

Some Thoughts on the Taxonomy of *Hieracium*

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Summary

In *Hieracium* two taxonomic schools exist with rather different and often not transferable concepts: (1) Every recognizable taxon is named on species level or (2) the vast amount of taxa is grouped in collective species. Two recent attempts to clarify the *Hieracium* taxonomy are discussed on the background of the situation in Central Europe: the treatment in Flora Europaea trying to find a compromise between the two schools and the recent concept of TYLER for the subgenus *Pilosella* in Scandinavia recognizing only a very limited number of species. Both concepts do not account for the varied reproduction systems occurring in *Hieracium*.

For the taxonomic handling of *Hieracium* is made attentive to some necessary presumptions. The collective species of the Central European school are from their concept not intended to be artificial taxa. They have been created as morphological units with an at least partly common descent, a similar distribution area and a comparable ecology. Genetic isolation cannot be considered as a species criterion in taxa that reproduce by obligate or facultative apomixis. For practical reasons only taxa rich in information should be treated as (micro)species. The minimum area concept applied in *Rubus* is for several reasons not helpful in *Hieracium* as a prerequisite for the recognition of species.

Different taxonomic approaches are suggested for hybrids to reflect their different reproduction behaviour. However, their inclusion into red lists is explicitly recommended as they can be starting points for new taxa. For established taxa dependent on the degree of their morphological and biological isolation the ranks 'species' or 'subspecies' should be used respectively. Independently of used ranks the circumscription of the supraspecific taxa decides on the information density of a system of microspecies.

Zusammenfassung

In der *Hieracium*-Taxonomie stehen sich die beiden Richtungen der „Kleinarten-Schule“ und der „mitteleuropäischen Schule“ schroff und mit teilweise nicht ineinander überführbaren Einheiten gegenüber. Als neuere taxonomische Entwürfe werden der Kompromissversuch in Flora Europaea und die stark zusammenfassende Darstellung der Untergattung *Pilosella* für Skandinavien von TYLER skizziert. Die Nachteile beider Ansätze werden aus mitteleuropäischer Sicht aufgezeigt: Vor allem die sehr starre Sicht der Biologie der Arten erschwert einen pragmatischen Blick auf die Sippen und ihre Bewertung.

Für die taxonomische Behandlung von *Hieracium* wird auf einige notwendige Annahmen aufmerksam gemacht: Die Sammelarten der mitteleuropäischen Schule sind nach ihrer Idee keine rein künstlichen Taxa, sondern morphologische Einheiten mit gemeinsamer (Teil-)Abstammung, einem ähnlichen Areal und vergleichbarer ökologischer Einnischung. Die genetische Isolation kann kein Rangstufen-Kriterium bei obligat oder fakultativ apomiktischen Taxa sein. Aus Gründen der Praktikabilität und Übersicht sollten nur Informationsreiche Taxa als (Klein-)Arten geführt werden. Die Fassung von Taxa als Arten wie bei *Rubus* von einer bestimmten Mindestgröße des Areals abhängig zu machen, ist bei *Hieracium* nicht praktikabel.

Für Hybriden werden je nach ihrem Verhalten unterschiedliche taxonomische Vorgehensweisen vorgeschlagen; allerdings wird ihre Aufnahme in Rote Listen ausdrücklich befürwortet, da Hybriden

potentielle Ausgangspunkte zur Bildung neuer Sippen sind. Für die etablierten Sippen wird abhängig vom Grad ihrer Selbständigkeit die Beibehaltung der Rangstufen von Art und Unterart vorgeschlagen. Unabhängig von den benutzten Rangstufen entscheiden Fassung und Umgrenzung der übergeordneten Taxa über den Informationsgehalt eines Systems aus Kleinarten.

Sometimes, the discussion about the taxonomic position of apomictically fixed units in *Hieracium* seems rather political than scientific. STACE (1998) states without further argument: "However these [infraspecific] ranks have now been generally abandoned and replaced by the species, a change in agreement with the theme of this paper."

The outstanding „Systematik als Ideengeschichte“ of HUBER (1995) should make aware that everyone's systematic decisions as well as the evaluation of former systematic viewpoints are dependent upon the person's sociopolitical background. Thus, how great is the influence of the „Zeitgeist“ changing in time and range e.g. for *Hieracium* taxonomy?

