

**TAXONOMY IN CHANGING WORLD –
THE ENDS AND THE MEANS
(COMMENTS TO AGNARSSON & KUNTNER, 2007)**

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That taxonomy is in a very deep crisis is evident to anybody having something to do with this discipline: both the symptoms and the final diagnosis have been well known and widely disputed, and Agnarsson & Kuntner (2007) aptly identify [some of] them: “*taxonomic experience is rarely required, or even relevant, when it comes to securing a job*” “*most top-ranking ... journals do not consider taxonomic revisions, and only allow species descriptions in exceptional cases of certain high-profile fossils and mammals*”, “*some lower ranking journals reject taxonomic descriptions unless in a paper on a broader subject*”, “*journals focusing on taxonomy typically have low measured impact ... [because] taxonomic descriptions are – not necessarily by fact ..., but by convention – low-impact scientific publications, barring those of newly described bird species, large mammals, or certain fossils*” – and thus, despite the alarming fact that “*species are disappearing at an ever increasing rate*”, “*species discovery and description – taxonomy – is facing a crisis*”. The ways to overcome the crisis are also frequently proposed, but unfortunately what overwhelming majority of them have to offer amounts to implementation of some fashionable (“modern”) technical gadgets [internet (Erwin & Johnson, 2000), DNA “barcoding” (Hebert & Gregory, 2005)] or procedural shortcuts like parataxonomy (Oliver & Beattie, 1993) and “PhyloCode” (Pickett, 2005). It is not my aim to evaluate these earlier ideas here (some of them have been commented upon, also by me – see e.g. Holyński, 2001, 2008 – elsewhere), I wish only to remark on the suggestions of Agnarsson & Kuntner (2007).

The most extensively analysed by them is the problem of “*low measured impact*”, as one of the causes of the increasing aversion to taxonomy among “decision-makers” (funding agencies, institution directors, journal editors etc.). The measured impact is indeed inherently low in case of taxonomy (and not only of taxonomy), and this certainly negatively influences the attractiveness – and consequently funds, options for publication, availability of jobs, etc. – of the discipline, but this is only one aspect of much wider and much deeper problem. First of all, “impact factors”, “citation indexes” and similar “parameters” are grossly misconceived as measures of the value of publication – and not only, as frequently argued, for some particular disciplines [see Krell (2000, 2002, 2006), Valdecasas et al. (2000), Werner (2006), etc. for biodiversity studies, or e.g. Wiśniewski (2006) for humanities], but for

most if not all (even applied, though the consequences are much more dangerous for “basic” research) branches of science in general. They are indeed “objective”, easy to computerize etc., what makes them likable to “science managers”, but these qualities are of no more than secondary importance – otherwise we could evaluate scientists and their publications according to e.g. stature or date of birth, which are still more objective and manageable... Objectivity is of any importance if – **only** if! – the measure is relevant, and the problem with the “*measured impact*” is just its negligible relation to what it is claimed to measure: to the **real** scientific value of the publication or even to its influence on other scientists’ work! In this situation, Agnarsson & Kuntner’s (2007) suggestions how to improve the impact factors of taxonomic publications, though obviously rational in themselves, are doomed to remain insignificant cosmetics with but minor effect on the situation of taxonomy and taxonomists.

But the supremacy of impact factors and other elements of “parametric evaluation” of scientists and their work is itself also but one of the manifestations of the general decadence of science – and not only science... The overwhelming practice to evaluate everything in terms of profit (or success in “rat-race”) has increasingly detached large domains of human activity – economy as well as e.g. sport or art – from their essence and declared purpose: not the real achievement counts, but only marketing tricks, swindles, unfair competition. And this is now the preferred – in fact, coerced! – style of doing science: this is why scientists, journals, scientific institutions are evaluated not according to the true scientific value of their publications but according to formal “impact factors”, in fact according to the position (“quartile”) of the journals in which they have been published among the (also formally) selected set of periodicals (impact factors being based only on citations in journals from some “Mutual Adoration Society” membership-list, and at that excluding just those citations most closely related to the scientific value of the publication: the **true** impact of a paper cited 3, 5, 10 or 50 years after its appearance is (at least on the average) much greater than of that never referred to after one or two years! The inevitable consequence is just the situation that also in science marketing juggles and – to say it mildly... – not quite honest practices dominate over fruitful cooperation and truly valuable results: divide one serious study into ten “contributions to the knowledge of...” – that they will be less accessible and more difficult to use, no problem, but you will have ten publications instead of one; add the names of ten colleagues as “co-authors” of your paper, then they will add you as co-author of theirs – a fraud? well, but your publication list will expand again; perform a phylogenetic analysis of 10 species based on two molecular sequences taken from one specimen each – of course morphological characters checked on numerous specimens of all 100 known species would be much more informative, but your DNA-based grant application looks more “modern” and has much more chance to be accepted; publish your paper in a journal from the “top quartile” – it is

