special one, and one that outsiders also tend to view as special, albeit for different – and usually wrong – reasons on account of old myths that continue to have wide currency. The interesting thing is that most researchers simply don't get around to reflecting on their activities very often or very much. And if they do, their main concern is with the impact of scientists and their products on society. These more concrete matters – the problems of science and society – are easier to analyse and usually more interesting than more abstract questions involving intrinsic aspects of the scientific endeavour. Nonetheless, some reflection on science, its products and its internal and external dynamics is essential given the immense and growing significance of scientific research in modern society. For over 150 years, and since the 1930s in particular, science and technology have played a crucial role in more developed economies as one of the key factors behind far-reaching economic and social innovation. These technological and scientific innovations have radically changed our social environment and the way we live. This seems to have happened autonomously, as the product of pure scientific research and the free market economy, a process often referred to as 'the invisible hand'. Science and technology, together with the innovation and economic growth that they bring about, have taken over our thinking about how our society and our lives are organised, resulting – it would seem – in a new ideology with its own intrinsic values and norms. Of course, this is just an illusion. Closer analysis will show that in today's modern society it is economics above all that is key to direction and pace of scientific development. Since the emergence of modern science in the seventeenth century, there can be no doubt about the enormous socioeconomic impact that the products of research have had, either directly or indirectly. The active manipulation of natural processes in an experimental design within a laboratory setting (which is one, if not the most important, feature of modern scientific research) and the subsequent successful translation to the real world made it possible to apply these very useful discoveries to industry, agriculture, navigation and health care. Once this process got underway, industry invested increasing sums of money in research aimed at product development. This interaction between science and society suggests a high degree of laissez faire, based partly on a popularised and often unconsciously assimilated mix of the ideas of Francis Bacon and Adam Smith about how scientific research generates prosperity and which economic system will best support this. But as soon as we researchers take time to reflect on the everyday reality of conducting and funding research, several questions immediately present themselves. The first – and at first glance perhaps the most important – is the mythical autonomy of science and scientists. Where do matters stand? Who decided in the past what should or could be investigated and when? Who decides today what research should take priority? How do people choose between research on rheumatism or malaria and HIV/AIDS and research on the possible existence of the Higgs boson?

Looking back

Scientific endeavour began as the hobby of wealthy or leisured individuals who possessed an intrinsic motivation, and above all a curiosity, to understand all manner of phenomena in the world about them and to search for laws of nature that could best describe these phenomena. Thus the first researchers were highly autonomous and enjoyed considerable freedom about their choice of subject. Because they didn't have jobs with the explicit aim of conducting scientific research, and in some cases because they had no paid employment at all, they did not have to answer to sponsors. Their aim was to present the results of their work to their peers, who were also engaged in research, and to be the first to make a discovery. It was at this time – the seventeenth century – that scientific societies were founded to enable these individuals to report to their fellows in person or in writing. These first instances of something resembling institutes were soon followed by the founding of research laboratories at universities where professors were originally appointed to provide instruction. By and large, these were private institutions that derived their income from tuition fees and from no-strings donations made by very wealthy citizens. Modelled on Wilhelm von Humboldt's initiatives in Germany, universities in all the developed countries were gradually transformed during the nineteenth century into independent research institutes which took on a key role in shaping modern society. In this way, the intellectual legacy of the Enlightenment was inherited, as it were, by universities and the modern academic system was born. It wasn't until the twentieth century that fundamental academic research was primarily and on a large scale paid for by governments out of public funds. Added to this were the various charitable foundations which offered grants in competition for special purposes. Rereading this brief historical synopsis, it occurs to me that after the Second World War, when all this was happening, there was fierce opposition – especially among scientists in the United States – to the idea that in order to do their research they would have to become dependent on the government. Surely this would undermine the unfettered academic freedom that they had so far enjoyed?[1] As we now know, scientific research has become completely institutionalised since the Second World War. A host of quasi-economic principles prevail in these institutions and the autonomy of researchers has been severely curtailed on a number of levels.

