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The ferns and allies of the Far West Himalaya: Some additions and corrections

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Abstract

Fraser-Jenkins C.R. 1992. The ferns and allies of the Far West Himalaya – some additions and corrections. Bot. Helv. 102: 143–157.

Selected nomenclatural and taxonomic revision is made in order to provide solutions to the problems raised in a previous paper (Fraser-Jenkins 1992) giving taxonomic and distributional details of all the pteridophytes of Pakistan including Kashmir. Species of the following genera are discussed further in a series of 33 numbered paragraphs: *Ophioglossum*, *Cheilanthes*, *Dennstaedtia*, *Cosentinia*, *Onychium*, *Thelypteris*, *Hypodematium*, *Gymnocarpium*, *Diplazium*, *Dryopteris*, *Cyrtomium*, *Asplenium*, *Loxogramme*, *Lepisorus* and *Marsilea*; and hybrids are discussed in *Cheilanthes*, *Diplazium* and *Asplenium*. New taxa described are *Cheilanthes leptolepis* Fraser-Jenkins, *Hypodematium crenatum* (Forssk.) Kuhn ssp. *loyalii* Fraser Jenkins & Khullar and *Cheilanthes* × *vermae* Fraser-Jenkins & Viane; *Aleuritopteris* Fée is combined new at subgeneric level.

As a result of many years of herbarium and field study on the ferns of Pakistan (including Indian occupied Kashmir) a parent paper has recently been published in Pakistan revising the seminal work of Dr. R. R. Stewart (1972) *etc.* (Fraser-Jenkins 1992). Since returning to Europe after nearly two years in the Himalaya it has been possible to carry out further, more detailed revision addressing some of the problems raised in the main paper, necessitating the following additions and corrections:

1. *Ophioglossum reticulatum* L. seems to be occurring as a somewhat small form of the species behind the Himalaya as well as in the plains and foothills. The specimens on which the E. Pak (NB) record of *O. polyphyllum* A. Br. ex Seub. was based belong here [one of the the two being from Skardu (not “Sharda”) to Damboodas] and it also occurs in E. Pak (HZ) (Chalt. A. Bosshard, F. Klötzli & R. Schaffner 122.02, 9/8/1989. ZT!). These higher altitude trans-Himalayan collections correspond in morphology with a small *O. petiolatum* Hooker, Exot. Flor. 1: t. 56 (1823), which I agree with Clausen (1938) to be merely a form of *O. reticulatum*, connected by an unbroken range of fertile intermediates. They also correspond exactly with *O. bucharicum* (B. & O. Fedtsch.) O. & B. Fedtsch., *Not. Syst. Herb. Hort. Petrop.* 4: 8 (1923) which appears to be another Soviet-central-Asian “pseudo-endemic”.

2. Since discovering *Cheilanthes bicolor* (Roxb. in Griff.) Griff. in Fraser-Jenkins as the name for "*C. farinosa*" *sensu auct. Ind.* I can now confirm that its basionym, *Pteris bicolor* Roxb. in Griff., is not a superfluous name for *C. dealbata* D. Don, *non* Pursh., nor is it synonymous with it. It is not to be used instead of *C. doniana* Ching & Wu, the *nomen novum* for *C. dealbata* (a central and east Himalayan species related to, but distinct from *C. anceps* Blanf.). *P. bicolor* was named and described by Roxburgh in (not ex) Griffith (Art. 46.2) and Griffith cannot be taken as its author but merely as its publisher. By subsequently inserting *C. dealbata* into the synonymy he did not affect the legitimacy of Roxburgh's name since Art. 63 defines a superfluous name very precisely as being one which, "as circumscribed by its author," includes the type of another name. Roxburgh, who died in 1815, rather than 1822 as I had stated following Clarke's (1874) introduction to his edition of Roxburgh's *Flora Indica*, could not have been the person who added Wallich's and Don's names into the synonymy of Roxburgh in Griffith (1844) since they did not come into existence until well after his death. It was presumably done by Wallich, or more likely Griffith, and Roxburgh himself did not circumscribe his name to include *C. dealbata*; hence *P. bicolor* is not superfluous. Roxburgh's type (holotype in BR!) is, as I stated, quite obviously the present distinctive, diploid species and cannot be confused, at least by anyone who knows the genus at all well, with any other species in that part of the subcontinent. Since writing the present note I am grateful to hear from Professor R. E. G. Pichi Sermolli, of Firenze, that he agrees entirely with this conclusion.

