Chapter 14
Some Recent Trends in Science

14.1 The Impact of University Mass Education

Since antiquity and until rather recently very few people were occupied with what we now call science, i.e. systematic thinking about nature, society and humans. This has changed profoundly the last decades in all western societies, and is on its way in the entire world. This is, one may assume, related to the expansion of higher education. Some 50 years ago, relatively few people, less than 5 % in advanced countries had a university degree, while nowadays it is 40–50 %. One may perhaps assume that most of what students learn during their academic studies is based on science and hopefully approximately true. Hence one may entertain the hope that greater and greater portions on mankind are able to make better decisions, act cleverer and not so prone to devastating mistakes as in older times. I do entertain that hope, albeit there is also reason for pessimism.

One such reason may be found in research into human decision-making, as reported by the Nobel laureate Daniel Kahneman in his book *Thinking: fast and slow*. In this wonderful book a huge number of experiments on human thinking is discussed. Kahneman's core message is that we have two systems for decision-making, the fast system and the slow system. The fast system acts unconsciously, using inherited and early in life learned emotional responses: good or bad for me? Thus, for example, new faces immediately give us a positive or negative feeling which guides our actions. Sometimes this fast decision system is very useful, even life saving, as when we feel a strong impulse to run away from a dangerous situation. Kahneman's analysis of this system is that it often uses rules of thumb and association circuits for producing a quick response. But association mechanisms sometimes produce clearly irrational reactions, via a mechanism called priming.

The other system is the slow one, where we deliberate, consider alternative actions, estimate how reliable the available information is etc., in short when we use our rational faculty. The most interesting result reported in the book is, in my view,

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that the fast system often makes the decisions, while the agent himself believe he has considered the alternatives and can give good reasons for his choice. The conclusion to draw is that we often think we are more rational than we in fact are.

It is possible to improve ones rationality, and to train oneself to be a bit sceptical against ones 'gut feelings' in situations where there is some time do make a decision. On the other hand, 'gut feelings' may be the result of unconscious reception of information, of for example other people's attitudes and emotions, which they try to hide. So when should one succumb to the first thought, to ones gut feelings? No easy answer is available, so far as I know. Nevertheless, I think one may entertain some hope that the future for mankind will improve because of deeper and more widely disseminated scientific knowledge.

All in all, there is, I think, *some* hope that more widespread scientific knowledge will improve the conditions for mankind, and for many individuals. And in democratic societies one may hope that policies will be cleverer if the general level of scientific education improves.

14.2 Publish or Perish: The Value of a Research Paper

Not so long ago (100 years?) nearly all scientists were either wealthy men (seldom women) doing science as a hobby, or university teachers who primarily was hired for teaching while doing research in their spare time. This has changed. Very few people do research without being paid for doing just that and universities now select professors for hire based primarily on their track record as researchers (although most have a non-negligible teaching load). Professors all over the world now see their prime duty as doing research. And since the enrolment to university and college studies has risen tremendously in the developed world, a huge number of university teachers have been hired. All these people compete for better positions and promotion, and since the main merit in academia is research results in terms of scientific publications, they focus efforts mostly on publishing scientific papers, not on their teaching. So, because of the enormous increase in students at universities there has been an enormous increase in university teachers and thus in the number of published papers. Hence this increase is to a considerable extent a side effect of the increase of basic academic education.

Scientific papers must be read in order to be of any value. Alas, there is strong evidence that the great majority of scientific papers are never read by anyone else than the referees and the journal editor.

First of all the majority of papers are never cited, and secondly, there are findings (see for example http://www.complex-systems.com/pdf/14-3-5.pdf) suggesting that only circa 20 % of cited papers are actually read by the citer (This paper I have read!). One may conclude that only when a paper are cited five times or more can we have some confidence that it actually have been read.

One could of course argue that many papers are read without the reader finding reason to quote it. This is plausibly true, but it only strengthens the hypothesis that,

from the mankind's point of view, most scientific papers are of no worth. For if a scientist read a paper but don't have reason to refer to it in his future research, it had plausibly no or little impact on this researcher's thinking.

