Some remarks on the philosophy of science (see the last chapter of the book)

Science is located between concrete empirical facts and remarkably abstract general principles.

The best scientific-philosophical article I have ever read is Anderson (1972) “More is different”. Starting with a particular example it embarks on an extraordinary study of symmetry, emergence and complexity. Although these subjects are at the edge of our conceptual grasp, the author (who became nobel laureate for physics in 1977) manages to describe a number of hierarchical layers, each with its own distinct set of rules. Altogether there even seems to be a general organizational principle at work. But be this as it may, in any case, Anderson has a deep understanding of what is going on. Thus, despite a fair amount of speculation, the reasoning is thoroughly based on undisputable physics and chemistry.

At the other end of the spectrum one finds concrete, impressive applications of science. The laser, i.e. the effect of **l**ight **a**mplification by **s**timulated **e**mission of **r**adiation, is one of these. It was first derived from physical theory by Einstein in 1917, and experimentally confirmed in subsequent decades. However, it was not before the 1950s that physicists began to build technical devices that could emit coherent light reliably. In subsequent years the field boomed, with the number of laser types increasing rapidly. Nowadays there is an impressive list of applications and the laser has become a part of everyday life. Last but not least, empirical progress retroacted, i.e. it prompted interesting conceptual developments, in particular in the field of phase transitions (e.g. synergetics).

Finally, even without much theory, there are striking and useful hard facts. I think it was an extraordinary discovery that microbes can live in gastric acid, and that at least one of them may cause stomach ulcers. (For the detection of “heliobacter pylori” B. Marshall and R. Warren received a Nobel prize in 2005.) Although Popper would be happy with this example - since he could claim that the theory that bacteria cannot survive under such harsh conditions was falsified - I think it is rather the unexpected fact itself that is of major importance. At least, because of this discovery, many patients suffering from stomach diseases can be treated more efficiently. On the theoretical side, the unexpected observation encouraged research in search of bacteria living under even harsher conditions. This search was successful, in particular near hot springs in deep sea. We have thus learned that life is much more adaptive and robust than previously thought. It is truly “made to survive” which is quite an insight.

All these lines of inquiry are consistent with one another: On the one hand remarkable data help improve theory or may even trigger completely new developments. On the other hand “nothing is as practical as a good theory” (Lewin 1952). That means, if we have understood something, an amazing application that makes a difference in the real world is not far away. Laughlin (2005) says: “… the invention of the original transistor set the standards of our discipline, and it still leads most of us to consider the highest achievement in science to be the rendering of facts down to their essentials so effectively that some practical invention becomes possible.”

Unfortunately, in everyday life, the gap is typically larger. On the one hand there are theorists mainly working at universities and discussing abstract theories with other theorists. Thus there is the real danger of “inbreeding” with respect to personnel and subjects being studied intensively. Peer review rarely remedies this flaw but rather adds to it. On the other hand, in the economy, in administration

etc., practitioners are muddling through, making things work – somehow. There, very often, systematic and theory-based efforts would be more efficient, i.e., they could provide faster and more elegant routes towards a desired result. Thus it seems to be a good idea to bring both clans together, encouraging abstract theorists to work increasingly on concrete applications, and practitioners to think more about the secrets of (i.e. the real reasons for) their successes.

In addition to "good scientific practice" efficiently bridging the gap between the conceptual and the observational, I am absolutely in favour of studies excavating the personal, organizational, and social factors that help make progress. What are the crucial personal traits of the successful researcher? How do innovative scientists reach their conclusions? Since people are working together there are social processes going on. For example, as in most other parts of society, science is organized hierarchically, and interests and power have a certain role to play. Thus it is surely important to understand which organizational forms are (un)favourable to science's goals. A prominent recent example is the peer review process. Instead of believing that it works the way it should or criticizing it on principle terms, it is surely better to study its merits and shortcomings scientifically. There is a good chance that this way of proceeding creates both evidence and insight on which specific measures may subsequently build. And if we knew better how useful science could be accessed and encouraged, quite a few dollars might be saved. In a nutshell, empirical and theoretical research on science that helps us to understand how science really works and how these processes could be improved is to be welcomed.