After these marginal remarks I will give a short historical survey of *Hieracium* taxonomy, sketch present-day concepts and, finally, will present my own thoughts. The nomenclature of collective species follows GOTTSCHLICH 1998, the subspecies are named after ZAHN 1922–1938.

The history of *Hieracium* taxonomy starts with a description of the most important forms. They correspond to present-day basic species („Hauptarten“) or are often types of sections. In the 19th century, what we call today microspecies were described, e.g. by TAUSCH, JORDAN or ARVET-TOUVET. With their monograph of the Piloselloids, NÄGELI & PETER (1885) tried to put in order the permanently increasing number of species:

1. They considered their approximately 2800 described biotypes as the true taxa. However, they decided to use the rank of the subspecies in order "to bring the genus *Hieracium* in agreement with the arrangement practiced in the entire plant kingdom" [„die Gattung *Hieracium* bezüglich ihrer Gliederung mit der im ganzen Pflanzenreich üblichen Eintheilung in Uebereinstimmung zu bringen“.]

2. To group the 2800 taxa they introduced the ranks „Hauptart“ [principal or basic species] and „Zwischenart“ [intermediate species]. „Zwischenarten“ display characters that are intermediate between two or several „Hauptarten“. By this taxonomic procedure a framework was created that allowed to assign a taxon to a „Hauptart“ or a „Zwischenart“ according to its morphological characters. The origin of the intermediate species remained unexplained in principle. Only in some cases, a certain origin was postulated: recent or historical hybridisation, or evolutionary variation.

Retrospectively, the introduction and the characterization of the intermediate species proved to be not successful. Far too often, they are simply regarded as being hybrids. The subgenus *Pilosella* received the reputation of a group undergoing permanent hybridisation. Later ZAHN continued in developing this system but added nothing substantially new. Outside Central Europe (for example in Britain, Scandinavia or Russia) the system of NÄGELI & PETER and ZAHN was hardly used. In these regions the microspecies concept prevailed.

Two disadvantages of the microspecies concept must be mentioned:

1. The model of ZAHN offers a quick orientation based on morphological characters and of the supposed parentage of a taxon. For instance: *Hieracium [murorum*]* NN., on Scandinavian labels, in contrast with *Hieracium fuscocinereum* ssp. NN., nomenclature after ZAHN. In the first case, nothing is known about the taxon NN., unless one is very familiar with thousands of Scandinavian taxa. In the second case, one immediately knows that it is a Scandinavian taxon with the growth form of *H. murorum*, with relatively long hairs and shorter glands on the involucre and many stellate hairs on the involucre bract margins.

2. The second disadvantage becomes obvious by looking at the history of *Hieracium* taxonomy in Scandinavia: After a period of intense taxonomic activity and the description of thousands of microspecies nearly nothing was done for decades. The majority of the Scandinavian *Hieracium* microspecies are really unknown today. Moreover, the Scandinavian supraspecific taxa are circumscribed rather wide, a fact that has further decreased their detailed knowledge.

Recently, two revolutionary approaches in *Hieracium* taxonomy were presented.

In the Flora Europaea (SELL & WEST 1976), the two subgenera are treated as clearly different genera: *Pilosella* is regarded as almost completely sexual; all intermediate taxa are regarded as hybrids, with the

spectacular exceptions of *H. flagellare* and *H. sphaerocephalum. Hieracium*, on the other hand, is considered to be a completely apogamic complex. Therefore, an exemplary selection of Zahn's subspecies are regarded as microspecies, but many others are not mentioned at all.

The differences in the reproduction biology between the two subgenera *Hieracium* and *Pilosella* are not so fundamentally, as from SELL & WEST (1976) presupposed. In both subgenera sexual diploids and partly apomictic polyploids exist and reticulate distribution of characters can be observed. Differences are more noticeable compared to the American subgenus *Chionoracium*. However, the ratio of sexual to apomictic forms and the mode of apomixis are different in the two subgenera: Only in subgenus *Pilosella* (in Central Europe) recent hybridisations are proven to occur. In subgenus *Hieracium* the observed diversity must be the result of a process which is no longer or only rarely verifiable.

