Genus	Vol. **16**(4): 469-502	Wrocław, 28 XII 2005

Philosophy of science from a taxonomist's perspective

ROMAN B. HOŁYŃSKI
PL-05822 Milanówek, ul. Graniczna 35, skr. poczt. 65, POLAND
e-mail: holynski@inetria.pl

ABSTRACT. This paper is devoted to discussion of some aspects of philosophy of taxonomy (understood as a system of thought determining taxonomist's approach to aims and principles of his work) from the viewpoint of their impact on the final results of study. After general remarks on the role of strictly philosophical considerations in scientific practice evaluated are various meanings and applications of stylistical and terminological "exactitude" (with example comments on the terms "phylogenetic", "monophyly" and "speciation"), hierarchy of the purposes of scientific research (esp. the relative primacy of accuracy of results vs. their testability and, by the way, the applicability of Popperian "falsification not verification" dogma to biological hypotheses), the nature and value of commonsense and intuition (with attempt to clarify some common misapprehensions), the limits of desirability and real attainability of formal "rigour" (standardization, "objectivity"), &c. In the last chapter some direct or indirect consequences of the educed conclusions for taxonomic work have been exemplified by their application to two hotly debated problems: the principles of classification and the species concept.

Key words: philosophy of taxonomy, aims of science, exactitude, accuracy, verifiability, intuition, objectivity, principles of classification, species concept.

To Prof. Ernst MAYR,
the founder of modern systematics,
my "personal" – though never personally
met – guide in the Evolutionary-Taxonomic Wonderland

> *"Students doing only practical work, who imprudently afford to listen to philosophers' lectures, leave them with the timid feeling persuaded that their works are logically unfounded, that all scientific laws accepted by them lack substantiation, and that generally they live in the world of gullible illusions. Naturally, having come out to the daylight, they see again that it is not so bad, rules of research prove efficient, bridges stay, eclipses ensue according to calculations, atom bombs explode".*
>
> WILSON (1968)

Dephilosophied philosophy, or what this paper *is not* about?

The main meaning of the word "philosophy" is "*the study of ultimate nature of existence, reality, knowledge and goodness*" (GARMONSWAY 1969), and indeed it has been popular among modern scientists to interpret the natural phenomena in terms of profoundly underlying philosophical concepts; in particular, many publications on plant or animal systematics focus so strongly on ontological or epistemological aspects, that their relation to biological reality is – to say the least – not quite clear. I am biologist, not philosopher; as an obvious (even if seemingly paradoxical in the context of a paper like this…) consequence of this statement I am not interested here in philosophy as defined above: of course, "*ultimate nature of reality*" is ***in principle*** the most fundamental question in scientific research, but – just because of that "fundamentality" – its relation to the observable and interpretable biological phenomena is so remote and indirect that ***in practice*** its adduction in taxonomical (or evolutionary, biogeographical, ecological, *&c.*) discussions may serve to obscure the problems rather than clarify them ["*Much has been written on the philosophical bases of classification ... While this literature is academically interesting and has important implications for essentially simple (but not always easy!) domains of inquiry such as mathematics, its application to essentially vague (i.e., very highly complex) domains such as biological taxonomy seems to be of little practical assistance*" – JOHNSON 1968]. Generally, "fundamental" questions are rarely helpful in solving actual issues in natural sciences: chemists usually do not refer to the properties of quarks in explaining those of acetic acid or mercuric sulphide, relativistic structure of the Universe is notoriously left out of consideration in geological studies, and biologists would also do better concentrating on ***biological*** nature of taxa and leaving their "ontological reality" to philosophers; applying ELDREDGE & CRACRAFT's (1980) phraseology, these are "*different phenomenological levels*" – so different as to render the interconnections between them hopelessly untraceable! Thus, philosophical questions in the above-mentioned sense remain out of my interest and will not be discussed here.

However, the word "philosophy" has also other meanings, one of them being "*any specified system of thought*". In other words, besides "***the*** philosophy" there are various ***particular*** "philosophies", and one kind of them is the philosophy of taxonomy: system of thought determining taxonomist's general approach to aims and basic principles of his work. Indeed, there are in fact many such systems of thought, and it is not my intent to review or compare them: instead, I will present one – ***my*** – philosophy of systematics, quoting others only when necessary to illustrate my points. But even so defined subject of this paper must be further restricted: it is not intended as anything like comprehensive "treatise", I wish merely to discuss some questions of – in my opinion – special importance. The respective criteria, again, issue from my being a biologist, not philosopher or anything else: I am interested in "scientological" considerations only as far, as they influence "practical" results; so, I will treat of only those aspects of my "philosophy of systematics" which are not identical to those generally accepted (this restriction is rather a "formal" one, as there is hardly any "generally accepted" statement in the theory of systematics…), and only as far as disparate opinions can – demonstrably or at least conceivably – lead to differences in taxonomical conclusions and/or to difficulties in mutual understanding between my colleagues and me.

One may wonder, what is the sense of publishing one's "own" views on "systematic philosophy"? The purpose is twofold. My initial goal was to provide a general "theoretical" background to my "practical-taxonomic" papers: a great deal of arguments, misapprehensions, and even rancours could be avoided if scientists understand well one another's way of thinking, so every active taxonomist should make his "system of thought on systematics" known to others. Some workers can do it by simply including in the introductory part of their paper[-s] a statement like "I follow the cladistic approach as described by WILEY (1981)"; for others (including me) this is not so easy: although the general framework of my "philosophy" is that of the synthetic ("evolutionary") school, there is no single author whose views I would accept as a whole – in some questions I agree with MAYR, in others with SIMPSON, BOCK, or even DE QUEIROZ, while sometimes my opinions differ from anything published (to my knowledge) heretofore.

On the other hand, my paper is intended as a voice in the general discussion on theory of systematics; as such it does not differ in character from any other similarly motivated publication: each "system of thought", from ARISTOTLE (and his predecessors) to present day, was originally ***one's own*** view – even if its author presented it (explicitly or "between the lines") as "***the*** modern approach", "***the only*** logically coherent set of principles", *&c.*, proclaiming it the Last Word of Science which must be accepted by everybody capable of rational thinking (unfortunately, such claims are often taken for serious by editors of scientific journals, the particular opinion becomes a dogma, and discussion is practically stopped: papers advocating alternative views have limited chances to be published…). I am not persuaded that "modern" is necessarily better than "traditional", so am not particularly interested whether my views are modern or not; on the other hand, though ("by definition": otherwise it would not constitute ***my*** philosophy…) I consider the "system" presented here as better than any of those proposed hitherto, I am also fully aware that this is ***only*** my opinion, which may or may not be shared by anybody else. If my "voice in the discussion" will – as I dare to hope – not be the last one, *i.e.* if the publication of this paper will indeed be followed by open, constructive exchange of arguments, then perhaps I have a chance to convince my opponents (or be convinced by them…).

Some "linguistic" remarks

I am a scientist, so I should – and wish to – express my views exactly; I do not expect much opposition to this statement, and therefore it may seem superfluous. The problem is, however, that when several people say the same, they often say quite different things… In this case, the critical word is "exactly". For some scientists (and especially for some editors) "exact" equals to dry, schematic, monotonous – for them, the best proof of "exact, scientific language" seems to be the reader's yawning at the third sentence and loud snore at the tenth… For others, "exact" means always concretely, precisely defined. I decidedly refuse both interpretations: for me, "exact" expression is such, which maximizes the probability that the reader will have "read precisely what I have written", *i.e.* understand my text ***exactly*** so, as I wished it to be understood. The language of a publication is a tool, designed to transmit the author's thoughts to the readers, so it should be used according to what (and to whom!) the author wishes to communicate; as each author in each publication (and even in each part thereof) wishes to communicate something else (the addressed readers being not always the same, either), elements of language (from words and punctuation-marks to displays of print and genetral structure of the paper) must also be chosen and used differently. Sometimes – *e.g.* in "routine" enumerations of logically independent statements (like in taxonomic descriptions) – short, simple sentences are acceptable (even articles can be omitted); other cases, as those involving subtle argumentation, demand elaborately structured phrases where differentiated punctuation-marks and relation-indicating words ("therefore", "indeed", "nevertheless") play a very important role. On the other hand, unless formalized, schematic form is specially motivated, the use of flexible, fluent "essay-style" is preferable on obvious "psychological" grounds: dry, monotonous text causes weariness and makes it difficult for the reader to concentrate attention on – and, consequently, to properly understand – the author's reasoning; in this sense, according to the definition proposed above, such text is "immanently inexact"!

Similarly, there are situations (*e.g.* determination keys) where as a rule only precisely defined terms are appropriate, and others, where the meaning of what the author wishes to say can be ***exactly*** expressed only by the use of ***inexact*** words ("group", "medium-size", "substantially", "some", "more or less", "rather", *&c.*)! The same is true of "exact" definitions: sometimes they are unquestionably needed, but – as DOBZHANSKY (1957) fittingly observes – in the case of "*some ... fundamental concepts of biology ... an attempt to produce a formal definition would land us in a morass where no simple idea can be conveyed without endless quibbling and hair-splitting*". So, for example, WILEY (1981) introduces the chapter on characters with very apt observation that he has "*talked to no taxonomist who did not have an intuitive idea about what characters are and there has been little or no confusion in taxonomists communicating their ideas about characters*" – and nevertheless, as (according to him) "*the term is usually defined in an inadequate manner*", he found necessary to define it "more adequately": "*A character is a feature of an organism which is the product of an ontogenetic or cytogenetic sequence of previously existing features, or a feature of a previously existing parental organism(s). Such features arise in evolution by modification of a previously existing ontogenetic or cytogenetic or molecular sequence*"; I wonder whether the above-mentioned lack of confusion would survive the eventual introduction of such definitions into taxonomic practice… Even in case of numerical values the formal "exactitude" is sometimes ***inversely*** proportional to the real: the truly "exact" average length of seven specimens measured as 16, 17, 19, 21, 22, 24, and 27 mm. is "***about*** 21" and not – as it would be given in many publications – "20.857"! Or, what may be the meaning of collecting-locality coordinates given (as in EVANS *& al.* 2003a, 2003b – of course I quote these papers, by mere chance just lying before me, only as an example: such "precision" has become a custom nowadays) with the "exactitude" to 0.001': *e.g.* the Tangkoko National Park is evidently larger than 3 m^2, so the "*N 01°34.205' E 125°09.416'*" is probably meant to refer either to a place within it from where the GPS measurement happened to be done, or perhaps an abstract "midpoint"; but even if (what does not seem likely) all 19 specimens of three species of frogs and toads reported from (one of them even "*near*") the TNP have indeed been collected exactly at that spot, the "accuracy" to less than 2×2 m. would anyway be without any biological significance whatsoever: a single leap of any of those animals would have moved it to different coordinates!