The players and the game

The most obvious curtailment, though less drastic in substantive terms, concerns the fact that researchers are embedded in teams and departments that are more or less controlled by a manager. They work on a particular topic, which is grouped according to output (e.g. genetic cancer research) or field (e.g. the molecular biology of cell division). More far-reaching in terms of research content is the fact that most funding is allocated in 'open competition' by committees in which a dozen or so peers, top scientists in the respective field, assess a great number of projects. These committees base their assessment on the quality of the project and the applicant and on potential scientific significance. As there are always far more deserving projects than there is money available, choices must constantly be made. The upshot is that many researchers are simply unable to do the research that they would like to! After some discussion, project review committees may conclude that 'we already know enough about receptor X in transgenic mouse Y' and that 'researcher A seems to have her best work behind her', although this is naturally worded very differently in the rejection letters. This system means that countless academic careers are ruthelessly terminated or even are nipped in the bud. It raises the question of which elite determines the priorities and whether that is not a highly subjective process. Clearly, here too we see our usual human fallibility at work. Our response is then to say 'Yes, that may be the case but there is no better system', 'but science has produced such marvellous results' and 'You can't make an omelette without breaking eggs!' What's more, we tell ourselves firmly that decisions *are* made on the basis of quality, especially that of the researcher. Through high-quality publications, and for the natural and life sciences this means in reputable journals or with frequent citations, researchers can demonstrate that they rank among the best.

Thus there is clearly a mechanism – known as the 'credit cycle' – that enables researchers to accumulate credit so that they can secure a new grant for follow-up research in the next funding round. While it is true that increasingly more objective quantitative measures are being used to gauge quality, the biggest problem is that we don't know how these figures relate to the true quality of a person's work, which is what a group of independent colleagues might be able to assess with integrity. And in exceptional cases, it is only after quite some time has elapsed that we are able to measure 'impact' within a field. This measuring and counting, or bibliometrics, arose in the natural sciences and although it can be used in behavioural sciences, with the caveats mentioned above, it is at present no good objective solution for measuring quality in the social sciences and humanities.

It is clear that there is little trace left of researcher autonomy in modern science. You will be dependent on colleagues at various stages of the credit cycle – firstly and repeatedly, on those who assess the articles you submit, preferably to a leading journal. Unfortunately, the short-term news value of your research is playing an ever-increasing role. In the next stage, you have to keep hoping that, despite your good publication record, a committee will find your new funding application of sufficient interest and quality. And finally, even if you eventually get through (i.e. survive) this cycle many times and your work is frequently cited, the committee may still prefer a proposal from a bright young thing who has incorporated sexier techniques and research questions into his or her project proposal following a successful postdoc period at a top-flight institute. You then have to go all out to somehow secure funding for your own research, which might be less fashionable, but which in your view is still sorely needed. If you don't, it could mean the start of a downward spiral and a rapid end to your research career. With this type of research agenda, there is a danger that committees will look for short-term success. While fashions and caprices within a discipline are often – though not always – relevant in terms of subject matter, they tend to be short-lived. Smart, shrewd researchers are often the first to rush to the next hype, publish early so that they can quickly secure new grants. This system is at odds with the ongoing need for long-term investment in more fundamental research. Fortunately, though on the face of it less sexy, this research which advances the discipline is still often paid for by direct government funding to universities. For this reason alone, we should nurture this source of funding. It must not, however, become a source of easy money and the research funded in this way still needs to be rigorously assessed.

Real science

This brief description of the economic planning cycle for science reveals a significant human component. Alongside strictly content-related matters, this human factor places considerable demands on researchers when it comes to negotiating, presenting, persuading, networking and playing politics. You learn this as you go along, just as you learn all aspects of research along the way, guided by a mentor who sets a daily example. In so doing, you learn what constitutes acceptable experimental results, what is publishable and how it can be most usefully written up. You practise communicating with colleagues, you learn to respond to fashions or the issues of the day, and how to satisfy and placate journal referees. Finally, you are trained to use flair and sound bites when telling your story at conferences. You also gradually discover which researchers you can believe – or rather, whose results you can trust. This real-life account of the practice of science is well portrayed in books by David Hull (*Science as a Process*)[2] and Frederick Grinnell (*Everyday Practice of Science*).[3]