A schematic solution is of no advantage, even if it seems new. After the scheme of SELL & WEST there would occur in Bavaria about twenty (in the Eastern Alps still some more) mostly sympatric microspecies of one *H. glaucum*-series which are very imprecisely defined. In subgenus *Pilosella* the concept is too schematic as well: It ignores isolated well defined taxa. In Central Europe among others the following complexes of „intermediate species“ seem to be stable and well defined: *H. visianii*, *H. brachiatum* ssp. *villarsii*, *H. fallax*, *H. spurium* (ssp. *tubulatum*), *H. fallacinum*, *H. zizianum*, *H. densiflorum*, *H. glomeratum*, *H. macrantbelum*, *H. floribundum*, *H. iseranum*, *H. guthnikianum*, *H. fuscescens*, *H. rubrum*, *H. nothum*, *H. fuscum* and *H. stoloniflorum*. Some of these (and other) collective species contain taxa which are apomictically fixed and separated by morphological, chorological, and ecological characters, e.g. *H. densiflorum* ssp. *baubiniifolium*, *H. floribundum* ssp. *suecicum*, *H. guthnikianum* ssp. *rubrisabinum* and ssp. *algovicum*, *H. baubini* ssp. *radiocaulis*.

The second revolutionary draft was presented by the Swede T. TYLER (2001a) for the subgenus *Pilosella* in Scandinavia. He presumes that isolation barriers between the species are virtually lacking and consequently regards hybridisation as a common phenomenon. The assignment of the subspecies to collective principle and intermediate species [Hauptarten and Zwischenarten] by Central European authors is regarded as speculative. He quotes three points of criticism against the concept of *Pilosella* in Flora Europaea:

(1) "... the knowledge about the origin of many of the ‚hybrids‘ is based on guesses at best and their morphological distinctness is in many cases very unclear". (2) He considers it not to be justifiable to define the 8 Scandinavian species in such a way that 27 intermediate taxa (of presumed hybrid origin) are more frequent and more widespread than the 'true' taxa. (3) With a species concept like this, it cannot be decided, which morphotypes are primary species and which originated through hybridisation.

Therefore, he comes to the following concept (TYLER 2001 b):

Species are groups of morphotypes sharing several unique morphological characters or character-combinations, whose intermediates (presumed hybrids) are rare and seemingly unfit in natural habitats.

Subspecies are groups of morphotypes sharing several unique morphological characters or character-combinations, but whose intermediates are relatively common and obviously stable in natural habitats.

Varieties are groups of morphotypes (or single morphotypes), that show but one (or very few) distinctive morphological characters and which share a particular ecology or distribution, distinct from those of the other varieties within a species.

The main objection against TYLER's concept is that it cannot be generally applied. In his idea like in that of Flora Europaea, LÖVE's aversion against hybrids might still be extant. On a symposium preparing Flora Europaea LÖVE (1960: 139) stated: "To classify such taxa [by agamospermy perpetuated hybrids] as species without indicating their hybrid origin, is an absurdity, which would never be allowed in amphimictic groups and this practice is the greatest cause of the disrepute, into which apomictic taxonomy as a whole has fallen." Without clearly saying it, the intermediate species of NÄGELI & PETER are regarded by him as hybrids. In my opinion a hybrid origin is not decisive for the evaluation of a species. Far more important is a regular reproductive behaviour and an ecological and chorological fixation. A look back into history might clarify this: Former taxonomists have treated taxa like *H. dentatum*, *H. calodon* or *H. fallax* as good species. It was the classification as intermediate species and the presumed hybrid nature that made these species vanish from floras and let them fall into oblivion.

TYLER's assertion that the assignment of infraspecific taxa to collective species is usually based on suppositions seems not to be correct. In *Hieracium*, the collective species created by NÄGELI & PETER and

ZAHN are—at least according to their ideas—not completely artificial products as are the collective species of MARKLUND in the *Ranunculus auricomus*-group. They are thought to be biotypes of joint origin, mostly with an own area of distribution and comparable ecological adaptations. The rejection of collective species as artificial units in *Ranunculus auricomus* by ERICSSON (1992) must not necessarily lead to the assumption that there are no natural collective species in *Hieracium*. A conceivable method could be the recognition of the collective species as natural defined taxa what would mean a re-examination and possibly new circumscription of the ones in use.