Another "linguistic" question is the use of (often deliberately coined) misleading terminology. The "evolutionary" school of systematics had originally been so labelled to distinguish it from the admittedly "non-evolutionary" phenetics: at that time both qualifications characterized the respective philosophies very adequately. The situation changed when HENNIG (1950) proposed to base classifications on exclusively one aspect of phylogeny – branching sequence – and named his system "phylogenetic systematics": an evident "propaganda-trick" to suggest that other schools are "not phylogenetic", what was true of phenetics but obviously not of "evolutionary" approach, which is no less (in fact, arguably ***more***) "phylogenetic" than HENNIG's (indeed, as phylogeny is a result of evolution, a classification cannot be evolutionary not being phylogenetic – and *vice-versa*). To avoid this conflict between general meaning of the words and their restrictive usage, I prefer – and apply – rather the unambiguous qualifications: "synthetic" (as being an offshoot of the ***Synthetic*** Theory of Evolution, and as attempting to ***synthetize*** in classifications various – branching pattern, tempo, direction – aspects of evolution) and "cladistic" (as making classification to reflect only one – ***cladistic*** – aspect).

"Monophyly" poses similar problem. Originally it was meant to denote the situation where all the ancestors of any member of a group, back to – and inclusive of – the last common ancestor, belonged to that group. Then cladists began to use it in different sense (demanding a monophyletic group to include ***all*** descendants of the last common ancestor), again with obvious intention to suggest that synthetic classifications were based on "non-monophyletic" (what to most biologists seemed synonymous with "polyphyletic") groups, and so were "non-evolutionary". This, not unexpectedly, has created confusion to the extent that it is sometimes perfectly unclear what is being spoken about; as an instructive example let me quote a sentence from MARTENS *& al.* (1994): "*Monophyly is a term originating from cladism where it implies that a given taxonomic group has the same common ancestor*" – the authors ascribe the term to cladism [although it had been coined already by HAECKEL in early 1860-s (ASHLOCK 1984) and introduced as one of most essential properties of "evolutionary" classifications long before "phylogenetic systematics" was born (WILLMANN 2003)], but interpret it according to the "synthetic", rejected by cladists, definition! ASHLOCK (1971) introduced a compromise terminology, allowing to distinguish between groups which include all descendants of the common ancestor (holophyletic) and those which do not (paraphyletic), and to remove the confusion by restoring to "monophyly" its generally accepted meaning; although cladists – apparently for the above-mentioned "marketing" reasons – rejected that proposal, it is accepted here as joining precision with unambiguity. So, here (and in my other papers)

monophyletic = **holophyletic** + **paraphyletic**;
non-monophyletic = **polyphyletic**

The word "speciation", as currently understood, is in my opinion also misleading. Admittedly a synonym of "origin of species", it is in fact restrictively used to denote branching events to the exclusion of phyletic transformation. So we have – at least in principle – two "kinds" of species: those which have originated by splitting of the parental species into two, and those (evolved by anagenesis – without split) which… have no origin at all (origin of species = speciation = splitting)!!! As this is evident absurd, as these two "kinds" of species do not differ in anything, and as even the very difference between "splitting" and "phyletic transformation" is more apparent than real (the evolution of ***any*** particular species from its ancestor was always "anagenetic", whether some group[-s] of populations had or had not separated to go their own way), it is important to have a short and unambiguous word for formation of new species irrespective of ***how*** they were formed. The best term for this purpose would be just the "speciation", but its restrictive meaning has already become so inveterate that any other usage would be confusing; as it is misleading also in the currently accepted sense, it should be abandoned at all. Instead, I propose the following terminology:

transspeciation – origin of one (descendant) species from another (ancestral) with or without splitting of the lineage;

disspeciation – origin of two (or more) descendant species by splitting of the ancestral one;

conspeciation – origin of descendant species without splitting of the lineage.

The purposes of science and what do they mean? or what this paper *is* about?

As mentioned above, I am interested in philosophy of systematics only as far as it may influence the results of a systematist's work. Hence, I do not intend to present here any abstract "system of thoughts on aims and principles", but rather to show "practical" consequences of particular aspects of such system. From this perspective, ***philosophy*** (of systematics) is inseparable from ***methodology***, which may be defined as "general approach to means and ways of accomplishing the aims" of this branch of science. Methodology generates ***methodics*** ("strategy" of solving various systematic problems) which, in turn, determines the choice of most appropriate ***methods*** in actual cases. Thus, I will try to show how philosophical and methodological principles, through methodical strategies and actual solutions, influence the end-products of systematic study – phylogenetical trees, biogeographical reconstructions, and the Grand Synthesis: biological classification.

Systematics is a branch of science, thence philosophy of systematics is an aspect of philosophy of science, and aims and principles of systematics are aims and principles of science as restricted to systematics. So, what are the aims of science? To me, there is only one important aim: to increase knowledge of the World – its origin, history, and present state – in all its aspects. The study of one of such aspects – variability and genealogical relationships between organisms – is the subject of biological systematics. Scientific research in systematics – formulation and verification of hypotheses as to the relationships between organisms – is called taxonomy, and taxonomical hypothesis (or system of hypotheses) is a classification. The definitions proposed above are more or less vague, and I deliberately do not attempt to "improve" them: as I have already argued, terms of not too clear-cut denotation often better serve the purpose to exactly express one's views than "overdefined" ones – they can be used in greater variety of situations, are partly interchangeable, their range of applications is closer to the "general usage" (and so less likely to provoke misunderstandings), *&c.* A definition should be ***only*** as strict as really necessary, *i.e.* to exclude ***only*** such meanings which would essentially change the gist of the respective [part of] text, or at least make it more difficult to understand properly. Thus, *e.g.*, I do not introduce here the (proposed by some French zoologists) restrictive denotation of "*science de la définition des unités systématiques ou* ***taxonomie*** *... qu'il ne faut pas confondre avec* ***taxinomie*** *qui est la science des arrangements, des lois de classification ...*" (Dussart 1974), because in the type of argumentation planned for this paper the exact meaning of the word "taxonomy" in particular cases will be either immaterial or evident from the context.

In most general terms – I do not expect much disagreement at this point – the aim of natural sciences is increasing the verifiable (or: falsifiable – see below) knowledge about the real world. The primary – also rather obvious – consequence of this statement is, that the most important requirement for scientific ascertainments is their being true to the reality. In other words, the ultimate criterion for the evaluation of a hypothesis is how adequately it describes and/or explains the (particular aspect of) structure and functioning of the Universe. Of course, having no "external" knowledge about the reality, we cannot compare our results directly with it, therefore the verification of our hypotheses must be based on their compatibility with other, more or less "highly corroborated" ones ("theories" and "facts").

I do not wish to get implicated in a quibbling dispute as to "what is reality?", "what is truth?", *&c.* – these belong to the class of basic notions, the meaning of which is proverbially "*known to anybody except some philosophers*" and we do better leaving the detailed analysis of such questions to them. This, of course, is not intended as depreciation of philosophy – I wish only to point out, that philosophy is one thing, biology is another, and they should not be confused: each of them has its own language, its own problems, and what is a very intriguing and difficult topic of basic importance for one of them, may be perfectly trivial and/or irrelevant from the other's perspective. I fully agree with Johnson's (1968) "self-criticism" that biologists "*have perhaps already ventured too far into the philosophical chamber of horrors*"; so – as a biologist, not philosopher, writing this paper for biologists – I think it perfectly enough to accept the basic assumption of any (natural-) science: that our World is (in the above-mentioned, "known to anybody", sense) real and there is a single truth about it, one (and ***only*** one!) system of hypotheses adequate to the reality. This is equivalent to the statement, that there "exists" a solution to each scientific problem ("*raffiniert ist der Herr Gott, aber boshaft ist er nicht*" – according to Einstein's famous aphorism), *i.e.* each aspect of the reality can be accurately explained by an adequate hypothesis – we must only find this hypothesis...

It has been customary to distinguish between (conjectural) "hypotheses", (well substantiated) "theories", and (supposedly firmly established) "facts", yet the differences are only those of degree: "*facts ... are nothing more than highly corroborated hypotheses*" – Eldredge & Cracraft (1980). Thus, the process of "increasing knowledge of the World" – scientific research – consists in principle of two steps: formulation of hypotheses and testing (verifying) them. Though the reality is only one, and so there can be no more than one true hypothesis about any aspect of reality, this does not mean that an easy way to discover it or – perhaps still less so – to prove its veracity must also exist. Indeed, strictly speaking no "proof" is ever possible: we can never make our hypothesis "absolutely sure", we can only make it "very probable". This sorrow situation is perfectly symmetrical: the widely adduced Popperian stipulation, that we should discuss in terms of "falsification" instead of "verification", because hypotheses "*may be disproven but never verified*" (BOCK 1974) is perhaps true in some philosophical sense, but from the viewpoint of normal scientific reasoning (at least in biology) is an absurd – "*logical axiom, that one can never prove an empirical generalization, can be raised as an objection to all empirical science and much common knowledge. ... Consequently, this argument is irrelevant when attempting to discriminate between hypotheses within empirical science*" (MOONEY 1993). In particular, it is utterly unsuitable to discriminate between "verification" and "falsification": falsification of "A" is equivalent to proof of "non-A", falsification of "non-A" verifies "A" – if we cannot prove "A", we are equally unable to prove "non-A" (*i.e.* to falsify "A") [even if we examine a million of ravens and all will be black (to quote POPPER's own example), this indeed will not be a proof of the hypothesis that "all ravens are black" (there always remains the possibility that one or more of those not examined is green or cupreous); but neither will it be a ***falsification*** of the opposite hypothesis that "some ravens are cupreous-green"]! While some of such "non-A" statements are so broad as to hardly deserve the qualification as hypotheses, others are perfectly equivalent to "A"-s: if somebody will succeed in falsifying the hypothesis of autochthonic origin of life on the Earth, the alternative hypothesis of its "cosmic" origin will be automatically proven; falsification of polyphyly is a proof of monophyly; disproof of conspecificity means verification of specific distinction; *&c.* Indeed, usually the easiest way to "falsify" is to "prove" the opposite: the best disproval of the hypothesis of homology between fins of shark, ichthyosaur and dolphin is the demonstration how did they develop separately. In most situations "verification" *vs.* "falsification" is no more than a matter of formulation: the hypothesis that hard external skeleton cannot develop without segmentation of the body is, indeed, not "verifiable" (in this abstractly philosophical sense) but only "falsifiable" (by finding an unsegmented animal with exoskeleton); but for the alternative hypothesis (that these features can occur separately) exactly the opposite is true: it can be proven (on the same way) but never falsified! Or, how could the hypothesis "corals belong cladistically to plants" be falsified, except by ***proving*** that they belong to animals? Of course we could accept the ***convention*** that "in fact" we do not prove that corals are animals, but only falsify the hypothesis that they are not, yet such a semantic consent does not seem to make any sense in biology (maybe it does in philosophy or formal logic, but "*not surprisingly*" "*misunderstandings arise when definitions developed in one domain are applied to the other*" – REEVE & SHERMAN 1993).