3. I have now re-examined the type of *Cheilanthes duthiei* Baker at Kew, which is a rather small, immature specimen, and compared it with different stages of the larger, more dissected plants from Nepal at BM (mentioned in the main text and also occurring in S. Tibet and Yunnan in PE!), as also with various stages of what was formerly known as *C. dalhousiae* auct. *non* Hooker from throughout the Himalaya. It is quite clear that the larger Nepalese etc. plants also belong to *C. duthiei*, which is a distinct species, and because of its typification problem, "*C. dalhousiae*" auct. (i.e. in the old sense), therefore lacks a name. The latter is thus described as a new species:

Cheilanthes leptolepis Fraser-Jenkins, **sp. nov.** *Morphologia aliquantum similis ad illam Cheilanthis bicoloris sed lamina tenuis farina candida semper omnino destituta, pinnula infima pinnae infimae paulum sed non valde longiora quam pinnula proxima et segmenta aliquot latiora tenuioraque. Stipites caespitosi crassi fragiles, paleis tenuibus, latis concoloris pallidis dispersis instructi pro parte maiore longitudinis eorum. Sori saepe separati, interdum contigui ad frondes maturas et magnas. Cytotypus diploideus, n=30 (Verma & Khullar, Caryologia 18: 85–106 (1965)). Holotype: India, Himachal Pradesh, 3 km. E. of Matiana on road from Simla to Narkanda, 2400 m. C. R. Fraser-Jenkins 6956, 6/9/1977 (BM). Paratypes: ditto. 6955, 6957 and 6960 (Herb. CRFJ, temporarily at BM).*

This species is in general slightly similar to *C. bicolor*, which with other species is often misidentified as *C. dalhousiae* when the farina has faded due to inadequate speed of drying in the field, or has been washed off due to treatment with alcohol and insecticide in herbaria. However the thinner lamina, broad, concolorous, pale to mid-brown, thin scales up the stipe distinguish it at once, as does the complete lack of visible white farina in fresh material at any stage, along with the less markedly longer lowest basiscopic pinnule in the lamina and other differences. The major confusion of this species has been with *C. dalhousiae* (= *C. albomarginata* Clarke), as explained in the main text, the summer fronds of which may be completely without visible farina, as in its type. Although the present species has been separated by Wu (1979) (sub *C. dalhousiae*) into a genus, *Leptolepidium* (non *Leptolepia* Mett. ex Kuhn) apart from the rest of subgenus

Aleuritopteris (Fée) Fraser-Jenkins, comb. nov. (basonym: genus *Aleuritopteris* Fée, Mém. Fam. Foug. 5, Gen. Fil.: 153 (1852)), there is not even a single characteristic, or combination of them, given for the genus which does not also occur in other species correctly accepted as belonging to *Aleuritopteris*. Furthermore this species appears to be relatively closely related to other members of *Aleuritopteris* and not as closely related to *C. subvillosa* Hooker which was placed with it but should, if anything, have been the type of a section. However, given the uncharted mess that cheilanthoid ferns are in at the present time, it seems less than wise to raise any new subgenera or sections, let alone genera, on a local and incomplete basis. *C. belangeri* (Bory). C. Chr., *Ind. Fil.*: 172 (1905) and *C. thwaitesii* Mett. ex Kuhn, *Linnaea* 36: 82 (1869) (= *C. keralensis* N. C. Nair & S. R. Ghosh, *J. Indian Bot. Soc.* 55(1): 52 (1976)), for example, seem to connect *C. subvillosa* with *C. chusana* Hook., *Sp. Fil.* 2: 95 *et t.* (1852) and thus with *C. swartzii* Webb & Berth., *Hist. Nat. Iles Canar.* 3(2, 3): 453 (1847) (= *C. mysurensis* Wall. ex Hook., *Sp. Fil.* 2: 94 *et t.* (1852)); so until a comprehensive world-wide study of the cheilanthoid genera can be made I do not feel happy about several of the higher ranks which have been poorly defined both in the New World and in Asia and many of which, as currently defined, appear quite inappropriate.