On species and partially on subspecies level, TYLER's system leads to a high loss of information. One could argue, that by his procedure the objects are only transferred to a lower (the variety) level. But even then, valuable information is lost. Some examples of taxa, which then cannot be separated taxonomically, are: *H. macranthelum*, *H. zizianum* ssp. *zizianum* var. *calvescens*, *H. densiflorum* ssp. *baubiniifolium*, *H. densiflorum* ssp. *cymosiforme*, *H. calodon* ssp. *phyllophorum*.

In my opinion both „revolutions“ do not lead to a better system. What can be done? In order to escape the unfruitful squabbling about microspecies and subspecies, I tend to take up the suggestion of VALENTINE & LÖVE (e.g. Löve 1960) to distinguish subspecies within the collective species, and add the appendage „apg“ for apogam if necessary. However, this is not accepted by the ICBN and has already been rejected categorically by the community of taxonomists.

My considerations originate rather from certain examples and from my own experience than from broad theoretical reflection. Before continuing, I like to recapitulate some facts well known for a long time however, which might clarify my thoughts:

1. Apomictic units do not fit the biological species criterion: “Groups of actually or potentially interbreeding natural populations, which are reproductively isolated from other such groups“ (MAYR 1942, quoted after STUESSY 1990: 164). I actually don't understand why the first half of the species criterion is usually not taken into consideration. The genetic isolation would mean for apomicts that each clone should be regarded as a species of its own. Apomixis is not consistent with the conventional concept of species or subspecies. Therefore, the genetic isolation should not be regarded as too important when establishing the taxonomic level. Overemphasizing it, brings about unsolvable problems even in amphimictic taxa, populations and single plants: e.g. *H. echioides* was observed within one population as di-, tri- and tetraploid (SCHUHWERK & LIPPERT 1997) or *H. racemosum* ssp. *leiopsis* as di- and triploid (SCHUHWERK & LIPPERT 1999). Even “in a single capitulum a combination of both amphimictic and apomictic seed production is relatively common“ (KRAHULCOVA et al. 2000: 323). Further problems arise from the joint occurrence of morphologically not distinguishable plants with different reproduction, being partly diploid sexual and partly polyploid apomictic, for example in *H. alpinum* ssp. *alpinum*, *H. echioides*, *H. cymosum* ssp. *nestleri*. However, the stability of the reproductive mode emphasized by HÖRANDL (1998) is important as it has a major influence on biotype stabilization.
2. The objections of American botanists (described by RICHARDS et al. 1996) against apomictic groups containing thousands of taxa may be transformed into a demand: We should consider only those microspecies as taxa that are sufficiently rich in information: morphologically distinguished and separated by different chorological and ecological characters.
3. Species are very different in their content even if one looks at strictly sexual species. Is this an argument, however, to define species even more different as it is done today? Anyway, when establishing the species numbers for check lists or for biodiversity indices a filter has to be used to reduce the number of apomictic microspecies according to a comparable size of sexual species.
4. DAVIS & HEYWOOD (1963: 384) consider it “quite evident, that no one system of classification will be possible for all groups of apomicts“. However, at least for Europe, I see a consensus in the taxonomic treatment of microspecies within apomictic groups. Is there a genus besides *Hieracium* where subspecies or lower ranks are still applied? Is it conceivable to treat *Hieracium* in opposition to the consensus of taxonomists? Central European hieraciologists are already subjected to the reproach of “intense backwardness“ (LOOS 1997). My reflections may not be misunderstood: I don't object consistency and agreement. But there should be arguments for it.

5. In the apomictic *Rubus* complex WEBER (recently 1996) has taken up and refined special criteria for the taxonomic treatment: Taxa are only considered as species if the distribution area exceeds 50 km in diameter. The procedure has often been misunderstood. It is not based on a new defined species concept, but rather establishes priorities for the practical taxonomic treatment. Occasionally, this procedure was adopted for other apomictic groups (e.g. STACE 1998 for *Hieracium*). The explicit identification of the effectivity of propagation agents of *Rubus*, *Hieracium* and *Taraxacum* is not justifiable without all verification. In *Hieracium*, however, the factors for the formation of distribution areas differ from those in *Rubus*. The assumptions of WEBER concerning *Rubus* are not applicable to *Hieracium*: Neither reveals *Hieracium* a progressive distribution tendency nor are relic endemics missing like in *Rubus*.