In 1984, while doing my PhD research in a team that was run along modern lines at an Amsterdam research institute, I discovered a book with the intriguing title *Laboratory Life*. Published in 1979, this work by an English sociologist and a French anthropologist describes in realistic detail the everyday practice of research and its products. The authors adopted the same approach as that of anthropologists investigating a newly discovered Amazonian tribe during a field study.[4] This study has since become a classic and is still a unique example of its kind within ethnomethodological science studies. The book makes it immediately clear why researchers show little or no concern – either during their university studies or in their work – for the formal relationship between their results, statements and claims, and reality. In practice, this issue is simply never addressed. If the experiment can be repeated and it works, it is a publishable fact – at least for the time being. As soon as researchers find themselves in the laboratory, and this includes the odd amateur philosopher of science, they start to think like realists. This means that they accept that their conclusions and statements really do say something about reality. If closely questioned, however, they are usually shown to be pragmatists or instrumentalists. In simple terms, according to this philosophy, things are held to be true that have been shown to work – that is, until the opposite is proven to be the case and another explanation begins to prevail. Such changes often result from discussions between researchers about who has the best experimental data to persuade opinion leaders. Frequently, different explanations may exist alongside one another until one of them dies out. The idea that everyone in the field is working within the same paradigm is only partially true. Researchers may take older work as their foundation and may use many common techniques and concepts, but at the real frontline of science pluralism reigns supreme and researchers battle with those who have different hypotheses and opinions. These different views may coexist for years, with different schools working on rival hypotheses. These schools may comprise international consortiums of varying sizes which engage in debate at conferences and in publications, battling for supremacy. Senior researchers who have spent some time working in the frontline are well aware of these schools and anticipate them in their research and publication strategies. When results are published that contradict a prevailing major hypothesis, this then makes things very difficult, as it may mean the beginning of the end for this hypothesis, which is passionately defended by a researcher or a research team. New research data can be a form of 'creative destruction' that strikes at the heart of these vested interests. The proponents of established hypotheses frequently have a big reputation to lose, as well as material interests that hinge upon the success of their hypothesis, such as their job, their tenure, large NIH or ERC grants, invitations to speak at major international conferences – with all the emoluments that this entails – and to write reviews in leading journals. It is therefore not only for reasons of content that people cling to ideas, defending them against newcomers. This makes science as an industry unexpectedly conservative, which is not at all in keeping with Popper's philosophy that we should all be working together in a constant attempt to falsify established theories and hypotheses. The elite write the review articles that are most widely-read and influential (thus most 'believed') and they regularly help to update the textbooks that pass on the current state of knowledge to the next generation. This could involve research into subatomic particles, but also geoscientific hypotheses about the causes of global warming, or the pathogenesis of schizophrenia or AIDS. Debates of this kind are ongoing in developing fields such as research into cancer, cardiovascular disease, infectious diseases and their subfields. These discussions and their provisional outcomes often have immediate implications for funding allocation, and whether or not particular treatment methods or prevention strategies are incorporated in policy. So it's not just academic posturing!

Post-academic science

Postmodernism, at least in its extreme form, called all science into question, casting doubt on its rational foundations when it claimed that all knowledge – as the product of a culture-bound and subjective negotiation between researchers – is a form of construction. This led to heated discussions that were not confined to the social sciences. Some argued, for instance, that the fact that physics has always been dominated by men has had a direct impact on the products in that field. According to these authors this then gave rise to a typically male science that seeks to control and change nature in order to subject it to our will. Female physicists, these critics argue, will have a less invasive style of thought and show more respect for their subject. This position engendered deep mistrust among some natural scientists with regard to all modern sociology of science and science studies. There have been some wonderful, extreme responses that have strayed into classical positivism, with its adherents declaring passionately that in the natural sciences there is no social interaction, let alone negotiation, and that nature itself provides the answers and decides on the truth of our hypotheses and findings.[5] Needless to say, these 'culture wars' went entirely unnoticed by most natural scientists and the worst of the storm has now passed. A calm and fitting response to this discussion can be found in *Real Science* by the English theoretical physicist John Ziman.[6] In his work he is very much aware of the personal and subjective element at the experimental stage of research and he shows a balanced judgement about the intercollegial negotiation process and the criterion of experimental reproducibility. He also looks at a number of irrational phenomena, already described by Thomas Kuhn, which active researchers are all too familiar with. One example is the persistent belief in certain hypotheses, often flying in the face of people's better judgement, and their very slow demise, even after experiments by mainstream scientists have long since shown such hypotheses to be untenable. This is all rather irrational and not in keeping with the hyperrational image of the 'Legend' that we once believed science to be. Nevertheless, the collection of facts that the scientific community holds to be true is based very rationally on a well-founded, historical knowledge structure within a broader discipline that is constantly being tested for 'truth', i.e. utility. Ziman arrives at a fairly pragmatic definition of scientific truth which most scientists will not have difficulties accepting and which does justice to the day-to-day practices we are familiar with.