Thus, if "proof" is only a matter of probability, so is "falsification" too: even if the "*mistake that a hypothesis is definitively falsified with a single fact ... which does not match it"* is indeed "*widespread – also among those working on theory of knowledge"*, it is nevertheless a mistake: *"A hypothesis is never falsified with any disagreeing fact, but only with another hypothesis matched by* ***more*** *facts*" – Lorenz (1988). Or, as Sherlock Holmes says, "*if a fact apparently contradicts a long chain of deduction, this only proves that we must find another explanation for it*"!

"*Let me illustrate the principle involved.*

The world was created at 9:31 a.m., June 30, 2003, *EST.*

I cannot prove this.

You cannot disprove this.

You may say it is ridiculous. You can remember what happened on May 17 and you certainly were born earlier than that. You remember President Clinton *and the* Gulf War.

I agree, you can; but as long as we are hypothesizing, is it not possible that each of you was crated as you now are – complete with all your memories; and that the world could have been created last June – complete with all its geologic history and structure?

Again – this assumption cannot be disproved. It can only be disbelieved. There is a fundamental difference.

However, if it is true, then with one exception, this or any version of special creation at a specific point in time is incredibly complex" (Baldwin 1981 – to "up-date" the background, some details [shown as non-italicized] have been changed).

This example excellently illustrates the point: if we replace the word "complex" with "improbable" (what we can legitimately do, as complexity is roughly inversely proportional to the probability of occurrence – a rule applied to scientific reasoning as "Ockham's Razor"), then the meaning of "verifiability" (or "falsifiability") is at hand: a hypothesis should be accepted as "valid" if it offers a likely explanation of observed patterns or phenomena, and should be rejected as "invalid" if the offered explanation is unlikely. But what does it really, in "scientific practice", mean? Should we reject a hypothesis if its probability is less than 50%?; less than 10%?; less than 1%??? Evidently, in this form the question is unanswerable, as well for technical reasons (how to measure the probability that the **Arthropoda** are monophyletic?) as "in principle": we know that even "incredibly complex" (extremely unlikely) situations ***do*** occur (indeed, the very notion of probability ***demands*** that they occur: low probability of some situation means not only that it ***does not*** occur ***frequently***, but also that ***sometimes*** it ***does***!), and automatic rejection of "improbable" hypotheses would preclude the recognition of the true one. The key to solution of this dilemma is in ***relative*** likelihood: we rarely (if ever) can decide whether the probability of a hypothesis is 3 or 30%, but the assessment that the hypothesis A is more likely than B or C – and, thence, is "valid" while the latter two are "falsified" – is usually possible. Naturally, "valid" does not mean "certainly true": the process of verification is never definitely ended, the accepted hypothesis is always confronted with new evidence which can eventually "falsify" it either by changing our assessment of its probability in relation to B and C, or by inducing creation of a new hypothesis D more likely than any of the hitherto considered ones. [The claim that "valid" hypothesis is that most likely might seem to contradict POPPER's stipulation that "*the degree of corroboration*" stands "*in inverse ratio to ... logical probability*"; however, the contradiction is only apparent, as his "logical probability" is almost exactly the opposite of the "common-sense likelihood" invoked by me: he demands ***low*** probability of the occurrence of the observed phenomena under the assumption that the tested hypothesis is ***false*** ("*the 'logical probability' reflects an explanation of the observation in some manner that is distinct from the stated hypothesis*", it "*reflects the chance of finding the hypothesized cladistic structure under only the assumption of the random-covariation null modell*" – FAITH & CRANSTON 1992), while I look for a ***relatively high*** likelihood of their occurrence under the assumption that the hypothesis is ***true***!].

The precept of relative probabilities as the only criterion of the validity of hypotheses seems almost trivially obvious, but its consequences are not always realized:

1). That the most important attribute of a hypothesis is its being a possibly true description of reality is but a tautological version of the statement that ***any*** other property is of ***less*** importance; it means, in particular, that testability ("verifiability", "falsifiability") – however essential it certainly is – cannot be ascribed primacy: the widespread stipulation "*that among two scientific theories, the one should be preferred that has the highest degree of falsifiability*" (LØVTRUP 1975) evidently confuses a tool with the purpose. The world is real and knowable (otherwise no science would be possible), but there is no reason to assume that its structure has been adjusted to the convenience of scientists: the true hypothesis may happen to be the easiest to test, but it may equally well be the one offering the ***least*** possibilities of testing. I am aware how easy it is to ridicule this opinion, and that it is not likely to be spontaneously acclaimed by the scientific community – the lust for certainty, "objectivity", "philosophical soundness", "logical clarity", "mathematical precision", *&c.* is too strong – but it must be remembered that the function of scientific hypotheses is different from that of mathematical, logical or philosophical theorems: while the only requirement for the latter is consistency with their own basic assumptions ("axioms") and "rules of game" [*i.e.* they must not contain inherent contradictions: for a mathematician or philosopher – according to their own proclamations – "*interesting is not what the world is, but what it **should be***" (K. LÁNCZOS, *teste* MARX 2000), "*the existence of an object is sufficiently confirmed by uncontradictoriness of its description*", *&c.*], the former's main – and, in fact, only! – *raison d'être* is cognizance of the reality. The best example of how bizarre may be the results of subordination of the goal to a tool is

ELDREDGE & CRACRAFT's (1980) defence of "*implicit assumption that a particular autapomorphy cannot revert to its 'primitive' condition ... ('character reversal does not occur')*", because – according to them – this "*is methodologically essential if hypotheses of ancestry and descent are to have any degree of testability whatsoever*" and "*viewed in this light*" "*is perhaps not an overly steep price to pay for testability of phylogenetic trees*"; in my opinion, scientists' task is to disclose "*what the world is*" rather than "*what it should be*" (phylogenetics is not a kind of "logical game", whose basic assumptions can be agreed upon at will without reference to the "external" reality), and "*viewed in this light*" the price proposed by ELDREDGE & CRACRAFT (1980) ***is*** unacceptably steep indeed: what is the value of "testability" based on ***evidently false*** assumption (perhaps, in order to make our hypotheses formally testable, reversals "should not" occur, but we know that they very frequently ***do***!)???

2). Invalid ("falsified") view means: found to be less likely than its alternative; a direct consequence of this "equation" is, that – except for some rare extremal situations – a hypothesis should not be rejected as long as it is the only one, *i.e.* until another, roughly equivalent, has been proposed and shown more likely ("equivalent" means here "of similar degree of concreteness": it would be obviously meaningless to compare the likelihood of the hypothesis that *Hallucigenia* belonged to the **Onychophora** with the "alternative" view that its relations are "somewhere else"). Unfortunately, rejecting of hypotheses on grounds of "maybe it might be possible to concoct another one, which perhaps could eventually be considered as better fitting our preconceptions" is by no means rare; as an instructive example may serve the "falsification" by ELDREDGE & CRACRAFT (1980) of SIMPSON's hypothesis of equid evolution: "*Simpson ... fails to point out that at the among-species level, the actual geometry of evolution* ***might*** *be radically different, such that the bold extrapolation of Wright's within-species allelic imagery* ***might*** *be consistent with, but inappropriate as a simple depiction of, among-species evolutionary phenomena*" [bold-face mine – RBH]; the authors do not propose (or quote) any other explanation of the "*evolutionary phenomena*" in equid evolution, they do not even deny that SIMPSON's "*scenario*" is "*consistent with*" them, yet they reject the hypothesis because it had not been formulated in terms of their dogma (conceived, at that, many years after SIMPSON's publication...) of fundamental difference between "macroevolutionary" and "microevolutionary" processes! Similarly improper is outright refusal of some kind of data (*e.g.* palaeontological) on grounds of their incompleteness (though, of course, the degree of incompleteness must be taken into account in evaluation): as pointed out by PAUL (1989), "*no science is based on complete knowledge*", and very often just such "proverbially" incomplete data provide the best available evidence for (or against) the particular hypothesis!

One may ask, how to treat a hypothesis that seems evidently contradicted by known facts, but we are unable to conceive an alternative one which would better comply with the available evidence? Even in such situation the automatic rejection is unwarranted: the accurate explanation of facts ***does*** exist, thence if the hypothesis at issue is false, some another must be true – and *vice versa*! Maybe it is false indeed, but this is not the only – and not necessarily the most probable – possibility: there are several!

A. The hypothesis is false indeed. In this case, we have been unable to conceive the better one either

a) because of lack of imagination; or

b) because our interpretation of "facts" is also false, to the point that no hypothesis can be in accord with it!

B. The hypothesis is true. In this case, we have found it inconsistent with facts either

a) because our interpretation of "facts" is false; or

b) because our interpretation of the relation between hypothesis and facts (*i.e.* of the "predictions" of the hypothesis) is false.

