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How to Learn Lessons and to Beat the Challenges or Rats Learn from Experience, Why Don't We ? by Alfred Uhl, (LBISucht, Vienna, Austria)

The German philosopher Carl Friedrich von Weizsäcker (1978) said on the relationship between philosophy and the so-called positive sciences: "Philosophy formulates the questions which when not asked constitute the conditions of success of the scientific process!" This sharp-witted interpretation holds true for those large areas of the natural sciences that form the basis for technical applications – i.e. areas where grave logical and conceptual errors instantly produce failure. The situation is very different though in the human and social sciences, where a diversity of contradicting scientific claims coexist without any clear-cut strategies to unequivocally judge their appropriateness. Under such conditions, philosophy – at least philosophy of science and basic research methodology – has an important role to play. Here logical reasoning to detect intrinsic contradictions in the current body of science as well as using and interpreting formal models correctly is not only important in exploratory research endeavours but equally essential in the context of confirmation.

The human and social sciences are characterised by severe economic, technological and ontological research limits, whereby the term "economic research limits" refers to the fact that interesting research projects are far too expensive to have any chance of realisation, the term "technological research limits" points to the fact that some research questions cannot be tackled since technology is not yet adequately developed and the term "ontological research limits" refers to limitations imposed by reality itself. Due to these limits the human and social sciences do not impress as a permanently growing body of cumulative evidence but resemble a continuously changing puzzle, where missing pieces have to be substituted by logic, analogy and common sense, where pieces have to be rearranged if new pieces turn up, where commonly more than one interpretation is justified and where well founded conclusions are the exception rather than the rule.

In some areas of human and social sciences, randomised controlled trials (rcts) are highly useful procedures but this "gold standard" is not feasible in most research areas. Therefore it makes little sense to rigidly insist on "evidence based research" in the sense of well controlled experiments and depreciate all other forms of research endeavours. The term "evidence based" only makes sense if it is understood in the much broader sense to which Richard Hartnoll defined in the background paper to this conference, i.e. as "a step-by-step process of building evidence through observation, developing theory, testing hypotheses and crossing information". Aiming for as much empirical evidence as possible is essential for a researcher, but equally important is interpreting existing evidence rationally and dealing sensibly with missing evidence.

The intrinsic problems of scientific research are a very important aspect for discussion. The second important aspect is how research is organised practically. The basic epistemological

and methodological problems on one side and the organisational and practical problems on the other side are highly interrelated, but they should be kept apart in the discussion. I will now move to the organisational side of the problem. Adequate research can only evolve if the organisational context is set up adequately.

The wider understanding of “evidence based research” defined above, demands for specific background conditions. The process must be led by experienced senior researchers who are truly at home in their field, who aware of the blind spots in the research puzzle and who know the limits of research. The necessary competence can only evolve if the researchers can remain in their field for many years, if they have time to reflect and if they have a chance to exchange routinely with colleagues. What we need are “centres of excellence” respectively “think tanks”. This means research should be organised on a long-term basis largely independent of every-day politics with reliable core funding. It is counterproductive to create institutions depending on short-time projects with a highly fluctuating staff and no core funding, who can only survive if they chase desperately for projects and have to accept any project regardless if the topic fits into their overall programme or not. Quality in research depends highly on experience. It is naïve to believe that students who have just finished their university courses and were taught basic research tools can produce quality in research without being intensively supervised by experienced senior scientists.

In the last decade the research situation in Europe has continuously moved away from the above defined ideal, driven by economic constraints and by the emerging idea that the optimal research quality can be guaranteed by formalistic tendering rules, going for the cheapest bid and by funding individual projects rather than core funding institutions carrying out long-term research programmes. Due to limited time, I cannot cover this matter systematically but only mention some characteristic examples and arguments.

Realism in cost calculations: *If an architect has calculated the costs to build a certain house, if the customer then asks him to build this very house for half of the price and if the architect accepts the task anyway, we should be highly sceptical. Either the architect wanted to cheat his customer initially by overestimating the costs dramatically, or he now plans to cheat the customer by not fulfilling the contract adequately, or he is just an incapable businessman who may go bankrupt before the house is finished. For good reasons unrealistically low prices should make customers as suspicious as an unrealistically high prices, but nevertheless, in the research funding world decision-makers commonly try to cut the project prices dramatically and still expect the same outcomes. The loss in quality to be expected very likely exceeds the reduction in costs by far. Somewhat different in procedure but similar in outcome is to tender for research projects publicly and to define an objective decision algorithm based on tender content and the price of the bid only, since such conditions force all competitors to offer unrealistically low prices which do not allow to do the job well. This is particularly true in large international projects, where a large fraction of the planned costs is reserved for travelling and meeting expenses, and where a dramatic budget cut means that no money is left for the essential research work. I will deal with this issue specifically later.*

Long term perspective: *If the manager of a research institute cannot plan his research projects on a long-term basis, since customers and funders do not want to bind themselves too early, he has to potentially overbook his staff by excessively tendering for projects. If he*

acquires many more projects than his staff can handle, he has to instantly employ additional personnel to fulfil the project. Under circumstances where there is hardly time to thoroughly select new employees, particularly if so many newcomers are contracted that the capacity of the senior staff to supervise them adequately is overdrawn bad quality is inevitable. If on the other hand the manager fails to acquire enough projects to finance his staff he has to release personnel, even though he is aware that building up the same level of expertise in newcomers will take years again and that therefore the quality of future projects will suffer. On a purely operational level it makes sense for funders to remain flexible by making vague commitments and to save money through not investing in core funding, but if output quality is the issue, such a strategy is highly counterproductive. Research institutes need a relatively continuous staff and at least some core funding independent of research projects to develop the competence they need to contribute sensibly to science.