Real Science is Ziman's magnum opus, the perfect culmination of all the themes in his work of the previous 25 years. This alone makes it relevant, but there is another reason that lends it significance. He discusses in very direct and highly specific terms what has gradually befallen the time-honoured notion that science and its practitioners place great value on the ideal of disinterestedness. He contrasts the classical, value-free science – in other words, the proverbial academic science – with today's postacademic science: *'The ethical code supporting the norm of disinterestedness cannot stand up to the external pressure to exploit the ever-growing instrumental power of science'*. There is indeed a discussion within the scientific community about how we arrive at agreement on our views about the subjects we investigate. Another important question, however, is who decides which subjects should be researched. As we have concluded above, the last 60 years have seen an enormous influence from outside, with social and economic forces increasingly setting the science agenda. Whereas in the past, say a hundred years ago, we might have found this strange and undesirable, we now live at a time when the public and politicians simply demand that research should be of benefit to society and have certain expectations concerning its economic and societal applicability. The government insists that public institutions work in partnership with the corporate world, with the commercial sector, and has made this a precondition for allocating funds. This same thinking has placed immense pressure on universities to patent and exploit the knowledge generated by their researchers. Universities are obliged to become entrepreneurs and to enter the marketplace in order to supplement their budgets. The informal pursuit of intellectual hobbies, motivated purely by personal curiosity, is now regarded as abhorrent and irresponsible vis-à-vis the community that keeps the universities afloat with taxpayers' money.

Although much can be said here by way of counter-argument, scientists need to realise that this is the new reality of their social playing field. As Ziman rightly points out, the disinterestedness or value-free nature of science as an absolute principle has always been difficult to sustain, but now there's no holding back and we have to accept that science, and therefore scientists, are being harnessed for a wide range of specific societal issues. Scientists are operating in the marketplace, with clients and direct sponsors. As a consequence, researchers run the risk of losing once and for all their status as independent 'truth-tellers' and being reduced to mere hirelings who, as Ziman says, will not lie outright but who will sometimes not tell 'the whole truth' in the interests of the market operator who is their client and sponsor. It is hard to say just how this will affect the credibility of scientists as consultative experts, but it will certainly not improve it. As Ziman bluntly puts it: *'The proprietorial attitude to the results of research has become so familiar that we have forgotten how damaging it is to the credibility of scientists and their institutions'*.

It works!

Indeed, a number of prominent science watchers observed in 2008 that this now fully accepted societal fusion of scientific and commercial activity has profoundly and permanently changed the mentality and culture of universities, especially in faculties where this fusion is most in evidence.[7,8] As in the humanities and, say, economics, we now see in the natural sciences too the rapid emergence of a pluralism coloured by the subjective and sometimes political background of the researchers, but also by their investors and clients. The upshot, as we witnessed as recently as 2009, is that the opinion of an experienced scientist may be called into question. In this particular instance, the fact that the research was funded by the AIDS Foundation rendered its findings unreliable – at least in the eyes of some – in a public debate instigated yet again by a layperson about whether AIDS is in fact caused by HIV! Understandable perhaps, but certainly galling. In a similar vein and even more so than in the past, we critical citizens will have to scrutinize the opinions of experts on climate change, the benefits of the mass use of AIDS inhibitors in Africa, the best economic and political measures to prevent yet another banking crisis or the benefits of the micro-credit programme in the light of financial and/or political ties between these 'truth-tellers' and interested parties. In his book, Ziman has made a complete synthesis of what science today actually is, addressing at length all the relevant elements of epistemology, the sociology and philosophy of science, and postmodern science studies, including interaction with external economic factors. It is clear that the myth of what we once believed science to be, which he calls the 'Legend', has been unmasked once and for all as a result of the shift in the way we think about science – from normative to descriptive – and by taking a close look at the practice of science. Along the same lines, inspired by Ziman's earlier writings, Henry Bauer, in his book *Scientific Literacy and the Myth of the*