There is no science of the sciences – just some philosophy of science

This said, I am somewhat sceptical with respect to contemporary "mainstream" science studies. It is no coincidence that, to date, the field that is supposed to be the *science* of the sciences is called the *philosophy* of science. In other words, it is rather text- and authority-oriented than empirical. A contribution first of all rather belongs to some "school", i.e., it follows a certain intellectual tradition, instead of starting with a particular problem or putting facts first.

For decades, the field has thus seen a fair amount of dogmatism; at times it has even been the battlefield of rival ideologies: Deductionists are convinced that Popper discovered the "logic of research" in the early 1930s. Consistently, to this day, they think that the modus tollens is science's crucial mechanism; they idolize falsification and denunciate induction. Choosing a completely different starting point some twenty years later, Kuhn realized that science does not always evolve gradually. Rather, "paradigms" come and go, i.e., the way we explain certain facts may alter strongly, if not dramatically. Since such frameworks are only partially compatible, the shift from one of them to another inevitably has an irrational element to it.

Not surprisingly, this has proved to be an excellent starting point for much post-modern theory of science. Over the years, much has been said about the researcher's personality, in particular his/her attitudes, gender and affiliation. In addition, the "strong programme" has given social factors a leading, if not the dominant, role. In a still famous quote, Collins (1981) said that "[the] natural world has a small or non-existent role in the construction of scientific knowledge". Others followed suit, making their concern or some particular feature they thought to be important the crucial property of science. That way science has been compared to all kinds of social endeavours, such as politics, art,

philosophy, religion, culture, etc. And since science - not just via technology - gives us power, which can be abused, it has also been criticized intensely.

Thanks to these efforts there is now some indication that other factors than just empirical evidence and rational thinking (in particular in the guise of quantitative methodology and mathematical reasoning), influence science's course. Yet the problem remains as to how much they do so. Are these external factors crucial or just circumferential?

Successful science echoing Galileo

My claim is that, despite all similarities to almost any other kind of human endeavour, these analogies are far less important than the special features of science, i.e., its empirical-experimental base (facts!) and its thorough, rational-quantitative methodology. I suppose that many, if not most, scientists would agree that, first, good science has to be strongly oriented towards reality. Real-world data must have a major impact for the simple reason that if this were not the case, scientific theories and the exceedingly non-trivial technologies based on them would not work. Just consider Hume (1748): "None but a fool or madman will ever pretend to dispute the authority of experience."

Second, rational, strict and subject-oriented arguments must also be important, with discussions focusing on topics, not persons. Why? If this were different, science would (and at times did) degenerate since speculation, unfocused thought, random debate, unsound argument, contradictions, political preference, prejudice etc. are no substitute for precise measurements, quantitative arguments and logically sound contributions. Hilbert (1930) concluded: "The instrument that mediates between theory and practice, between thought and observation, is mathematics; it builds the bridge and makes it stronger and stronger."

It is quite obvious that these are – and one could almost write "must be" - the major reasons for the enormous success of science; why we see obvious progress in this area every year. And, of course, the intellectual godfather to all this is Galileo Galilei (1564–1642), who was the first to emphasize the outstanding roles of experimental methodology and formal reasoning. To cut a long story short, science does not live by untested ideas, ideology and extremism. Quite the reverse: it is our perpetual wrestling with hard but also evasive facts and our tenacious conceptual-theoretical efforts both aiming at understanding what is "really going on" that are crucial.

This should also hold for science's theory, i.e. the science of science. Alas, it is at least a pity if not a scandal that the dominant paradigms of science are rather intellectual fashions (some of them rather eccentric), going in and out of style than a well-founded theory. For a nice overview see Koertge (1998).

Where we stand (1)

Why is the theory/philosophy of science doing so badly? I think that one main reason is straightforward and was given by Kempthorne (1971) long ago: "It is quite fantastic to me how individuals can try to develop theories of science and knowledge without doing science, like someone who works on the foundations of mathematics without actually doing any mathematics.'' Weinberg

(1995), like Anderson and Laughlin another physicist and winner of the Nobel prize, adds: “We learn about the philosophy of science by doing science, not the other way around.” For the same reason, i.e., for their competence, I recommend the writings of the famous Vienna Circle (in particular Schlick, Carnap, Feigl, Gödel, Hahn, and Kraft), the less known Berlin based Society of Empirical Philosophy (in particular Reichenbach and Hempel), and their down-to-earth successors, like M. Bunge, M. Gardner, I. Hacking and E. Nagel.