C. leptolepis is a rather high-altitude species, distributed from Pakistan (Baltistan and Hazara) east along the Himalaya to Kashmir, India, Nepal, Sikkim and probably further; it is also present in S. E. Tibet and S. W. China (Yunnan and Szechuan).

4. The combination, *Dennstaedtia wilfordii*, was not made by Christ (1910) as I had put, as he did not cite any authorities, but was validated as (T. Moore) Christ ex Ching, *Flor. Tsinglingensis*: 45 (1974).

5. I was mistaken, or at least partly so, in thinking the *Cosentinia vellea* (Aiton) Todaro in the area belonged to subsp. *vellea* since a recent examination of the spores of material of mine from W. PAK (PE) (5 miles S. of Malakand. C. R. Fraser-Jenkins sheets 306–309, 6/10/1990. Herb. CRFJ) by Dr. R. L. L. Viane, of Gent, has shown it to be subsp. *bivalens* (Reichst.) Riv.-Mart. & Salvo, *Anal. Jard. Bot. Madrid* 41(1): 196 (1984). This will be reported fully by Viane & Reichstein in *Flora Iranica* (in preparation). The possible presence of subsp. *vellea* has not yet been established in Afghanistan, Pakistan or India, though it may also be present.

6. Dr. Sarnam Singh (Ph.D. thesis, Calcutta Univ., 1992, *ined.*) has astutely pointed out the fact overlooked by all other authors that *Onychium contiguum* Hope is a superfluous name for *O. japonicum* (Thunb.) O. Ktze. because Hope not only correctly cited *O. japonicum* var. *multisectum* Henderson ex Clarke, *Trans. Linn. Soc. (Lond.)*, 2 Bot., 1: 459 (1880) in synonymy, but also and erroneously, *Leptostegia lucida* Ham. ex D. Don, *Prodr. Flor. Nepal.*: 14 (1825), descr. gen.-spec. The latter was nomenclaturally available for adoption at the specific rank under the rules even though it actually belongs (holotype in BM!) to *O. japonicum*, a different species from what Hope intended to refer to *O. japonicum*, a different species from what Hope intended to refer to *O. contiguum* has therefore to be replaced and I have found that the next available name for the species is *O. cryptogrammoides* Christ, *Not. Syst.* 1: 52–53 (1909), whose holotype I have seen (P!).

7. What I had reported tentatively as *Hypodematium crenatum* (Forssk.) Kuhn subsp.? *hirsutum* (D. Don) comb. *ined.*, referring to the tetraploid taxon first outlined by the late professor D. S. Loyal (Mehra & Loyal, 1965 and partially unpublished) from the west

Himalaya, now requires a new name. Having examined the various relevant types for specific names close to *H. crenatum* it now turns out that probably all of those described from the Himalaya appear to apply to the diploid taxon as, most likely, does the type of *H. crenatum* itself, even though these names have been confused and often misapplied, referring to a mixture of genuinely and imaginedly distinct taxa, by Ching and his co-workers. The differences between the Himalayan diploid and tetraploid are only very slight and they clearly form a closely-knit complex, probably with a part-ancestral relationship between the two taxa. They are therefore treated as subspecies, the new-one being as follows:

Hypodematium crenatum (Forssk.) Kuhn subsp. **loyalii** Fraser-Jenkins & Khullar, **subsp. nov.** *Similis subspeciei crenato sed lamina magis dissecta segmenta ultima minora et sori ad medium inter costulas et margines dispositi. Cytotypus tetraploideus.*

Holotype: India, Uttar Pradesh, Kempty Fall, Mussoorie. *Gunwant Tiwana*, Sept. 1964. 4 ×, grooved lamina on (upper surface) vein. Herb. CRFJ ex herb. D. S. Loyal, 11/2/1990 (BM). *Paratype:* On way to Kempty Fall, Mussoorie. *Gunwant Tiwana* no. 5, 19/8/1965. n=82 (PAN).