The possibility **Aa** seems in most cases the ***least*** likely; on the other hand, in ***all*** the remaining situations we do better tentatively accepting the hypothesis (as has been shown, ***any*** acceptation or rejection of a hypothesis is always tentative: it is only the ***degree*** of tentativeness that varies) as the most probable one (however unlikely, a hypothesis is the most probable one if the probability of others is still less, or – what amounts to essentially the same – no alternative can be conceived); doing so we have at least a "detector of errors" (pinpointing contradictions in our knowledge, which should be subjected to specially critical evaluation), while simply rejecting the hypothesis we have nothing…

3) If – as has been argued above – the "falsifiability" of hypotheses is no more "absolute" than their "verifiability" (both being only a matter of relative probabilities) then hypotheses "testable" in Popperian, absolute sense do not exist. On the other hand, in the sense accepted here, ***all*** hypotheses are testable: for ***any*** hypothesis we either can or cannot propose a more likely alternative! Of course such a statement does not say anything about how to decide which one is more likely in any particular case – but this is a completely different question, of which little can be said in general terms, except for the triviality that the testing is done by confrontation of logical consequences ("predictions") of the hypothesis with accepted theories and "facts" (remembering, however, that these are also "*highly corroborated hypotheses*" at the best…).

Intuition, testability, and commonsense

It has been repeatedly stressed above, that researchers' approach to scientific questions, to be maximally efficient, must remain flexible, adaptable to the specificity of particular problems and particular sets of data; absolutely rigid, exceptionless formulations "work" only in fields like logic or mathematics (though even there "*one must perforce use certain terms and syntactic relationships of undefined meaning*" – JOHNSON 1968). Especially in biology all (including this one…) statements, rules, definitions must be treated like *e.g.* zoogeographical regions: perhaps unequivocal in their "main body", but more or less blurred at borders. In other words, we should consider all our methodological principles as infinite series of hypotheses claiming that the particular method, rule, definition is without modifications applicable to the particular problem and particular set of available data; not unexpectedly, there exists no general criterion for testing such hypotheses – except MICKIEWICZ's warning: "*Ucz się i pracuj, tak poeta radzi, lecz trochę rozsądku nigdy nie zawadzi*" ["learn and work, the poet advices, but a bit of common sense will never harm"]…

Common sense has become as unpopular, almost insulting word to many philosophically or mathematically minded scientists, as is beauty to some modern artists – and the results are often equally disastrous… Its "outlawry" from natural sciences is sometimes evidently based on hardly understandable misapplication of the word, like MAYR's (2002) assertion that "*common sense tells us … that the Sun revolves round the Earth, which is flat*" – for the great zoologist "common sense" evidently means uncritical (with brain "switched off") acceptation of appearances: common ***sense*** could perhaps tell something like that to a prehistoric mammoth-hunter, but in face of so ample evidence accumulated thereafter (observations of ships approaching from behind the horizon, ERATOSTHENES' measurements, MAGELLAN's travel, photographs from satellites, *&c.*, *&c.*, *&c.*) the belief in flat Earth might only follow from "***un***common ***non***sense"! In other cases the depreciation of common sense is motivated by quoting situations – usually some relativistic or quantum "paradoxes" – where it is apparently unhelpful. It must be remembered, however, that all such instances refer to those aspects of nature which are completely out of our "evolutionary experience" – and even in these realms of knowledge conflicts between common sense and "highly corroborated hypotheses" of physicists are astonishingly rare! On the other hand, the "study" of biological phenomena (and especially "systematics": classification of animals and plants into groups of similar properties) not only "*predates the beginnings of oral and written histories*" (BOCK 1974), but has been the most important business of man and his ancestors since the origins of life, and our brain has evolved under strong selection pressure directed mainly towards making this study most efficient. So developed well-proven "tools", maximalizing and evaluating available information, are just these currently outmoded methods usually referred to as intuition and common sense! Indeed, virtualy all (at least in biology) greatest theoretical achievements – CUVIER's Principle of Intercorrelation, DOLLO's Rule of Irreversibility, DARWIN's Theory of Evolution, and so many others – are a result of largely intuitive interpretations supplemented by logical reasoning under powerful control of common sense. If we can learn anything from the history of science, it is that "*historical study of the improvement in the classification of any taxon suggests that the major reason for improved comprehension of systematic relationships between these organisms has been a more thorough, detailed study of their characteristics, not the application of new or different philosophical approaches*", "*most evolutionary taxonomists knew what they were doing and* *why, even in the absence of a clear statement of philosophy and methods. This lack of attention to the philosophical basis of a science is not unique to biological classification and is not necessarily a serious detriment to rapid progress*" even for such proverbially "exact" science as "*physics during the nineteenth century*" (Bock 1974).

"Logical reasoning under control of common sense" might seem a strange "upside down" stipulation, but is rather easily justifiable. Logical reasoning is done by people, and "*errare humanum est*" no less in performing logical operations than in any other human activity; the more complicated and farther from everyday practice the introduced theorems are, the greater the probability of error, and the more difficult its detection. On the other hand, common sense is also nothing else than a system of logical "algorithms", but these are well-matched (by evolutionary development and individual experience) to one another as well as to the very kinds of problems and evidence usually met in our ancestor's environment – and in biological sciences! "*For an analogy we may look to language. Natural languages are complex, not very logical systems, full of redundancies and overlaps. ... Nevertheless, most of us have not yet found it necessary, feasible, or desirable to discard common language even in scientific communication ..., it serves us better than any substitute ...,* ***because it suits our psychology*** *(which is far from simply logical) and because it links us with the knowledge of the past*" – Johnson (1968) [display in the original]. How easy is it to stray under the sole guidance of mathematical/logical "rules of game" is best shown by well-known "proofs" that swift-footed Achilles could never overtake a turtle, that 2=3, that every number is greater than any other, or the common experience summarized in the popular sneer that "*there are three kinds of lies: simple lie, unblushing lie, and statistics*" – it is by no means easy to detect the error in the "logical reasoning", and we would usually not even suspect it had the common sense not caution us that "something is not in order"! This is just the very kind of "control" I have in mind.

Intuition is being held in perhaps still deeper contempt than is common sense: qualification of a hypothesis as "intuitive" is frequently considered sufficient for editors to flatly refuse publication of the paper, and for fellow scientists to leave it out of serious consideration. And yet, the majority of really valuable results in natural sciences have been based on what Albert Einstein recommended as "*intuition supported with experience*" (Newton 1996): "*the very character of the revolutionary research work in theoretical physics, astronomy and cosmology contributed to the denial of that model of scientific reasoning, which presented it as a procedure joining rigorous deduction with systematic control of the conclusions derived from empirical observations. Into the foreground came especially the role played by imagination, metaphore and analogy, as well as speculative reflection transforming principal categories and unconventional inference based on intuition (some are inclined to contend that these factors have always occupied their proper place in the actual processes of scientific discoveries, irrespec-*

tive of the dominant concept of ‘scientific method’)” – Collini (1999). Even the most vehement advocates of “strict” procedures cannot exclude intuition from their work: “***Intuitively**, it **may seem** that organisms most like those being investigated would be most appropriate* [as outgroups] ...”; “*None of these features ... appear in other organisms that are not ... **suspected** to be a part of a larger group defined*”; “*the choice of characters involves a **perception** of similarity, i.e. a **perception** of comparable form and spatial relationships relative to other features of other organisms. In fact, more fundamental **subconscious perceptions** probably precede even this elementary level of comparison ...*” (Eldredge & Cracraft 1980; bold-face mine) – such elements (even if not always so explicitly declared) can be found in virtually any scientific work, also in those whose authors earnestly claim to have applied only “objective”, “rigorous” methods!

The basic misunderstanding lies apparently in false associations: intuition is comprehended as a kind of fraudulent jugglery, soothsaying, or at the best some inexplicable mystical “paraphenomenon”. In fact, there is nothing deceptive or metaphysical about it: it means simply making use of that wealth of experience – embodying factual knowledge as well as associational and inferential skills, and exceeding by far the clearly realized “top of iceberg” – that has been accumulated in scientist’s mind but remains unconscious and/or cannot be expressed in “exact” terms. Intuitive conclusions come frequently much closer to the truth than strictly analytic ones, and provide solutions to problems in many situations where rigorous reasoning is impossible or inefficient. A good example is chess (frequently dubbed “***scientific*** game” because of so many similarities in ways of reasoning), where the ability to quickly calculate positions which may appear as the result of long series of possible moves, and to remember multitude of previously analysed configurations, decides upon victory or defeat. Calculations and memory are just those fields where computers are overwhelmingly superior to man, so it would seem that already a relatively primitive one, properly programmed (what, given the very simple rules of game, might seem rather easy), should leave no chance to even the strongest grandmaster – but… despite many years of continuously very great effort of constructors and programmists it was not until 1997 that a superpowerful machine for the first time won (***minimally***: by one point out of six games) a match against the world-champion (Garri Kasparow)! On the one side a formidable (1.5 ton!) enginery able to analyse 200 000 000 (!!!) positions in a second, having its memory loaded with “*enormous data-bank with millions of games, analyses of openings, mid-games and endings, ... ideas conceived during centuries by leading players with… Kasparow on the head*”, which “*is never tired, is absolutely immune to stress or fear*” (Wach 1997), on the other a human being whose (in the case of Kasparow) analytical capacity is estimated as 3 (yes: ***three against 200 000 000!***) positions per second (Miś 1997) and memory also incomparably less receptive, with all his psychological weak points – and the result… 3.5:2.5! Not too impressive… Why? – because “*computer must at each* *time perform the analysis of all possibilities, while man – through his intuition, talent and other properties which we (at least for the moment) are not able to programme or even algorhithmize – chooses only those relatively few which promise the success in continuation of the game*" (Miś 1997). "*Not always the outcome of a combination can be calculated. The attribute of true talent is intuitive assessment of real value of the idea*" (Filipowicz 1997). This holds equally true for scientific research – and especially in fields like systematics: the skill of recognizing things as belonging to this or that category makes a dominant part of our (and our ancestors', back to invertebrates) everyday experience, and has been developed under very strong selection-pressure on the basis of "holistic" rather than "analytic" perception. Everybody can easily distinguish between, say, 18 and 45 years old persons, although it would be rather difficult to find reliable "key-characters"; any little child confidently classifies an animal as "miaow-miaow" or "bow-wow", without checking its dentition or claws and having never heard of the very existence of "exactly formulated criteria"; and so it is by no means surprising that an experienced taxonomist can "at a glance", from the "general appearance", recognize most taxa in his group even if the "critical" features are invisible or misleadingly (*e.g.* teratologically) developed. Indeed, every zoologist knows how frequently the determination of a particular group according to rigorously constructed algorithm ("key") is very difficult or even leads to misidentification, though "intuitive" recognition poses no problem whatsoever! And it is also "*common experience for the systematist revising a particular group to reach the conclusion that a particular assemblage of species constitute a natural sub-group, before he has discovered any single character by which this sub-group could be defined*" (Crowson 1970), and such conclusions usually prove at least as reliable as those defined in "objective" terms. "*Intuition, i.e. the ability to guess, predict, and constructive imagination, presentiment, instinct play a very important role in chess. … Few years old children often place figures on proper squares not really knowing why they do so. Sometimes they achieve great successes, ... Later many of them, influenced by inapt trainers, lose the God's gift*" (Filipowicz 1997). Not only in chess… some taxonomists also seem to have lost the ability to distinguish a locust from a mosquito without having examined its wing-venation, genitalia, and/or DNA-sequence…

