

Editorial

On public opinion, a scientist's mental state, and other trivialities

Can it actually be that nowadays no one does research to satisfy personal inclination, that the contemplative *homo ludens* no longer exists, that modern researchers never find themselves in awe at unexpected revelations of nature's secrets? Just think back: surely it was mainly his personal inclination that enabled M. F. Ledermüller in 1761, by the pure power of observation, to bury the ancient dogma of the *spiritus animalis* (although he didn't take full advantage of his opportunity). Think of L. Galvani, who at first was merely playing when he began to demonstrate (not prove!) the existence of animal electricity in 1791, and of W. C. Röntgen, who in 1895 marvelled at X-rays and their ability to penetrate living tissue without (as it then appeared) doing any damage.

Of course, the constantly accelerating progress in the biosciences that ensued was brought about only by those who knew how inclination, the play instinct and the ability to marvel could effectively be supplemented with meticulously planned experiments leading to logically compelling conclusions. For instance, the history of science has long since handed down a decision on the vehement altercations between the first true giant of electrophysiology in the 19th century, Emil Dubois-Reymond, and the last great *homo ludens* of the early period of exact biosciences, Carlo Matteucci. But it was also on account of radical revisions of social policy that simple inclination, enjoyment of play and the capacity for amazement were ever more rarely sufficient for successful research, and that logically justifiable planning of research took their place – until finally we were left with the succinct appraisal: “each and any scientist ought to be ready, at any time, to doubt any given scientific statement” (G. Gale 1979). So it is only a natural consequence when the source of your financial support asks bluntly: “What is your research actually for? What's the point of it?” Because everyone knows that funds are limited everywhere, and that research projects are always becoming more expensive and also more urgent, especially in the biosciences.

And in the view of the public and hence of most funding agencies, the urgency of a research project is basically equivalent to its practical relevance. Don't complain about it, my colleagues in basic research, but see whether there is something in your own projects that might be of practical significance! That is how you can establish your research activities as worth of being supported in the eyes of your sponsors. After all, these activities not only put bread on the table but also put you in a position to experience, perhaps, at least a bit of the non-material satisfaction

enjoyed by the scientists of days long past – something not measurable by impact factor and citation index or patronising recognition from the high-and-mighty in your field or by the even better contribution to your privy purse when hierarchical hurdles are overcome, or the other side-benefits in the life of a scientist.

Furthermore, do give your younger coworkers a chance to become acquainted with the possibilities for quiet pleasure in research, which is incommensurate with financial reward! Then we need not be quite so apprehensive about their future scientific vocation (in distinction to career) as we sometimes are for other reasons.

GENERAL PHYSIOLOGY AND BIOPHYSICS is well known as a journal for basic research in biology, and so from the outset has had the duty to include research unconstrained by practical considerations. Therefore we editors, now as always, by no means consider practical relevance to be a prerequisite for the publication of submitted manuscripts, although it can certainly sometimes be an informative cosmetic addition for our readers. Of course, our readers are also told nothing about the author's scientific mental state – which is just as well!

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