The researcher's subjective aim of publishing a paper is his/her own career, as already pointed out. But why not putting more effort in writing for a broader audience, thus promoting the general level of scientific understanding in society? From a societal point of view it would be clearly rational, but, alas, from the point of view of the individual researcher it is not, since writing for a broader audience is not valued in academia. As Nicholas Kristof wrote (New York Times, February 15, 2014): 'If the *sine qua non* for academic success is peer-reviewed publications, then academics who 'waste their time' writing for the masses will be penalized.'

One may conclude that in most cases writing a scientific paper is waste of time. But there are, of course exceptions, some papers are indeed highly valuable.

14.3 Research Funding and Planning

Could one diminish waste of money and time on bad ideas and increase the proportion of useful research? I'm sceptical. Research funding bodies must act just like venture capitalists. Such capitalists, I have been told, select a number of projects/companies and put in their money in these. They know that most projects are bound to fail, but since it is impossible to know which, they have to spread the risks and a reasonable number of projects are supported. Hopefully one or two succeed so that the return on that investment compensate for the losses on the failed ones. The decision problem for research funding bodies is similar and they should apply a similar strategy in order to optimize the value of the research done. I see no other rational option.

However, those funding research naturally would like to get some guidance on which projects to support. One could for example look at history of science to see which kind of strategies have worked in the past. One such idea is give money to researchers that have a good track record, people who haven proven their ability to produce relevant new scientific knowledge. Several research councils have adopted this strategy of giving money to excellent researchers. Such a strategy will result in strong bias for senior, or very senior researchers. Is that a good idea? Wouldn't it be much better to give money to newcomers, who have new ideas?

The problem of deciding which projects to support and which hypotheses to test is the question about which inductive inferences to give credence. But this is an unsolvable problem. It is impossible to give a general justification for inductive reasoning and a fortiori for selecting one project among a number of alternatives that objectively is the most probable one to succeed.[1] There cannot be any way of

[1] To use Bayesian reasoning to update one's credence is no solution, since one must have a prior probability as a starting point and no objective way of finding that is available.

sorting out those inductive conclusions that later will be confirmed and those that will be falsified, and hence no good reason to invest resources in one project rather than another. As Quine once remarked: 'The human predicament is the Humean predicament' (Quine 1969, p. 72). Hence it is no good idea to concentrate too much resources in any particular research programme, whatever the arguments. Who is to know that the chosen line of research is not a blind alley?

My conclusion is that the great scientific achievements the last decades are explained by the enormous increase in money for research and in number of researchers. It is like mass bombing from high altitudes of an area: if the bombing is heavy enough, some bombs will hit the targets. More bombs, more targets being hit.

14.4 Big Science

Big science began in physics with the construction of big particle accelerators, such as that at CERN in Geneva or SLAC at Stanford. These investments were not motivated by any desire to test any specific hypothesis, but a recognition of the fact that testing any hypothesis about the fundamental structures of matter require this kind of equipment (And one motivation for the US congress' funding such research was the arms' race between Soviet Union and USA.). Well, one cannot, as a matter of principle, dismiss the possibility that it will be possible to probe deep into microphysics by other, cheaper methods; but so far we see no alternative.

Now we see something analogous in social science, although much less money is needed. Since the costs of collecting and storing enormous amounts of data has fallen dramatically one may consider building and maintain really big data bases, and the parallel rapid development of computers make it possible to survey these for correlations or other things. We can do data mining. Even though the costs are minute in comparison with the cost of building and running CERN, there is nevertheless a somewhat parallel phenomenon, in so far that basic research facilities have been constructed without there were any particular hypothesis up for immediate test.

Will these investments pay off? Well, I think theoretical physicists would say: CERN has given us the possibility to test the standard model; furthermore we have now confirmed beyond any reasonable doubt the Higgs mechanism, predicted 40 years earlier. This is deeply satisfying. But even the opposite result, a disconfirmation of the Higgs mechanism would have been exciting, since it had forced us to accept that there is something wrong with the standard model and new ideas would have been asked for. So from a scientific perspective the return of this investment was really good.

Some European taxpayers may think differently, asking: why should we pay for this? We can't see any benefits for us, nor that it has any public interest. CERN is a playground for academics.