The "original sin" of intuitive interpretations is said to be their "untestability" – this accusation is, however, neither decisive nor valid! It is not decisive because – to paraphrase Johnson's (1968) already classic (originally applied to measurements) statement – "*science is not the process of*" testing; "*that is merely a technique of science*": even if the imputation of "untestability" were true, the hypothesis might nevertheless be valid and fruitful. But the objection is also false: intuitively derived hypotheses "*are susceptible of demonstration that they are very likely to be untrue, and this is indeed the only disapprovability we can hope for in a good deal of science*"; they are as "*testable by predictive value, self consistency, and consistency with extraneous evidence*" as any other (Johnson 1968).

One could say, intuitive conclusions of one student may be quite different from those of another one. This is, of course, true, but: **a**) such situations (at least in the case of experienced specialists) are much less frequent than "anti-intuitionists" claim; and **b**) they do occur with "objective" logical reasoning as well ("*Logic has prompted many geologists, that ... humid periods – pluvials – should correspond to glacials of higher latitudes, but the same logic suggested to others, that pluvials should match warm, and thence humid, interglacials. Alas, hypotheses based only on logic, however 'iron-firm' may it be, often lead astray*" – VAN ANDEL 1991).

To the same category of misapprehensions belongs frequently formulated demand that the scientific work must be done "objectively", "without preconceptions". This is both unrealistic and unwarranted stipulation! Isaac NEWTON's famous declaration "*If I have seen further it is by standing on ye shoulders of Giants*" means nothing else than that every study ***must*** be based on the author's knowledge and experience – and the so disreputed "preconceptions" are in fact not fairy-tales but just this important set of evidence. It consists of enormous, undisentanglibly interwoven assemblage of (conscious or not, "firmly" corroborated or overtly hypothetical, based on own observations or directly on "shoulders of Giants": the results of previous workers' investigations) information, and the justification for particular elements of this set may be anything between ***almost*** perfectly "rigorous" and ***almost*** purely intuitive; indeed, the conviction that conscious and "rigorous" is always better than intuitive is itself a preconception [and ***false*** preconception at that: has anybody ever formally "corroborated" a hypothesis that tiger is phylogenetically closer to leopard than to mosquito? – to my knowledge not (in fact, such a study would be a strong candidate for "IgNobel"!), and nevertheless I wonder whether any taxonomist would feel necessary to specially explain his "preconception" to treat it as a member of **Felidae** rather than **Culicidae**]! Of course, in most cases the situation is not as clear but, as I have already pointed out, nothing in this world is absolutely sure ("*facts are nothing more than highly corroborated hypotheses*" – and most of our hypotheses are but moderately corroborated!), hence in practice everything is a "preconception", we have nothing else to base our research on: preconception is our selection of taxa to include in the phylogenetic reconstruction, preconceptions allow us to classify them as in- or outgroups, nothing but preconception is the choice and weighting of characters and their division into "states", application of the particular algorithm, and also the basic assumptions (parsimony, homology, polarity) of ***all*** formalized procedures! So, there is nothing like fundamental difference between "objective" and "intuitive" evidence, nothing like sharp distinction between highly corroborated "facts" and weakly supported conceptions, and neglecting ***any*** of them results only in loss of (more or less, but) valuable information on which our "null hypothesis" – to be accepted until ***convincingly*** falsified – should be based; of course it could be, and in most cases partly is, false, but so are the assumptions derived by formalized procedures as well! Fortunately, elimination of preconceptions is an illusion: conssistently applied it would amount to starting each project from zero, like a newborn having not yet observed, read, thought of anything; had we really complied to this stipulation, our knowledge would have never exceeded that of pithecanthropus…!

So, it is – I think – evident, that disqualifying intuition means to deprive ourselves of a very rich source of evidence, and is a serious (though, unfortunately, not uncommon) error! Naturally I do not wish to belittle the role of rigorous criteria, exact definitions, *&c.*: they are also very important in scientific reasoning, and their importance increases at the later stages of the "evolution" of hypotheses: when two or more competing views are being compared and tested. But even at the latest stage some savour of intuition and common sense is usually highly serviceable…

Standardization, mathematical procedures, computer programs

The strive for objectivity, repeatability, testability *&c.* is a perfectly right attitude as long as their proper place in the "hierarchy of aims" is maintained, but becomes very harmful if – as Polish proverb says – the nose is expected to serve the snuff-box: if these ***tools*** dominate over the ***purpose*** of scientific research (*e.g.* if hypotheses are accepted or rejected on grounds of whether they are easily testable or not, or whether they had been derived "objectively" or "intuitively"). Similarly, standardized (incl. mathematical and computerized) procedures may – properly applied – be very helpful or even indispensable in solving many biological problems, but we should always remember that they are "*only a technique of science, not the science itself*"! Unfortunately, there is a strong and constantly increasing tendency to evaluate scientific projects on the basis of complexity, "modernness", expensiveness (strangely enough, in practice ***positively*** appraised!) of the tools (methods, techniques, instruments) rather than on novelty and soundness of the results achieved: an electron-microscopic study is "automatically" considered more valuable than that done with simple magnifying glass or light-microscope; phylogenetic reconstruction based on "molecular" data better than that using morphology; principal component analysis preferable to simple comparison of means – even in situations where it is at glance evident that the less sophisticated approach would perfectly suffice to find a fully reliable (and usually more easily testable!) answer! In this way, expensive equipment, elaborate mathematical formulas, abstract indexes become the value in themselves, and the mere question as to what real purpose do they in particular context serve sounds vacuous and brands the inquirer as a backward blockhead… And indeed, for many contemporary (mostly, but not only, younger) workers the "recipe" for scientific eminence looks apparently very simple: put some raw data into a computer, select the newest software, push the button, and – wait for Nobel Prize… [this is not only my personal "crusty curmudgeon's grumbling" – *e.g.* fifteen years ago ELDREDGE (1989) wrote: "*It seemed incredibly easy to do very slick, hypermodern science: just grab a bunch of fossils, measure the hell out of them, crank them through the*

IBM 7090/7094 – preferably using one of the sophisticated multivariate statistical procedures – and voilà! instant answers, instant results, instant success”].

This is a self-perpetuating – or even self-amplifying – phenomenon: in the world where scientists compete with one another in using more and more far-fetched devices and procedures instead of original ideas and discerning interpretations [“*there has been (and still is) a tendency (ineluctable in some quarters) to let someone else's algorithm (numerical procedure) massage our data (more often than not these days collected by a technician) as a substitute for careful thinking about either the data themselves or even the assumptions and apparent results of the computer analysis*” – ELDREDGE (1989)], there is an overpowering pressure on young students to plan and carry on their research in the same spirit. Many of them surrender to that pressure with a good grace, either due to fascination with modern technique, or simply as the “line of least resistance” (it is easier to calculate – especially with computer – than to think…), or a kind of “mental *alibi*” helping to hidden lacks in one's imagination (criticism, interpretative ability, *&c.*) behind “modern procedures”, and shift the responsibility for eventually false conclusions upon “objective methodology”; those who try to resist must be prepared – among other vexations – for frustrating problems with publication of their opinions and results (*... the uncritical embrace of a technological advance may lead to an unwarranted 'find'em and grind'em' mentality, yielding plentiful results, the significance of which may remain uncertain at best. Recently, concern has been aired about whether the increasing use of computers in different fields of biological research may be diverting our attention away from attempts to really understand the generated data ... Computers are an aid to thinking; they should not replace it* – JENNER 2004).

This situation generates serious dangers. In science – like in medicine – *panacea* do not exist, and also sophisticated “modern” instruments or procedures, far from being always the best, are very often simply inappropriate. First of all, biology is not physics or chemistry and – in spite of frequent claims to the contrary – should not uncritically ape their methodological approaches or standards. Every piece of non-living substance will, under identical circumstances, behave always in the same way; these circumstances influencing the result of observation or experiment consist usually of very few, predictable and controllable, crucial factors, and so it is relatively easy to make them “identical”; moreover, the correlation between these “crucial factors” and the behaviour and properties of the substance is “mathematically strict”, what enables precise predictions (knowing the density of iron at 0, 10, 100 and 1000°C we can “foresee” its exact density at 30, 127, or 538.257°). Such precision is unattainable in biology not – as sometimes interpreted – because this branch of science is “underdeveloped” and should strive to “make up” the “arrears” to deserve the rank of “true science”, but just because of the nature of its subject: simple, exact relations between parameters in biology do not exist, neither do exactly controllable “identical conditions” or strictly predictable reactions! There are no “identical” objects in the living world: every higher group, species, population, every organism or cell is different and reacts differently, and even the behaviour of single individual is not always the same; moreover, the circumstances of observation are practically never truly repeatable, factors potentially influencing the result being very numerous, often difficult to measure, and almost never fully under our control. Thence, our results are – and must be – differently evaluated, and methods to derive these results are inevitably different as well!