It is no coincidence, I suppose, that most of these authors had some thorough training in at least one science. This was still true for philosophers Kuhn, Popper, Lakatos, and Feyerabend, but not for Collins, Lacan, Latour, and Rorty, let alone Derrida. Trained in a different way and having an emphasis on sociology, philosophy or the humanities, it is no surprise that the contributions of the latter group and their followers are in the best cases well-written, thoughtful and original. However, Sokal(‘s hoax) has shown quite impressively that, all too often, contemporary writers are simply incompetent when it comes to scientific questions. So, unfortunately, texts written by authors that do not have a deeper understanding of scientific work tend to become speculative, partisan and extreme, mainly referring to one another. Thus they may rapidly turn into a literary tradition or even into forceful intellectual trends. But yet, still, they are mostly irrelevant, plainly wrong or even misleading, in particular if they miss the main points (quite often formulated in mathematical language).

In addition, “one significant change has taken place since C.P. Snow's time: while humanist intellectuals' ignorance about (for example) mass and acceleration remains substantially unchanged, nowadays a significant minority of humanist intellectuals feels entitled to pontificate on these subjects in spite of their ignorance (perhaps trusting that their readers will be equally ignorant)” (Sokal and Bricmont 1998). In other words, the crisis of science’s philosophy is twofold. First, personnel: inadequately trained persons dominate the subject, whereas scientists working in the field constitute a small minority. Second, this overall lack of competence does not foreclose publication, so that, in the worst case, works of fiction about elevated topics become reiterated “fashionable nonsense”.

Where we stand (2)

At this point some readers may have the impression that my criticism is overly harsh. Being “moderate” they might suppose to agree upon the fact that there is a fair amount of idle talk and papers full of worm-holes. Philosophers as well as scientists have contributed a fair amount of gibberish. However, as long as some field remains intact – shouldn’t one come to terms with its rough edges?

I would be happy to agree. Nevertheless my impression is that the field of the philosophy of science isn’t basically healthy but sick. Here are a few of the symptoms:

1. The natural and the social sciences are “children” of philosophy. That is, these special fields were started in philosophy, matured, and finally became independent areas of research. The philosophy of science is an exception: In the first half of the last century it gained some independence and moved towards a science of the sciences. Today, however, it is “back to mother” philosophy, the philosophy of science being a part of theoretical philosophy.

2. At least in the German-speaking world, the Vienna circle never really became popular. Stegmüller and his disciples, who adopted some of the main ideas, did not succeed either.
3. Instead, (social) constructivism is the leading paradigm in the social sciences, as is relativism in the humanities. In other words the importance of external facts and observation is played down systematically and own contributions are emphasized.
4. "True" and "false" seem to be hopelessly out of date, and, very similar, "better" and "worse". You may describe an organization, some habit, tradition or culture in every detail but beware of weighing! Otherwise you are likely to be called a revisionist, a chauvinist, a racist, a colonialist, etc. Of course, it is wise to be tolerant and cautious with judgement. However, without an educated opinion, crucial ideas like those of the enlightenment (human rights, democracy, emancipation, etc. - you name them) quickly become a matter of taste. Much the same so with logic, rational thinking, facts, knowledge, discoveries and progress. If they all become dispensable, science is just one of many routes towards understanding the world, and it "prevails not because of its comparative merits but because the show has been rigged in its favour" (Feyerabend 1978).
5. Imprecise qualitative methods have become popular (see my separate remarks on this topic), and at the same time the results of traditional quantitative science are disputed. As an example take Dieter E. Zimmer's recent book on intelligence. (Zimmer is a leading German scientific journalist. See "http://dezimmer.net/") Though he states right away that there is a strong consensus about intelligence's structure and its heritability, the title of his book ends with a question mark: "Is intelligence hereditary?" Moreover, he says that just a handful of researchers are still working on intelligence and that many politicians (if not parties) simply ignore scientific evidence that has accumulated. Despite all the facts, intelligence must not be hereditary.
6. Quite fittingly, "grand theories" have remained popular. Like psychoanalysis, such theories are broad, rather unspecific and speculative. Take Luhmann's topical "system theory". Of course, in some sense, it is correct: almost everything can be regarded as a system, an observer often has some influence on the observed, and since social reality is at least to some degree shaped by us there may not be "Newtonian laws" governing our behaviour. However, all these claims are vague and very difficult to access experimentally. So on the one hand words beget words, but on the other hand evidence does not accrue. For example, does it really constitute progress or isn't it just trivial to state, as Luhmann did, that philosophers of science are "second-order observers", studying the work of scientists who are "first-order observers" of the world?
7. Thus, looking at contemporary curricula or typical educational books, eclecticism is the rule and not the exception. One finds a mixture of methods together with a blend of theories. If a "critical" attitude is combined with relativistic arguments the straightforward outcome will be a mix of science and ideology, well-meaning belief and politics, "gender studies" being a prototype example. (I know that the last sentence is politically incorrect. But think about it: isn't such a "silencing strategy" a very comfortable way to escape criticism?) Even worse, it has become rather difficult to find an explicit and outspoken scientific attitude making no compromise (see Gauch 2012). One seems to have to consult outstanding natural scientists (like Feynman, Weinberg or Sokal) to find it.