I don't know whether these sentiments are common, nor can I see any effective way to rebut the argument. At bottom, this is a dispute about basic values. If someone says, in the vein of Aristotle, that man by nature is curious, we want to know no matter the usefulness of the knowledge, and another says, 'I disagree and I don't want public money to be wasted on that', I see no rational way to adjudicate this dispute objectively. Disputes about values are settled at the polling stations in democracies.

A different, but equally unsolvable problem arises when constructing big databases with all sorts of information about human beings. The costs are much smaller, and the possible benefit might be easy to argue for, but privacy of individuals may be threatened. It is common practice to take measures to protect privacy and integrity of those the data are about, but are these enough? One need to strike a balance between privacy and usefulness of the data base, and it is bound to be people disagreeing about the balance.

14.5 The Scientific Attitude and the Search for Meaning

In Chap. 1 I described the beginning of scientific thinking as an effort to try to understand events in nature as effects of natural causes, not the result of interventions of gods, spirits, demons or other creatures with desires, volitions and goals.

In the course of time the idea of natural laws developed and therewith the conception that events and states in nature, including our bodily states, are determined by initial conditions and deterministic causes. Hence, there is reason to try to predict some aspects of future using hypothetical reasoning; if I do such and so, then that will happen; but I do not want that to occur, so I had better do something else. And success has sometimes, some might even say quite often, followed.

It should be noticed that such hypothetical thinking might be useful even in the absence of any detailed knowledge about the mechanisms. Clear examples of this are observations of how the plague spreads from area to area. Although one had no knowledge about the agent, bacteria, one could do *something* to restrict its distribution. During the outburst of plague in England 1665 one took strong measures, such as putting ships from abroad in quarantine for quite some time, restrict domestic travels and closing the universities (which, by the way, made it possible for Newton to return home to his mother's farm to work on his magnum opus *Principia*).

Our possibilities to control our environment has increased tremendously due to better knowledge about laws and mechanisms. But still there are, in Quine's words, unsettling surprises. When such surprises hit us, such as severe diseases, accidents, tsunamis, etc., most of us are hard to accept that there were no purpose, no reason, and no explanation. Our desire for an explanation, which provides a sense of meaning of a personal disaster, is deeply felt and few are by nature prone simply to accept that one had bad luck.

This desire for meaning-giving explanations of the misfortunes in our life is connected to our natural desire to understand our own life and ourselves. Few humans, perhaps none, are willing to look back to their own life and view it as a sequence of events totally without meaning, without purpose. Humans by nature construct meanings in their own life. We try to integrate random events with dire consequences into some of meaningful pattern when we reflect on our life. It is not uncommon to hear people saying about such an event that it had a meaning after all; I learnt something, I became a better person, I met my husband/wife, etc. That is, even if we know that it was a random event, we give it a meaning ex post facto. Pious Christians in particular try to see a meaning also in severe suffering (it make you a better human, more humble, more compassionate). And many people, when they look back at a disaster in earlier life, which they were able to cope with in the long run, say: there was a meaning after all.

This is a remnant of the pre-scientific thinking where all events were considered as the effects of purposeful actions of spirits, gods or demons. When we are able to think of something as an agent, as something with a mind, we say that we understand it. To understand someone, is, as Collingwood puts it, 'to put oneself in the other's shoes'.

Should one denounce this urge for understanding of disasters and other unpredictable events as bad thinking? My view is that it depends on which conclusions one draws. It should be clear that calling something meaningful, in the sense here given, has no implications for our future actions and no predictive force. So if it has no impact in our actions and decisions, it seems innocent and perhaps beneficial for well-being (It might even function as placebo.). But if someone allows thoughts about the meaning of natural events to guide his choices about how to act, he runs the risk of making, from his own point of view, bad decisions.

No one can live, I grant, without trying to find a meaning in his/her own life. But we should not allow this craving for meaning to interfere with our search for reliable knowledge about laws and regularities relevant when deliberating on which actions to take. Of course, we may make wrong inductions about which regularities there are and hence which actions best to do. But trying to use wishes of gods as guidance is bound to fail.