Back to taxonomy

The most essential conclusion from the considerations presented above may be concisely summarized as follows: biology in general – and systematic biology in particular – is **not physics**, and therefore need not (and must not!) provide absolutely exact predictions based on "mathematically strict" formulae: by Supreme Order of the Lord (or Lady?) Evolution, in biological ***reality*** everything ***is*** more or less blurred at least at fringes; it is **not patience-playing**, and therefore need not (and should not!) give preference to contractual conventions over factual knowledge: by Supreme Order of the Lord (or Lady?) Science, researchers are interested in what the world ***is*** rather than what some of us ***would like*** it to be; it is also **not philosophy**, and therefore need not (and should not!) overestimate the importance of ontological or epistemological subtleties for the accuracy of our conclusions: by Supreme Order of the Lord (or Lady?) Nature, the negligible interdependence between distant phenomenological levels ***does not*** warrant taking them into serious consideration. It is **biology**, the branch of science aiming at the study of ***biological*** patterns and processes, using methods appropriate to solve ***biological*** questions and presenting the results in ***biologically*** meaningful terms! Now let us give some thought to the – direct or indirect – consequences of this statement for our taxonomical work, as exemplified by two fundamental, closely related but nevertheless distinct, hotly debated problems: the **principles of classification**, and the **species concept**.

The progress in science is realized through the infinite cycle of hypotheses → predictions → verifications → new hypotheses → new predictions, *&c.*; if so, then the most essential property of a hypothesis is its predictive power ("*prediction is the very hallmark of science – indeed, ... a science isn't really a science if it lacks the power to predict*" – ELDREDGE 1989). As it is generally agreed that classifications ***are*** taxonomic hypotheses, they also ***should be evaluated*** on the same basis: according to the extent to which the (morphological, ecological, physiological, genetical, or any other) characteristics of an organism may be predicted from its placement in the system. Each contemporary taxonomist belongs – consciously or not, consistently or not – to one of three main "schools" of "classification philosophy": "phenetic", taking into consideration only the degree of "overall similarity" between taxa with full disregard of phylogenetic considerations; "cladistic", whose systems should strictly represent the "branching sequence" of the inferred evolutionary history; or "synthetic", striving to maximally reflect the pattern of similarities within the limits of compatibility (though not necessarily exact congruency!) with shape of genealogical tree. Which of them is the most promising from the viewpoint of predictive power, and thence "valid"? Species – by definition – evolve independently, and immediately after disspeciation [see above – **Some linguistic remarks** – for discussion of terminology] each genetical change in any of the daughter species can only ***increase*** the difference between them; later on, changing direction of selective forces (or, in some cases, "genetic drift") may eventually cause some degree of convergence, but that involves ***only a fraction*** of characters, while others continue to diverge. Arguably, even in the most striking cases of convergent evolution, the accumulation of differences overwhelmingly surpasses the development of similarities (albeit occasionally few superficial resemblances can make the appearance of the opposite). That is to say, the disparity between any two lineages always ***increases*** in time (the respective species are more different now, than their ancestors were at any time in the past) – ***"overall" convergence does not exist***!

An obvious consequence of this statement is, that the best classification (that of maximal predictive power) must not contain ***poly***phyletic groupings, and ***this*** – not any kind of "evolutionary dogma"! – makes systems agreeing with phylogeny the only acceptable. The continuous, irreversible increase of difference means, that the groupings based on ***truly*** "overall" similarity would always agree with phylogeny; this, however, is not necessarily true of small – and rarely approaching "statistical randomness" – ***samples*** of potentially relevant characters available to taxonomists: prevalence of convergent features ***among those actually examined*** is, unfortunately, by no means rare, what disqualifies purely phenetic approach as ***in principle*** wrong (though ***in practice*** phenetic methods can – and frequently do – produce correct classifications). On the other hand, the tempo of divergence – the rate of accumulation of new characters ("apomorphies" in cladistic terminology) – varies enormously between lineages, rendering many ***para***phyletic taxa more informative (of greater predictive power) than the respective holophyletic ones; the disregard of this fact is the "original sin" of cladistic classifications.

The dogmatic stipulation that paraphyletic taxa are unacceptable is motivated by another dogma: that classification must ***strictly reflect*** the cladogram; its advocates usually adduce, as the paramount advantage, the fact that – while all deductions from a synthetic classification are "only approximate" (the information that an animal belongs to the **Insecta** strongly suggests, but does not prove, that it has three pairs of legs) – cladistic systems ***exactly*** "predict" genealogical relationships. A moment of reflection is, however, enough to show that this argument is seriously flawed: on the one hand, it is not cladistic classification that predicts genealogical relationships, but the opposite: cladistic classification is nothing more than the pattern of genealogical relationships (as ***previously*** reconstructed!) presented in words (taxon names); on the other, the very fact that it is the ***exact*** "transliteration" of the cladogram makes it, in fact, glaringly ***useless***: containing (by definition!) no information beyond that (concerning genealogy) presented already (better!) by the source cladogram it is obviously superfluous! ***Any other*** prediction inferred from cladistic classification (unless identical to that based on a synthetic system) is (often by far…) ***less*** reliable [the (synthetic) statement that *Latimeria* belongs to **Pisces** (*i.e.* to a group containing also sharks, sturgeons, herrings, eels, *&c.*) allows to rather precisely deduce most of its (morphological, physiological, ecological and whatever we wish) characteristics – but what can we "predict" about it from the equivalent cladistic information that it is (together with toads, pythons, brontosaurs, kolibris and moles) a member of **Tetrapoda**???]! One could say (as indeed cladists do) that, since reconstruction of branching pattern is based on apomorphies and any character is an apomorphy of more or less inclusive clade, cladistic classification "predicts" (contains information on) all characters of all organisms; alas! this reasoning is also evidently "strained": if we compare the information content of the ***entire system*** of all the organic world, on the bafflingly irrealistic assumption that all characters of all groups (all apomorphies at all levels) are known, then the "predictive power" of any (no matter whether cladistic, phenetic, or synthetic) classification is of course equal (in fact, equal ***to zero***: there is nothing left to be "predicted"…) – the only comparison making sense is that between the predictions (based on what is ***really*** known about the included organisms!) of the taxa at that level of inclusiveness where the rival classifications ***differ*** (in this case the genus *Latimeria* or phylum **Chordata** is common to both, the critical difference being **Tetrapoda** *vs.* **Pisces**)! And in this comparison the superiority of syntetic classification is incontrovertible: "*Latimeria looks like a fish, tastes like a fish, behaves like a fish, and thus – in some legitimate, exceeding narrowly understood tradition, sense – it* ***is*** *a fish*" (GOULD 1991)!

Thus, attempts to make cladistic classifications "exactly" informative lead to heavy ***loss*** of both the informativeness and exactitude: they exactly "predict" only the (correct or not…) cladograms they have been based on, while no other inference is any more (usually less) exact than if drawn from synthetic system. But cladistic dogmas offend also against other two basic principles of biological research: that the organic world should be described as it is (not as somebody would like it to be), and that this description should make biological (not only philosophical) sense! Anxiety for "mathematically neat" (exactly reflecting genealogy) classification led cladists to negate the existence of paraphyletic groups; consequently – "by definition" paraphyletic – ancestors must have been declared non-existent too: no taxon can be ancestor of another taxon, ancestral taxa do not exist, descendants have no ancestors (so, ***whose*** descendants are they?)!!! Such assertion would be perfectly acceptable for a creationist, but no mean philosophical jugglery is necessary to reconcile it with belief in evolution… The trick is either to "dismember" the ancestral taxon until all the ***known*** subtaxa can be distributed among the resulting holophyletic groups (the – of course having once existed but yet unknown – "less inclusive" ancestors being simply ignored…), or to cram the descendant into the ancestor. It is a popular "joke for serious" among cladists to announce: "*today I had a dinosaur for lunch*", intended to mean that he/she consumed a chicken [indeed, for GHISELIN (1997) the very "touchstone" of one's being "*a real evolutionary biologist*" is the answer to the question "*when ... did the dinosaurs become extinct?*": the "real evolutionary" response is "*they never did ... they are still flying around in the trees*"]. The conclusion that birds ***are*** dinosaurs because they [supposedly] ***originated from*** dinosaurs is ingenious indeed, but it is a pity that its protagonists do not pursue this track further back: dinosaurs (through some primitive reptilians) originated from **Amphibia**, so they ***are*** amphibians; but amphibians originated from (and thence ***are***) fishes; but fishes … *&c.*, *&c.*, *&c.*, down to bacteria – so, in fact, what the facetious cladist ate was a bacterium (indeed, he himself – like me and you, dear Reader! – is a bacterium; all the organic world consists of ***nothing but bacteria***!). That a crocodile has much more in common with a lizard than with a swallow, that *Latimeria* "*looks, tastes, behaves like a* ***fish***" and not like a giraffe or pterosaur, that no bacterium resembles in virtually anything a butterfly or a squid – does not matter: the only important is that **Reptilia** not including **Aves** would be their ancestors, like **Pisces** for **Tetrapoda**, or **Bacteria** for animals and plants, and no taxon can be ancestor of another taxon! Why? – because paraphyletic taxa do not exist; why not? – because what is paraphyletic is not a taxon; why? – of course because no taxon can be ancestor of another taxon… That the common ancestor of insects, crustaceans, cheliceratans ***must*** have existed – does not matter: it did belong to the **Arthropoda** but ***not*** to ***any*** class, order, family, genus or species [once in Precambrium there lived a primitive arthropodan, say, *Protarthropodus verus*, member of the family **Protarthropodidae**, order **Protarthropodomorpha**, class **Protarthropoda**; later on some of its populations evolved further into divergent lineages, and at that very moment… the class, order, family, genus and species ***retroactively*** disappeared: not only they did not exist any more in Cambrium and thereafter, but their existence has been "erased" even from the Precambrian past!!!]. Maybe it is good philosophy, but good biology it is certainly not…

Somewhat related is the problem of species-concept: here also the discussions are dominated by exertion *per fas et nefas* for "strictness" or "objectivity" rather than accurate description of real taxonomic structures, and by disregard of observable facts in favour of metaphysical speculations. The situation is somewhat strange: half a century ago the agreement as to what is and what is not a species was better than it is now. The apparent reason is that nowadays the question – like many others in theoretical biology – has been "overphilosophied", overwhelmed by divagations on "*silly talks*" of "*natural selection acting upon*" "*the genotype, the phenotype, or both*" ("*selection is not an agent, and therefore cannot 'act upon' anything*"); on whether "*competition*" is "*interaction*" or rather "*relationship*" or "*situation or a standing-in-relation*" or they "*might better be called competitive 'interaffections'*" ("*because the competitors do not have to 'act upon' each other*"); on the metaphysical difference between "*class*" and "*individual*" and whether or not "*everything that falls under such* ["*bona fide*"] *an* *ontological category is either a class or an individual*" [the examples are from GHISELIN's (1997) book "*Metaphysics and the origin of species*" which is indeed the right place for these and similar considerations, but unfortunately it is enough to consult almost any relevant publication to ascertain that they are not restricted to explicitly "philosophically-minded" deliberations: in most disputes on theoretical aspects of (not only!) taxonomy observable facts are being obstinately stretched to fit the Procrustean bed of various philosophical preconceptions rather than these latter being evaluated according to empirically perceived reality…].