Differs from subsp. *crenatum* in having larger, more finely dissected and often slightly stiffer, less soft fronds with smaller segments and the sori borne half-way between the costules and margins of the segments. This is due to the segments' being less wide and extended than in subsp. *crenatum*, which usually has a wider sterile area of lamina extending beyond the sori. Subsp. *loyalii* is also less hirsute than subsp. *crenatum* with slightly sparser and shorter laminar and costular hairs, though they still remain very obvious on the costules. The spores are larger. Cytotype: tetraploid sexual (Loyal in Mehra & Loyal (1965)). *Distribution:* Along the Himalaya from Pakistan to Nepal and presumably further east; S. W. China; Thailand. Loyal (Loyal et al. 1977) also discovered a sterile triploid hybrid between the two subspecies, with abortive spores. I have not given this a nothosubspecific name here because it has not yet been collected in the area, though it should be expected to occur.

8. In his taxonomic revision of the genus, Sarvela (1978) identified the common Himalayan *Gymnocarpium* species as being the same as *G. jessoense* (Koidz.) Koidz. from the Far East. However I have never seen any material from there, including the type (KYO!), corresponding to the Himalayan species, which is readily distinguishable by its larger, longer, more obviously rectangular, sometimes slightly narrow-based, more prominently toothed segments and matt texture. True *G. jessoense* always has smaller, closer segments with more entire edges and, though often more or less rectangular, more rounded corners. The Himalayan plants seem to be all one taxon though some east Himalayan material from Sikkim may perhaps require closer study. Like several other critical species in other genera it has long been confused with distinct European species, namely *G. dryopteris* (L.) Newm., *Phytologist* 4: 371 (1851), or *G. robertianum* (Hoffm.) Newm., *Phytologist* 4: 371 (1851). Following Alston's determinations, Stewart (1972) has referred the Himalayan plant to *G. remote-pinnatum* (Hay.) Ching, *Bull. Chin. Bot. Soc.* 1(2): XIV (1935), sub *Thelypteris* "*remotipinnata*" (Hay.) Alston, *Symb. Afghan.* 4: 10 (1958). However I have seen the type of that species (TI!), which Sarvela took to be endemic to Taiwan, and in my opinion it is very close, or more probably identical to *G. jessoense*, which name it would have to replace if so, though I have not gone into this question fully. It is clearly not the same as the Himalayan plant. The next name for the latter is *G. fedtschenkoanum* Pojark., *Soobsch. Tadzhik. Fil. Akad. Nauk S.S.S.R.* 22: 9 (1950), of which Sarvela has kindly sent me photocopies of the types, and is adopted here.

It is not a Soviet-central-Asian endemic as thought by Mr. Sarvela mainly on the basis of its more scaly rhizome-apices, more glandular frond and paler spores, but is actually another Soviet-central-Asian "pseudo-endemic" (mostly resulting from the isolation of botanists in these former eastern-block countries). Scaliness, glandularity and (K. M. Pryor, pers. comm. c. 1985) paler spores all vary somewhat in the Himalayan plant and from what I have observed in the field I suspect they may be no more than responses to drier conditions, including the development of thinner, less dense exospores. This species is distributed from the Pamirs and eastern Afghanistan along the Himalaya to S. Tibet, where it begins to be replaced by the overlapping *G. remote-pinnatum*; it is also present in S. W. and central. China.

9. In strict compliance with the present Code (1988) it has unfortunately become necessary to use a name I had previously taken in a different sense, *Diplazium fieldingianum* (Kunze) Panigrahi, *Phytologia* 31 (3): 251–254 (1975), for the species I and others had referred to as *D. frondosum* (Wall. ex Clarke) Christ. But at the time of Dr. Panigrahi's publication his lectotypification, which is seriously in conflict with the protologue, would have had to have been superseded [under the Seattle Code (1972)] and it is only since the Berlin Code (1988) that the rules have been changed and oblige us to follow this typification, at least temporarily.