Where we stand (3)

You may say that this is just the unfortunate state of affairs in central Europe. Haven't the members of the Vienna circle and their associates made it to the English-speaking world, in particular Great Britain and North America? Isn't there the renowned London School of Economics (LSE) where Popper taught, the Minnesota Center for Philosophy of Science (Feigl's legacy, editing the most important journal in the field), Berkeley's department of philosophy established by Reichenbach, and the influential Stanford school organzing the Online Encyclopedia of Philosophy? Outstanding scholars like Suppes, Nagel, Putnam and Quine – to name but few – immediately come to mind.

With all due respect, however, a closer look reveals that, nowadays, philosophers are mainly talking to each other, their discussions often being based on old-fashioned scientific input: Contemporary empiricist philosophy of mathematics is far beyond professional mathematicians' limit of tolerance, and so is Lorenzen's "protophysics" for physicists. Despite its name and tremendous mathematical effort Krantz', Luce', Suppes' and Tversky's "measurement theory" is hardly ever applied. The foundations of statistics are dealt with in Mayo's 1996 work, in which she mainly echoes the Bayesian-frequentist dispute dating from some twenty years previously while also adding a partisan "error philosophy". Gillies (2000) discusses philosophical theories of probability thereby overlooking the twin concept of information. And though they cite Li's and Vitányi's breathtaking book on Kolmogorov complexity, Hájek and Hall (2002) still puzzle over the term "simple".

This list could be greatly extended: Although leading analytical philosophers agree that the problem of induction is of fundamental importance, they seem to have overlooked Solomonoff's general solution, already formulated in the 1960s. N. Cartwright hunts causes, but it is J. Pearl's seminal work that catches them. Or just compare "the Oxford Handbook of Free Will", focusing on the principal question whether/how free will could be reconciled with determinism, with "Are We Free?" also published by Oxford University Press, where psychologists approach the same problem but achieve real progress upon focussing on scientific methods and results.

One would hope that at least the modern classics could be excluded from this list. However, "in the light of logic" (Feferman 1998) Lakatos' "proofs and refutations" evaporate. Weinberg (1995) argues that Kuhn's main theses are "wormwood" and Jaynes' (2003) remarks on Popper are even less cordial. Finally, it was the philosopher D. Stove who, some thirty years ago, took pains to show how his colleagues Popper, Lakatos, Kuhn and Feyerabend had paved the way to "scientific irrationalism".

Perhaps contemporary "philosophy of science is about as useful to scientists as ornithology is to birds" (a quote attributed to Feynman). I would not go that far but all in all I am afraid to say that it would constitute progress if we were – with respect to studying science – back in 1914, when B. Russell wrote: "The one and only condition, I believe, which is necessary in order to secure for philosophy in the near future an achievement surpassing all that has hitherto been accomplished by philosophers, is the creation of a school of men with scientific training and philosophical interests, unhampered by the traditions of the past, and not misled by the literary methods of those who copy the ancients in all except their merits."

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(for additional literature see "Statistik im Forschungsprozess" and "Die Macht der Daten")

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