But is there indeed any "empirically perceived reality" about species? Is it not merely a convenient term which we can define according to our will and pleasure? The answer depends upon whether we consider it as something objectively (independently of our will, perception and knowledge) existing, as an "artifact" made by man, or as a product of our imagination. In the latter case – as in that of UFO-s, Liliputians, philosopher's stones, astral energies – the "diagnostic characters" and "properties" depend only upon our phantasy: there is no possibility to observe, and consequently no sense to debate, whether the Aladin's lamp "was indeed" made of copper, whether Mickey Mouse "truly" was a mouse (*i.e.* belonged to the family **Muridae**), or whether ghosts are "really always" invisible – the inventor of such a "being" is absolutely free to choose whatever definition and characterization he/she likes. In the case of veritable objects the "empirically perceivable reality" evidently does exist (they ***actually*** and ***verifiably*** either are or are not bluish-green, weigh either more or less than 20 kilograms, either consist or not of separable pieces), and our definitions must be based on these ***real*** properties; there is only some difference between man-made and naturally originated objects as to what kind of properties is most appropriate as the basis of the best ***"general purpose"*** classification (of course ***any*** feature may be the most suitable in some ***special*** situation) and – consequently – as criteria to define its units. Artifacts have been invented and produced for our own purposes, so their "natural" subdivision is that based on the way we use them: we usually speak in terms of such primary "categories" as chairs (to sit on), tables (at which we eat or write), cupboards (for keeping clothes), *&c.* – and group them into similarly motivated higher "taxa" (in this case "furniture") – rather than in terms of (irrelevant in most situations) colour, shape, date of production or number of "members". For all the remaining elements of the world (those really existing without having been made by man) no simple sorting criterium is obviously "the best" – the only generally applicable quality to distinguish a good (useful, meaningful) from worse grouping is its "predictive power": the amount of information ("weighted" by the degree of reliability) about particular members of the group, not contained "directly" in their definitions but derivable from the fact of the membership [if ***X*** belongs to the group ***A*** according to the classification **I** and to ***B*** according to **II**, and more characteristics of ***X*** can be more reliably inferred from the fact of its being a member of ***A*** then from its inclusion into ***B***, then the classification **I** is better than **II**; *e.g.*, much more interesting informations on mercury follow from its being a metal than from its room-temperature liquidity, so the subdivision of substances into metals and non-metals is – from the viewpoint of predictive power – better than according to the state of condensation].

There is much argument as to whether taxa are or are not "real", but it is not always clear what the debate is about: for at least some disputants "real" seems to mean "having stable content and sharply defined limits". However, beyond the realm of philosophy, formal logic, and mathematics stable content and sharply defined limits are rare exceptions rather than rule: I hope, nobody would deny that Poland is a "real" state, having existed since more than 1000 years, and that it was the same state in times of Bolesław Chrobry (when it included what is now Slovakia, Bohemia and southeastern Germany) as in the XV century (when its eastern border run close to Moscow, far beyond Smolensk and Kiew, and on the South reached the Black Sea and Lower Danube), in times of Napoleon (when it was restricted to a small east-central part of its present area), and even in some very ***real*** (though here irrelevant) sense between 1815 and 1918 (when all its territory was divided between Russia, Prussia and Austria); also real is Israel, irrespective of whether Cisjordania or the Ghaza Zone is or is not considered part of it; Atlantic Ocean is a reality, despite having no clear-cut borders *vs.* Pacific, Indian, or Arctic; and so there is no reason to deny the reality of *Daphnia pulex*, **Rosaceae** or **Mammalia** only because there is no full agreement as to their limits and content. The situation with general terms like state, ocean, or genus is similar: we may disagree as to the exact definition of, say, language, or the criteria to distinguish a language from a dialect (is Chinese a language with many dialects, or is it a group of many languages?), but nevertheless language as such is a real category (languages exist independently of whether we know of them or not); and similarly real category is genus, notwithstanding the lack of consensus as to whether all tits belong to *Parus* or only Coal Tit and some of its closest relatives do.

As to species, it is usually considered "more real" than other categories, and indeed its reality is convincingly proven by the fact that people having no biological training – who have never heard of species-concepts, allopatry, monophyly, synapomorphies, ontology, *&c.* – distinguish (within their area of interest) usually the same taxa as do professional taxonomists: a fisherman's species-level classification of (known to him) fishes, or a hunter's of wild ducks, does not significantly differ from that "officially" accepted, and also *e.g.* New Guinean Papuans recognized almost exactly the same (136 out of 137) bird species inhabiting Arfak Mts. as did ornithologists (MAYR 1968). It is sometimes said, that particular species (taxa) are real, but the general notion (category) of species is not – this, however, is again some conceptual juggle (or at least hairsplitting), perhaps significant in metaphysical considerations but not making much sense in biological context: if species-level taxa are almost perfectly the same for a Swedish fisherman, Chinese farmer, or Papuan native hunter on the one hand and modern taxonomist on the other, their notion of species-category (though of course they do not use any of these professional terms) must also have been roughly the same, what would be hardly conceivable if it is not real! And if species ***is*** real, then the respective "empirically perceivable reality" ***does*** exist, and the "concept" should be based on that reality, *i.e.* on the observation of properties and behaviour of real taxa in nature (whether, how, and in which situations they vary, change, hybridize, evolve, co-occur, compete, *&c.*, *&c.*, *&c.*) rather than on metaphysical theorizing; in other words, the direction of inference should be from facts to general idea (and then – if somebody is interested – to philosophy: "observations show that the majority of what has been considered species shows such-and-such attributes and behaves so-and-so, thence the most fitting species-concept is this-and-this, what means that in terms of ontology it should be considered that-and-that"), not the opposite (*e.g.* "ontologically species are [say] individuals, therefore our concept is the only acceptable and the attributes must be such-and-such"). I do not find it either necessary or useful to enter the dispute on "individuals" *vs.* "classes" and similar: as, in my opinion, such considerations are secondary to (should be based on) ***previously*** developed ***biologically*** meaningful definition of species, for a biologist it is better to put up with the traditional meaning of the terms – individuals are individuals, classes are classes, and species are species (or – if a classification into a "higher category" is needed – taxa)! Unfortunately, most discussions (even those specially arranged to enhance mutual understanding – see *e.g.* WHEELER & MEIER 2000) have been dominated by the "metaphysics first" style of reasoning, what – in my opinion – ***assures*** their fruitlessness: the adherents of particular views repeat the same arguments in almost the same formulation, hitting each other with invectives like "non-evolutionary" or "typological", but paying apparently little attention to the real (***biological***) meaning of the opponents' arguments…

The most important observable fact about species is that they are indeed ***distinct***: the range of variability of one does not overlap that of another, being separated by more or less evident hiatus, and this pattern is retained from generation to generation: the daughters and sons are not identical to parents, but the differences remain within roughly the same limits. Moreover, palaeontological, genetical, ethological *&c.* data support the theoretical prediction that "overall" changes are slow, gradual (at least in terms of short periods of time), and as a rule ***divergent***: distinct species become more and more dissimilar and but rarely – in most groups practically never – re-mingle into one. These facts have been summarized by SIMPSON (1961) as "evolutionary species concept": species is a basic unit of evolution, "*a lineage (an ancestral-descendant sequence of populations) evolving separately from others and with its own unitary evolutionary role and tendencies*"; however, he himself observed that the "lineage" is not quite fitting term ("*if you start at any point in the sequence and follow the line backward through time, there is no place where the definition ceases to apply*", *e.g.* "*you can start with man and run back to a protist still in the species Homo sapiens*"). As "*such classification is manifestly both useless and somehow wrong in princi-*

ple", he recognized that "*the lineage must be chopped into segments*" – and indeed, substituting the word "*lineage*" with "***taxonomically meaningful segment of*** a lineage", we arrive at "***the***" species concept: the most general definition compatible with all known facts and corresponding very well with the traditional meaning of the word [I use the words "***definition***" and "***concept***" (interchangeably) in the meaning of set of conditions that the object in question must satisfy to deserve recognition as species – irrespective of whether we have or have not any reliable ***criteria*** allowing to verify the compliance with these conditions in particular cases; and indeed, expressions like "*evolving separately*", "*unitary role and tendencies*" or "*taxonomically meaningful*" are evidently too vague and/or subjective to be directly helpful in solving actual problems].