Kunze's (1851) description mentioned remote pinnae with long petioles and deflexed lowest pinnules, a flexuose and asperulous rachis which is minutely paleaceous below, the sori becoming confluent *etc.* and does not fit Panigrahi's lectotype (G!) in these respects, but must obviously apply to the single specimen Kunze cited formally: *Plantae Nilagiricae. Dr. B. Schmid 7*. Kunze also stated in his formal description that he could not characterise the frond-shape of his species because the specimen had no base or apex, but Panigrahi selected as lectotype a collection of Fielding's from the Himalaya which is a nearly complete frond. This specimen was merely mentioned incidentally by Kunze in the discussion below the formal citation, as a plant which he thought to be the first collection of the species, but though it gives us some insight as to what Kunze's name meant, it was obviously not the specimen he used to make his description. Indeed Panigrahi actually stated what I also suspect, that *Schmid 7* most probably belonged to a different species from Fielding's specimen. Though not citing it, he then followed Sledge's (1962) well-known exposition of the true identity of *D. polypodioides* Bl., *Enum. Plant. Javae*: 194 (1828), as being different from the present species, in contrast to Indian botanists' ideas, and thus stated that *Schmid 7* probably belonged to the true *D. polypodioides* while, by his lectotypification, the name, *D. fieldingianum*, should apply to the present species, which had previously been widely but erroneously called *D. polypodioides*. Although he again did not say so, it is most likely that Panigrahi was strongly influenced by a comment of Alston's written on Fielding's specimen, "*Allantodia fieldingiana* Kunze? The only specimen with the right collecting data," but he had not realised that when Alston pointed out its existence but with a question mark he surely had a reason to question it and probably deliberately declined to publish it or take up the new name himself because it was obviously not Kunze's type.

As Panigrahi admitted, he was selecting a specimen which was presumably a different species from Kunze's holotype - *i.e.* contrary to Kunze's protologue - so in normal circumstances the lectotypification should properly be rejected. But since the present code (1988, Art. 8.1(b)) states that a lectotypification must stand unless there exists another element (*i.e.* original element) available for lectotypification, even when the lectotypification is as conflicting as this one, I have to presume that we must now use

Diplazium fieldingianum (in Panigrahi's sense) until the original holotype, Schmid 7, is located. I have not seen Schmid's specimen at TUB, where there are many of Kunze's Schmid and Wieghe types, often annotated by him, or, apparently, at B, K, BM, P or G, where one should look. But I have never deliberately searched for it so could easily have overlooked it; there is no good reason as yet to think that it was destroyed in LZ near the end of the second world-war. Panigrahi has been unable to tell me where he searched for it before taking action to make his new combination. As soon as it turns up it would be necessary to overturn this lectotypification and the correct name for the present species, the common west and central Himalayan "*D. polyodioides*" *sensu auct. Ind., non Bl.*, will be *D. frondosum* as listed in my main text. This would be an important contribution towards the clearing up of problems raised by several rather confused local Indian papers dealing with a fragmentary part of the range of *Diplazium* species and taking little account of other areas, or, it seems, of the types themselves. It would thus help to remove us from the murky realms of "accidental taxonomy," so prevalent in India and, for different reasons, in China. I have now seen material of this species from Afghanistan (Dar-e-Nur, N. of Jalalabad, 1700 m. *H. Freitag* 5917, 4/7/1969. KASSEL!).