Scientific Method has presented a powerful analysis of the fables and misconceptions about how science really works.[9] Bauer explicitly summarizes the problems regarding the proper understanding of the processes and the status of the knowledge claims (products) of science that result from these misconceptions which pertains to the general public, policy makers but in many cases even active scientists. He makes a strong case that, prompted by an enchanted view of science or political ideals, we mustn't seek to keep this myth of a perfect science and of a perfect scientific method afloat but that we must enhance the level of scientific literacy among our students by incorporating courses on science and technology studies (STS) in the PhD programs but also in bachelor and master curricula. Indeed, most of us and even our Bachelor students still have been mostly raised on this 'enchanted view of science' lectured to them by science teachers who themselves rarely have been in the trenches and at the front lines of modern science. Ziman proposes what he calls a naturalistic model or description of science, which is not normative but descriptive. Based on that model, he calls on us to honestly state in the public debate just what the limitations of science are with regard to the statements and claims that its experts make. There is no absolute truth to be found here; we must say goodbye to all transcendent pretensions regarding authority, while nonetheless powerfully communicating the message that science is the best system of knowledge production that we have and that we can expect it to make rational and reliable contributions and deliver working solutions to a host of problems confronting society.

Critical science

The present interaction between science and society is largely dominated, and this is certainly true in quantitative investment terms, by economic factors and hence by the private sector. This was one of the three options outlined in Jurgen Habermas's celebrated analysis from the 1960s.[10] In this option, scientific technocrats would determine what should happen, while in the other two options, the emphasis was on politicians or the people. In the 1930s, the Marxist sociologists were the first to see that socioeconomic forces would have an enormous influence on science, something which incidentally was understood by the most astute leaders of the American corporate world, albeit from a completely different political perspective. This last point was recognised by the American economic thinker John Kenneth Galbraith, who throughout his life warned against the excesses of the free market, with American society as his field of activity.[11] The 1960s saw the rise of left-wing intellectual movements, inspired by neo-Marxism and the critical theory of the Frankfurt School in particular, arguing that science should be deployed as a major innovative force to address the problems of society. The idea was that new technologies could eliminate tedious and mind-numbing manual labour, while at the same time tackling the problem of the gap between rich and poor, both in developing countries and between north and south, as well as global problems in the areas of energy, food and health. These were not matters to be left to market initiatives. Instead, the government should play a dominant role, a view most notably embodied in the Netherlands by the left wing government led by Den Uyl (1973 to 1977). It was also the first to include a Minister of Science, who was a member of the proto-green Political Party of Radicals. The academic community argued for a critical university which would proactively monitor, and where necessary criticise, societal changes. Entirely in keeping with the spirit of the times, people were apprehensive about the military-industrial complex and about large multinationals that not only increasingly determined what the world looked like, but which also had science in its grip. At that time Rose and Rose, a neo-Marxist British couple, published critical-theoretical analyses of these interactions, revealing that science had not been value-free for a long time and instead had been captured on a massive scale by capital. At the same time, and picking up on this idea, research centres in the field of science and society were set up at various Dutch universities. These centres studied the interaction between science and society from a scientific, economic and sociological perspective, but also tackled topical issues such as social lobbies, the desirability of nuclear energy and the energy question as a whole.

A moderate but leading representative of this school of thought was Egbert Boeker, who later became rector of The Free University in Amsterdam. In 1975 he published a book that was widely read at the time in the Netherlands but which now comes across as very idealistic, and above all somewhat naive.[12] Boeker accepted Galbraith's idea that the market would become important, but only if there was too little money for publicly-funded research – a situation, he believed, that the Netherlands would never have to face. He foresaw a host of societal forces, such as trade unions, civil action groups and politicians, playing a key role in directing science, and argued that only 15% of research funding should be made available for untied, basic research. He pointed to Francis Bacon's *New Atlantis* and to *New Babylon* by the Amsterdam Provo artist Constant as sources of inspiration for realising the utopian ideals and the enormous potential of science and technology. In this way of thinking, science in the public sector would be almost entirely deployed for the greater public good and every citizen would have an opportunity to participate. Politics and active citizens were the movers and shakers in this makeable society. This vision seems light years back in time if we analyse how we now think about the interaction between academia and economics. Despite Galbraith's words of warning, today's approach is driven by neoliberalism.