Objective rules for projects to accept: There are several examples where funders favour friends and examples where publicly funded researchers produce inadequate quality and charge too much. One approach to prevent such problems is to formulate objective funding rules and to demand that the cheapest one out of comparable competing tenders is chosen. The problem here is that written tenders – even if they are professionally formulated and very detailed – hardly allow for anticipation of the quality of future results and that the cheapest bid is often highly unrealistic. Such mechanical funding procedures force decision makers to make sub-optimal decisions. We should be realistic concerning quality indicators: the most relevant indicator for good quality in research is high quality of previous projects, that the experienced staff is not overbooked with other projects, that there is sufficient funding to take all necessary research steps adequately and that the project staff expects that good results will produce further projects.

Evaluation: Common sense tells us, that quality should be controlled and that demanding an evaluation of projects makes sense. There can be no doubt that documentations and independent experts judging the results make sense as well, but we should also be realistic and see that the categorical demand to evaluate projects often leads only to pseudo-evaluations. There are several reasons for this. To name just a few:

(1) Evaluations, like any other research endeavours require adequate funding and the amount that would be necessary to arrive at sensible answers may exceed the amount that is justified in relationship to the total project sum.

(2) Often the persons demanding and financing evaluations expect outcomes that cannot be achieved realistically and many evaluators are reluctant to inform them about this fact, to avoid losing the project.

(3) More and more evaluation projects are tendered throughout Europe causing evaluators, who live very far very away from the project location, to become involved. Consequently they often have to rely primarily on questionnaire data without being able to exchange adequately with the project staff.

(4) Commonly all stakeholders involved – even if the evaluation is organised externally – have a common interest in a “positive result”. The person or institution carrying out a project is naturally interested in positive results, most financiers, after the money has already been spent, also prefer a positive outcome to justify their spending and any evaluator interested in similar evaluation projects in the future has a lot of motivation to produce results in order to make his partners happy.

The desirable notion of developing well planned and methodologically adequate evaluations thus often deteriorates to a ridiculous ritual of pseudo-evaluation. One could speak of “evalopathy” as a new form of mental disease spreading through the scientific community. A development producing evaluation output which is not good enough to learn anything from, but nevertheless drawing on scarce resources from more sensible work (Uhl, 2000b).

National and EU interests are not research focused: *The national governments pay money to the EU administration and they get back some of the funds via EU-Research projects. Participation in EU-projects requires national co-financing. As a result the aim behind funding at national level moves away from the scientific goal of supporting good quality research towards the economic goal of getting as much money back from Brussels as possible. The EU on the other hand wants to create international networks and support international projects to create a European identity and to interlink European research. This again is not a scientific but a political goal. What commonly happens as a result is that small groups of competent researchers interested in a certain research topic form a core group, search for partners through various contacts in the other EU-States and then tender for a large EU-research project. Most of the partners recruited this way are somehow interested but it is foreseeable right from the start, that they will neither contribute much to the project nor be part of a lasting research network after the very project is over. It would be much preferable in terms of costs and outcome if only the core group started the project, but this is not in line with the funding ideas of the EU. Since the initially calculated project sum is usually reduced dramatically, hardly any money remains for essential research work and everything goes into project administration and financing travelling and hotel accommodations for international meetings. In some cases – due to large amounts of extra national funding or very involved individuals – the results of the project are nevertheless worthwhile, but commonly an honest comparison of the funds invested with the results produced gives a catastrophic picture. Here too, much more realism with regards to funding, less bureaucracy and more flexibility in topics would be a great advantage for the advancement of research.*

To expect unpaid work: *Bodies like WHO, EMCDDA, the Pompidou Group, the European Commission and some national research centres commonly initiate projects where a key researcher is paid to collect data from other international researchers. The latter not being paid for their work at all. If neither the bodies who started the project nor any national funders cover the emerging costs for the work of the national expert, it is foreseeable that the data quality will be extremely bad and that the whole project is very likely not worth the effort. I have repeatedly been asked to fill in extensive questionnaires from different international projects and if I usually stated that I could not invest very much working time without any funding, I was confronted with some incredible reactions. To just give an example: When asked to supply the average wine, beer and spirits prices in Austria for a very respectable international data base, I responded that I would try to collect the data if possible from the central census bureau and was confronted by the project leader with the following: “Don’t bother, just go to the next Supermarket, chose an average wine, beer or spirits product and write down the price!”*

I will stop giving examples now and try to arrive at a conclusion. There is a chance to improve the research situation if we decide to clearly mention the existing obstacles and criticise problematic developments. As long as we imply to be able to do impossible things we will be asked to do so, and eventually only work in impossible missions. If we express the inherent uncertainties encountered in daily research, put the finger on weak spots, reject tasks that are not feasible because of economic and/or epistemological restraints, are precise in terminology and do not avoid methodological problems, we may risk disappointing potential customers, but in the long run improve our profession and contribute to a sound foundation for a good and lasting reputation. Some researchers, frustrated by the complexity of their task and tempted by the need to tender for projects for economic reasons, may be tempted to ignore the problems or sympathise with opportunistic strategies - two strategies

that I previously (Uhl, 2002a) labelled “deliberate ignorance” respectively “cynical opportunisms”. But we should rigorously reject those notions. I am convinced we may be quite optimistic in spite of the difficulties we encounter. If we really understand our profession, despite all its limitations, there are numerous promising approaches, sensible options and solutions available. We have to convince the public and the public financiers, that research can only flourish under certain background conditions and that worth while results from research investments will only happen if the funding strategies and expectations are changed dramatically. Explaining to outsiders and financiers what is feasible and sensible in research is not equivalent to cutting the branch on which we researchers sit, but constitutes an investment to make these branches stronger, enabling us as researchers to do a useful and fulfilling work that at the same time serves public interests.

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