very expensive and so available only in few libraries, its publication cycle is very long, adjustment to editorial regulations requires detrimental modifications, few if any of your professional colleagues will find your paper in it, so many other journals would be much more appropriate for this kind of publication, O.K., but the “impact factor” will be high; etc., etc., etc. [if Darwin and his followers would have adopted this style of work, we would certainly have million of papers on minor specific questions but evolution would have probably still remained a controversial fancy concept, disdained by “serious” scientists but hotly debated by laymen on theological or philosophical grounds...]. This general attitude is the **real** cause of the troubles in taxonomy (and other disciplines not immediately and predictably profit-yielding nor impressive to laymen like cosmology or dinosaur palaeontology), and until this is not radically changed no (however reasonable as such) improvements in calculation of impact factors or any other formal index will significantly help!

While modifications of impact factor or citation customs would be only ineffective, the Agnarsson & Kuntner’s (2007) “advices for future taxonomists” are plainly **destructive**, accepting and in fact promoting just the formalistic and primitively utilitarian attitude described above: scientific value of your work does not matter, accuracy of your results does not matter, appropriateness of applied method to your particular problem does not matter, the only things which matter are “requirements of the job market” and your “career”! So you must develop a series of fashionable “modern” panacea skills “good for anybody and serving any purpose” rather than selecting them according to your predispositions and to what you consider necessary for optimal realization of the planned kind of research on the group of organisms you have chosen as your speciality! Indeed, you should not be specialist at all – or, more exactly (though in fact meaning the same...) you should be specialist in everything from sound knowledge of the studied animals (or perhaps **just this** is not needed? – sequencing-machine, computer programs, &c. can work without any such information...) to molecular methods and interactive databasing; after all, our stone-age ancestors also did everything themselves: performed magic celebrations and built huts, prepared stone-axes and hunted mammoths, skinned their prey and made clothes, so the XXI-century scientist should do the same: this is required by the job market, this is priced by decision-makers and funding-providers, so this most efficiently promotes your career...

The question is: **what taxonomists are for?** Is their prime duty to increase the ranking position of their institutions, impact factors of journals, and own score in the rat-race? – if so, then they should indeed be trained and, then, work according to Agnarsson & Kuntner (2007) advices: carefully watch the job market, funding policies, editorial preferences etc. and accordingly plan their activity – develop “modern” skills, choose “important” research projects, apply fashionable methods, publish each minor piece of results in separate paper (in mutual

“coauthorship” with several colleagues), send it to “high impact” journals, etc. So trained, and accustomed to this style, they will quickly ascend up the “monkey ladder” and soon, as influential professors, editors, directors or ministers will themselves similarly shape the job market, funding policies, editorial preferences... But, as Nobel Prize winner physicist Heinrich Rohrer (2006b) warns, “*competition is a cheap measure of whatever performance. ‘Better’ does not even mean ‘good’ and science is too serious a matter for racing contests*”, so if, however, the pivotal task of taxonomists (like other scientists) is to perform sound **scientific** research [“*science: human activity aiming at methodical study of the world, and description of the results of that study in the framework of a coherent system*” – according to the encyclopaedic definition], what for taxonomy means adequate discovery, description and natural classification of the elements of biodiversity, then Agnarsson & Kuntner’s (2007) postulates should be reversed: rather than to trim the qualifications and work of taxonomists according to Procrustean template of “requirements of the job market” etc., the job market, funding policies and editorial preferences should be so adjusted as to preferentially support the best taxonomists [some recent publications – e.g. Nentwig et al., 2007 – point somewhat in this direction, but generally in the present absurd situation “*the talents needed to perform good scientific work are radically different from those helpful in raising funds*” (Selve, 1980), although no much thought is needed to realize that the safest way to receive funds **should be** just good scientific work, and that the best scientists (taxonomists being no exception) are certainly not those “flexible” opportunists eager to subordinate their research plans and ways of their realization to perspectives of easy career! Scientists should be “*paid for what they do, and not ... do what they are paid for*” (Rohrer, 2006b)]!

As to the “*synthesising knowledge not merely describing species*”, “*integrating descriptive taxonomy with other biological fields such as phylogenies, biodiversity conservation, molecular biology, ecology, ethology and biogeography*” and “*embracing and acquiring skills in the use of new tools and technologies*”, these are very good ideas and I would not say a bad word on them as long (but **only as** long) as they are followed according to the specific talents and interests **of particular taxonomist** and **his/her** evaluation of needs **of particular project**, not to schematic – ruled by short-sighted expectation of immediate profit or simply snobistic “latest vogue” – “requirements of the job market”! Contrary to Agnarsson & Kuntner’s (2007) suggestion, the time of “experts in everything” is long over, the contemporary science is by far too “voluminous” and complex to realistically expect of anybody to acquire **true** skill and knowledge on more than few (rather narrow) fields; “multidisciplinary expert” is in fact almost invariably multidisciplinary fumbler! Thus, each scientist must carefully select **which** skills to train, **which** knowledge to acquire, **which** methods to apply, and the selection should be based on careful evaluation of his/her