To make the stipulation of "separate evolution" somewhat more operational we should ask what are the mechanisms assuring – in the face of omnipresent variability in space and time – intraspecific integrity on the one hand and interspecific disjunction on the other? In the original version of the "evolutionary concept" SIMPSON (1951) himself named such mechanism, defining species as "*a phyletic lineage (an ancestral-descendant sequence of* ***interbreeding*** [emphasis mine – RBH] *populations) ... &c.*"; I do not know why ten years later he omitted the word "*interbreeding*" – perhaps he considered it self-evident – but this is the clue to the problem (at least in case of sexually reproducing organisms: whether or not asexuals form species – and, if so, what are the factors responsible for their internal cohesion – remains unclear), the "keystone" of perhaps most widely accepted MAYR's (1940) "biological concept": "*species are groups of actually or potentially interbreeding natural populations, which are reproductively isolated from other such groups*". MAYR (1940) conceived species as "non-dimensional", so his definition was intended as "synchronic" (applicable only to the situation at a particular moment in time – *e.g.* present) but, as SIMPSON's (1951) formulation shows, the same idea can be used to improve the precision of "diachronic", evolutionary version. On the other hand, as an operational criterion the "biological concept" is not directly applicable to either allochronic or allopatric populations: the "***potential*** interbreeding" is a hopelessly abstract theoretical concept, virtually useless in the interpretation of particular taxonomic relations – it is practically impossible to prove the ***lack*** of intrinsic reproductive isolation between organisms that never meet (experiments in captivity, under unnatural conditions, being obviously inconclusive), and its ***existence*** can also be convincingly demonstrated only in rare (at least among closely related forms) cases of crude genetical, embriological, or morphological incompatibility. Obviously, the best way to escape from this "*cul-de-sac*" is the abandonment of the word "potentially" in MAYR's definition, *i.e.* acceptation of geographical – or, more exactly, spatial – isolation (on an equal footing with other kinds of reproductive barriers) as important criterion of specific level of divergence. As I (HOŁYŃSKI 1977, 1992) have pointed out, there is no serious theoretical reason for the discrimination of territorial isolation in this respect: species is a unit of evolution, and thus "*evolv-*

ing separately from others and with its own unitary evolutionary role and tendencies" (SIMPSON 1961); the ***kind*** of the isolating mechanism that assures the independence of evolutionary development in particular case is obviously irrelevant – it is only its efficiency that counts, and geographical barriers are at least as efficient as others!

However, it would be obviously impracticable to treat any isolated population as a separate species – this would inevitably lead to nomenclatural and taxonomic chaos – so it seems most advisable to ascribe the species rank to only those, which have already run along their divergent evolutionary paths so far away that the reversal seems impossible. As far as sympatric and synchronic bisexual forms are concerned, this in fact amounts to no more than a different formulation of MAYR's (1940) "principle of reproductive isolation", but in the case of allopatric (allochronic, parthenogenetic) populations evolutionary irreversibility can be judged only on the grounds of phenotypic (in practice, almost always morphological) criteria [to be sure, reproductive isolation between ***sympatric*** forms is also in but rarest instances actually observed, having been almost invariably deduced from the existence of morphological hiatus (to avoid misinterpretation please note, that I use the word "morphology" in its traditional, broad sense: while for some authors "morphological difference" means only the disparity in shape or structure, I include here all those features – form, size, sculpture, colour, *&c.* – detectable on a motionless specimen by the sense of vision)]. Theoretically, the differentiation of genotypes can be considered irreversible if at least one of the alleles, [or combination of alleles ("supergene")] fixed (present in all individuals) in one population does never occur in the other; in practice, such a disparity in gene pools remains undetectable: we can only observe and assess its phenotypic manifestations. Consequently, I have proposed to accept explicitly (it is generally accepted tacitly) a very simple, workable, and at the same time remarkably precise and theoretically sound "morphoevolutionary" criterion of "full species status" – ***constant morphological differences***: allopatric species is a group of populations consisting of unequivocally determinable (on the basis of morphology) individuals. Such an approach, suggested by me (HOŁYŃSKI 1977) almost thirty years ago, had been later independently formulated by CRACRAFT (1983) and is recently achieving acceptance (NIXON & WHEELER 1990; ZINK & MCKITTRICK 1995; WHEELER & PLATNICK 2000a,b; PLATNICK & WHEELER 2000; *&c.*) as "phylogenetic species concept" [species is "*the smallest aggregation of populations (sexual) or lineages (asexual) diagnosable by a unique combination of character states in comparable individuals (semaphoronts)*" – NIXON & WHEELER 1990] – the important differences are: [1.] what I propose is ***not*** a separate "species concept", but only an operational ***criterion*** applied (especially to allopatric or allochronic populations) under "evolutionary species concept"; [2.] while "phylogenetic taxonomists" do not accept the category of subspecies, what either leads to loss of much information on geographical structure or induces attribution of species status to [groups of] populations fully diagnosable in the "cores" of their areas but connected by zones of intergradation [see COLLINSON (2001) for more extensive discussion], my "morphoevolutionary diagnosis" demands discreteness (lack of intermediate populations or – except ***occasional*** hybrids – individuals): intergrading forms being classified as subspecies.

The equivalent criterion for allochronic populations is suggested by SIMPSON (1961) as his concept of "*unitary evolutionary role*". In his (and that of virtually all his opponents) interpretation, the "chopping" of a lineage into [taxonomically meaningful] segments "*must be done arbitrarily ..., because there is no non-arbitrary way to subdivide a continuous line*", what is notoriously interpreted as making "evolutionary" species "not real". However, as already pointed out above, the equation of "non-arbitrariness" with sharply defined limits would in practice make almost everything in the biological world "unreal": as an obvious consequence of the very fact of evolution sharp, "objective" delimitations are usually (in case of taxa virtually always!) impossible; on the other hand, if less restrictive criteria are accepted, then the reality of "diachronic" species diagnosed by its "*unitary evolutionary role*" does not leave much room for serious doubts. "*Roles are definable by their equivalence to niches*". "*The role cannot be directly observed in a series of dead specimens, recent or fossil, in a museum. Valid and sufficient evidence of separation and unity of roles can, however, be obtained from observation on such specimens. Morphological resemblances and differences (as reflected in populations, not individuals) are related to roles if they are adaptive in nature*". "*Successive species should be so defined as to make the morphological difference between them at least as great as sequential differences among contemporaneous species of the same group or closely allied groups*" [*i.e.*] "*When in a lineage the inferred ranges of observed changing characters for populations at two times do not overlap, those populations may be placed in different successive species and the division between them drawn approximately midway (in time* [I would rather prefer "midway" in terms of morphology]*) between them*" (SIMPSON 1961). So we arrived again at the "morphoevolutionary" approach as in case of allopatry, with the same criterion of non-overlapping morphology.

Surely the "diagnosability criterion" is not a perfect panacea: it does not fully exclude the reversibility of transspeciation, makes allopatric sibling species practically undetectable, some purely phenotypic differences may be mistaken for inherited "taxonomic characters", *&c.* These are evidently very serious shortcomings as compared to some ideal procedure allowing to establish the evolutionary status of any population beyond doubt. Unfortunately, such a procedure does not exist, the currently accepted practice of evaluating the diversification of allopatric populations against the "scale" of differences observed between sympatric taxa has all the above-mentioned flaws and several – more important! – additional ones: it is highly subjective equivocal criterion, based at that on series of totally false assumptions [that phenotypic disparities in sympatry develop comparably to those occurring between geographically isolated forms, that the degree of morphological differentiation is a reliable indicator of intrinsic reproductive isolation, that the evolutionary (and, consequently, taxonomic) meaning of the latter is the same for sympatric forms as for those never meeting in nature, *&c.*]; in this comparison, the "determinability rule" seems almost faultless...

To recapitulate, I advocate (and follow in my works – for more detailed discussion see Hołyński 1992) three complementary definitions of species:

(1) **evolutionary** ***concept*** [a modification of Simpson's (1951, 1961) evolutionary definition]: "***species are segments of phyletic lineages (ancestral-descendant sequences of populations), whose gene pools have differentiated beyond limits of reversibility***"; this is the most general formulation, closest to the basic tenets and applicable in principle to all groups of organisms, but transgression of the limits of reversibility is very seldom directly demonstrable in actual cases, so usually we must try the second-choice

(2) **reproductive** ***criterion*** [an adaptation of Mayr's (1940) "biological" definition]: "***species are groups of interbreeding populations, reproductively isolated by intrinsic mechanisms from other such groups***"; among allopatric, allochronic, parthenogenetic, *&c.* populations interbreeding obviously does not occur, and the proof or disproof of intrinsic reproductive isolation is either [almost] never possible or irrelevant, so this definition can be actually applied to co-existing bisexual forms only; however, even in overwhelming majority of such instances we have no data on the reproductive isolation as such – this can only be inferred from the observed phenotypical (usually morphological) hiatus, what in fact means recurrence to the only universally serviceable

(3) **morphoevolutionary** ***diagnosis*** [a re-formulation of Hołyński's (1977, 1992) "operational" definition]: "***species are discrete groups of populations, showing – at least in one class (sex, caste, developmental stage) of individuals – consistent unique combinations of morphological characters***".

For less differentiated populations Amadon's (1949) rule should be applied: if more than 75% of specimens are determinable, we have to do with a subspecies; if less, the form at issue does not warrant taxonomical recognition at all.

So, in my opinion (Hołyński 1977, 1992, *&c.*) there is only one – the "evolutionary" as formulated by Simpson (1951, 1961) – valid species-***concept***: all the others ("Phenetic", "Genotypic", "Phylogenetic", "Ecological", "Biological" *&c.*, *&c.*, *&c.*) are in fact but various criteria proposed to decide whether or not particular complex of populations has already developed the mechanisms necessary to assure its further "*evolving separately from others and with its own unitary evolutionary role and tendencies*". This is essentially the same idea as that recently presented as "general lineage concept of species" ("*I do not mean to say that there are no conceptual differences among the diverse contemporary species definitions but rather that the differences in question do not reflect differences in the general concept of what kind of entity is designated by the term species. All modern species definitions either explicitly or implicitly equate species with segments of population level evolutionary lineages*" and so "*are special cases of the general lineage concept*") by de Queiroz (1998).

CLOSING REMARK

As mentioned at the beginning, and anyway evident, this paper is not aimed as a "treatise of philosophy of systematics", nor even as comprehensive presentation of my own views, but only as critical comments on ***some*** aspects of the theory and methodology of taxonomic studies and their consequences for the interpretation of the results of our work, as exemplified by (also ***far from*** comprehensive!) discussion of two basic but controversial topics (principles of classification and species concept). We are living in the XXI century, and already the XIX was dubbed "Age of Science", so even though I seldom (except for wordly citations) quote (often am not even specifically aware of) original authors of particular statements, suggestions, or arguments, very few – perhaps none – of them is truly "mine" in the sense of having never been formulated before, by other scientists; if I nevertheless consider these divagations sufficiently "innovatory" to warrant publication and discussion, it is because not so much individual elements of reasoning, as their selection and combination decides of the value and originality of a conception as a whole – and in this meaning the "mineness" of the "philosophy of systematics" as outlined above is, I dare to believe, defensible. To what extent defensible is also its soundness? – this question can only be elucidated in the course of the discussion which (I hope!) this paper will provoke…

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