Before I present my own ideas I want to repeat that they are mainly based on own experience with certain taxa and populations observed in Bavaria and the Eastern Alps, not on broadly aimed, intensive theoretical considerations.

1. In hybridogenous taxa three situations can be distinguished, which probably need different treatment:
 - (a) Hybrid plants occurs only in low numbers between the parents or together with at least one of them. Examples from Bavaria are *H. x schultesii*, *H. x pilosellinum* or, only partially, *H. x brachiatum*.
 - (b) The hybrid forms a swarm linking both parental species nearly without a morphological gap but occurs sometimes apart from the parental species. An example is *H. niphostribes* [*H. niphobium*] between *H. glaciale* [*H. angustifolium* auct.] and *H. lactucella*.
 - (c) The hybrid seems to be fixed on the diploid level. From Bavaria, a single example is known to me, *H. hybridum calophyton* between *H. cymosum* and *H. peleterianum*, MERXMÜLLER (1975) mentions further hybrids.

In the first case the hybrid should be treated like in other outbreeding groups. In the second and third case, the situation and the populations must be analysed before making taxonomic decisions.

I would like to mention that at least in *Hieracium* hybrids are the starting point for speciation processes. Gene or genome mutations are of no great importance according to our knowledge. To exclude not yet fixed *Hieracium* hybrids explicitly from red lists (see GREGOR & MATZKE-HAJEK 2002) or from efforts of nature protection might prevent the development of new taxa. Diversity would then be frozen to a certain extent on the present status.

2. At the moment *Hieracium* taxonomy is in a transition period: Not yet all taxa of the collective species (or: microspecies of both subgenera) are analysed with regard to their morphological constancy, discrimination from similar taxa, mode and constancy of propagation, distribution area and ecological demands. In this situation it seems more appropriate that dubious taxa rather remain at a lower rank (i.e. the subspecies of ZAHN) than to give them the status of microspecies in advance and to delete some or very many of them after detailed investigation.
3. For fixed taxa, whose status is settled, I suggest two modes of treatment according to their degree of taxonomic independence.

Microspecies: Morphologically clearly distinct taxa, which exceed the framework of the collective species. Examples are the following pairs of taxa that should be separated as microspecies: *H. bifidum obscurisquamum* vs. *H. bifidum psammogenes* and *subcaesium*, *H. chondrillifolium* s. str. vs. *H. subspeciosum*, and *H. spurium tubulatum* vs. *H. spurium* from the Western Alps. Besides their morphological differences, these taxa presumably also differ in their origin.

Subspecies: Morphologically little deviating taxa with similar chorological and/or ecological behaviour.

It would be desirable if the newly circumscribed collective species (without the separated microspecies) could be limited more strictly concerning their morphology, chorology and ecology. Future collective species and supra-specific taxa (series, groups) should represent units of congenial origin. The nomenclature of these supra-specific units is a secondary question and should be discussed separately.

As a matter of course, the application of certain ranks is not the crucial point. Nobody should be called „pseudo-progressive“ just because he distinguishes only microspecies in *Hieracium*. It seems more

important to me, that well characterized biotypes can be assigned to supraspecific units corresponding to the collective species of ZAHN in scope and intention, even if – in some cases – their limitation has to be adjusted in detail. Scope and character of the supra-specific taxa decide about the information density of a classification in microspecies.

The careful assignment of single biotypes to collective species also helps to avoid incidental errors. By attaching the microspecies described by ARRIGONI (1985) from Sardinia to the system of ZAHN, already two of them could be recognized as synonyms: *H. gallurensis* Arrigoni = *H. racemosum* ssp. *crinitum* (SCHUHWERK & LIPPERT 1998), or *H. templare* Arrigoni = *H. bernardi* s. l. (SCHUHWERK unpublished).

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