abilities, interests, and particular needs of his/her actual and planned projects! Important is not whether a procedure or theoretical approach is new or old, “modern” or “traditional”, sophisticated or simple, but only whether it is the most appropriate for the project we plan to realize: molecular analysis may be necessary (e.g. in phylogenetical study of organisms poor in good morphological characters, like bacteria, nematodes, or parasitic copepods), may significantly improve the accuracy of the reconstruction (as in birds, mammals, and other taxonomically well known taxa), or may be practically worthless or even confusing (when the target group is very speciose, morphologically well differentiated, with good fossil record and/or museum material unsuitable for DNA extraction) – so a specialist in beetles, snails or brachiopods should think twice before he/she decides to enroll for a course of molecular techniques rather than to devote more time and effort to botany, geography, geology, or e.g. to study Chinese! On the other hand, people’s abilities and interests are also widely variable, and the applied methods and even the research projects should be selected accordingly: excellent field-collector need not be a polyglot, “theoretizing” imagination is not necessarily paired with technical skills, observational perceptivity and ability to keep pace with hard- and software armament-*race* are not always characteristics of the same person, etc. In particular, many (especially, but not only, elder) scientists have no talents or “skills” allowing to efficiently work with the technical gadgets currently *en vogue*: *“Ernst was truly a non-technical person; the most sophisticated tool he used was a Dictaphone. ... he did not even know the location of the keys on the keyboard ... Computers were out of question ...”* (Bock, 2007) – these words do not refer to some stupid backward but to the arguably most eminent biologist of XX century, a position which he had achieved, among others, just because his *“belief in himself rested on a realistic assessment of his own strengths and limitations, constraining him ... to stay within his competence. For instance, he decided after many months of preliminary study not to extend his book Animal Species and Evolution to discuss plants, or The Growth of Biological Thought to include physiology and embryology, because he recognized his lack of familiarity with these subjects”* (Diamond, 2007) – although he had evidently much better knowledge concerning e.g. plants than do most taxonomists **really** know of modern phylogenetical or statistical methods they, as required by “job market”..., regularly use *“Are molecular models poor fits to the highly complex datasets compiled by modern systematists? unfortunately, and a bit embarrassingly, we still do not know the answer to this tenth crucial question for the great majority of published datasets”* – Gatesy, 2007). Well, I do not believe that any editor would reject **Ernst MAYR's** paper for having not been submitted “online”, yet such “non-technical” but serious (even if less influential) scientists are many! Of course probably everybody (even Ernst Mayr...) would be able to learn how to use computer or internet, but it would cost him disproportionately much time and effort, what would be

lost (as ecologists say, *free lunch does not exist...*) for his really valuable work: a baker could perhaps produce butter or cheese, but it is certainly better if he concentrates on baking good bread, and similarly the task of a scientist is to do research rather than drudge at technical tricks!

At the end, I must apologize Drs. AGNARSSON & KUNTNER for the slight but – if not admitted – probably awkward “fraud” I have deliberately committed: to make my objections clearer I have “sharpened the contrast” by presenting the **consequences** of possible (and indeed, in my opinion, inevitable) tendentious or inadvertently oversimplified interpretation of their “advice” so as if it were their **intention**. In fact, of course, I do not believe that they really wished to promote unprincipled, in fact dishonest, careeristic attitude to scientific research, though unfortunately their paper can, and certainly sometimes will, be so understood...

To sum up, I hope that young talented scientists will prefer – and **be given the possibility** – to follow the advice of the already quoted Nobel Prize winner physicist: “**Ich wünsche, dass Ihr Euch nicht überrollen lässt von einer Welt gefüllt mit allgegenwärtigen Schlagworten wie kompetent, innovative, zukunftsreich, trans- und interdisziplinär, wettbewerbsfähig, ganzheitlich, nachhaltig, umweltbewusst, global, Kooperation, Programme, Wettbewerb, und was auch immer. Orientiert euch an Euren Fähigkeiten und Überzeugungen und konzentriert Euch auf Euer Kerngeschäft, erstklassige Wissenschaft und Forschung**” [**I wish, that you do not allow yourself to become overridden by the world filled with such ubiquitous slogans as competent, innovative, future-minded, trans- and interdisciplinary, competitive, comprehensive, lasting, ecological, global, cooperation, program, contest, and whatever else. Follow your abilities and convictions and concentrate on your main duty, the first-class science and research**] (Rohrer, 2006a; display in **bold** in original). This is the only efficient solution for a science in crisis (to get out) and for those currently not in crisis (to avoid getting into)!

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