Science 3.0

The world of science, and therefore the environment in which researchers have to work, has undergone radical changes since the 1970s. These changes in organisation and culture have occurred in parallel and concurrently with changes in our general socioeconomic thinking about the way in which society should be organised. Science has not proved to be a safe haven in a world that is increasingly dominated by global economics. There is constant, open interaction, which right from the outset – for more than 100, though we could also argue 200, years – has had an enormous impact on how research is conducted and on which research is conducted. There are also growing calls for an overarching science that will help solve the major problems of our times. This entails a different kind of research with different quality criteria than the classical, more discipline-based research tradition in which most of us have been trained. The main difference is that the problems addressed do not come from the discipline itself, arising out of earlier research, but from outside. This makes them by definition of a different order and complexity. For classical reductionist, monodisciplinary researchers, these problems are less 'clear-cut'. The research we need doesn't follow a linear progression from basic to applied research, followed by an implementation stage, but is organised in its entirety within the context of the societal problem.[13,14] A current example is research aimed at combating infectious diseases in developing countries. Here, basic research on the spread of infectious diseases only has value in the context of research in the fields of macro and micro-economics, social geography, public administration and political science. Researchers must therefore immediately engage in dialogue with the client, something which is already happening with the relevant industrial partners in the large government-funded life science programmes. In this new approach to the practice and management of science, there is a danger that dependence on research funding will allow business to take over science completely, placing it at the mercy of the free market. It is up to academics themselves and to administrators of universities and other independent knowledge institutions to engage in public debate, and to constantly and vigorously alert the public at large and politicians to this danger. While there are no guarantees of a positive outcome, it will help enormously for opinion leaders within science to be aware of science's altered position and status in society. This calls for lucid and repeated reminders of the need for an optimum mix of context-driven and curiosity-driven research that is appropriate to the knowledge economy that everyone quite rightly is talking about.

In our analysis and experience, science gradually shifted from Science 1.0, which was entirely autonomous and science- driven, to the Science 2.0 stage, when it entered into dialogue with social stakeholders about its results and products. From there, it has evolved to become Science 3.0, a form of co-creation in which scientists work in partnership with external parties in order to seek a solution to a problem. This cooperation encompasses the selection of the societal issue, the research design and the evaluation and implementation of results. Of course, these stages didn't occur in exactly this way, in these pure forms or in this strict sequence, but they do show in general terms the progression that science has made over the past 60 to 70 years. It is vital that scientists are aware of this change and thus aware of their position and role in society. Fortunately perhaps, these matters don't loom large in the hectic lives of most young researchers, who often don't become aware of them until a later stage, when confronting problems about independent career opportunities or when becoming involved as administrators in the politics and economics of the pursuit of science.

Reading guide for young scientists

This book addresses at length a number of these themes with the help of several authoritative authors whose thorough analyses on these subjects will withstand the test of time. They reveal the impact that changes in a wide range of external and internal factors have had on day-to-day scientific endeavour and on the status of science in society. These changes are often very gradual, and only become fully evident after some time and from a distance. These essays, written by a medical-biological researcher who has developed into a research manager and administrator, are intended for researchers in all the sciences, not just the natural and life sciences, as these themes are relevant to us all. This book is also aimed at interested laypeople, at the corporate world and at politicians in search of a better understanding of what is happening in the world of science and research institutes. Literary novels on the theme of science as an aspect of modern life also provide useful insights into how these changes can govern the daily life of researchers and therefore a chapter has been included that discusses a few of these novels old and new.

I hope that this volume will trigger a genuine curiosity among readers to consult the original works. Like a number of other classics within this discipline, such as Thomas Kuhn's 1962 work,[15] I believe most of these books will long continue to be of immense value in the debate.