10. The species of *Diplazium* I had referred to as being intermediate between *D. spectabile* (Wall. ex Mett.) Ching and *D. frondosum* (in that it is generally similar to the latter but has the short, central sori of the former) is actually *D. torrentium* (Clarke) Tard.-Blot, *Asplen. Tonkin*: 69 (1932). Though the species is distinct enough I had found it difficult to name because Clarke's (1880) original idea of *Asplenium torrentium* Clarke, *Trans. Linn. Soc. (Lond.)*, 2 Bot., 1: 500 (1880), was so mixed between *D. frondosum* and the present species that it was difficult to draw any conclusion as to which species best represented the central part of his concept. Most of his description applies equally to both species, except in two conflicting characteristics: he mentioned that the sori were long, which applies only to *D. frondosum*, but he also stressed as its most marked feature the very fugacious indusia, which indicates the present species. His habitat description also suggests the present species. But he then raised the possibility that it might be a form of *Asplenium latifolium* D. Don, *Prodr. Flor. Nepal*: 8 (1825), within which he included *D. frondosum*, though he clearly made the choice to accept the species at that time and did not invalidate it by suggesting the mere possibility. Turning to his specimens, there are two annotated by him at Kew as being his *A. torrentium*, both from "Sikkim" (in the old sense which included Darjeeling, though both areas have now been taken by India) and both illustrated in his book. But as might be expected from his mixed description, Clarke 25513 1/3 A from Kulheit (his *t.* 64, *f.* 2), with long sori but persistent indusia, is *D. frondosum*, while Clarke 13701 from Ryang, Darjeeling (his *t.* 64, *f.* 3), with short sori but fugacious indusia, is the present species. Subsequently he also identified an *H. F. Blanford* specimen from Simla as *D. torrentium* (in herb. K), which is again *D. frondosum*.

At first I was inclined to follow his statement concerning long sori, but I subsequently realised that this would contradict the fugacious indusia and indeed that whichever specimen I selected, an important part of his description would have to be negated. Thus there is no clear, onesided indication towards either species to be gleaned from the protologue, which would tend to leave a more or less free choice for lectotypification, unlike, for example, with *Cheilanthes dalhousiae*. Fortunately there exists some strong evidence in a later publication as to which taxon he had originally intended. In Clarke & Baker (1888) he stated that he had made a great error, pointed out to him by Beddome, and his *t.* 64, *f.* 2 was *Asplenium latifolium* (i.e. actually *D. frondosum*), which he therefore excluded from his species. In the absence of any other unequivocal evidence I think we

must accept that this emendment genuinely reflected Clarke's original central concept of *D. torrentium* and I therefore select as lectotype: "Ryang, Darjeeling, *C. B. Clarke* 13701" (K!), which belongs to the present species. Fortuitously this also happens to be the sense in which some recent Indian botanists have taken the name.

11. Careful comparison of a range of forms among my own and others' collections of *Dryopteris caroli-hopei* Fraser-Jenkins with *D. angustifrons* (Moore ex Hooker) O. Ktzer. at Kew and with material of it from China (reported by me, Fraser-Jenkins 1989) shows quite clearly that they are distinct species. Only the former occurs in the west Himalaya.

12. The *Cyrtomium* I had reported tentatively as *C. muticum* (Christ) Ching, based on some material at BM so named by Professor Ching, is not that species, which is really a synonym of *C. macrophyllum* (Makino) Tagawa and has no marginal teeth. However, it requires further research to find out what it should be called because of the multitude of names that have been given to this species, in common with others, by Ching in Shing (1965) and in some other Chinese listings. As a result of this approach the genus is now extremely difficult or almost impossible to unravel and understand, though Mitsuta (1986) has begun some commendable study which now needs to be expanded so that the Chinese names can be identified and mostly reduced to synonymy. I am not happy with having to use one of the effectively randomly applied Chinese names in this particularly badly affected genus because Ching's policy, as stated openly to me and to his students, was to name any specimen that was not readily identified as a "new species" without the development of any kind of species concept. Such random names, which I consider virtually invalid, are not genuine specific names even though they were published in a similar way to species and were called species by their authors. Effectively they are outside the system of the Botanical Code, particularly with respect to Art. 2 and Art. 35, since what are called species by Ching and his followers were never really placed in any genuine rank at all. They are against the preamble of the Code, having thrown science into confusion and being merely the useless creation of names on a scale far beyond even that of Komarov, who, through Ching, is still followed by the Chinese pteridologists today. The many more years of research and the experience required to identify these effectively unidentified specimens and place them in genuine species than are needed to set them up in the first place means that an increasing amount of effort has to be wasted attempting to correct them. Indeed, due to their not deliberately representing any particular taxon and often being immature and inadequate material, many will remain ambiguous for generations to come. However, while working in Peking (PE) on those types that could be found I was fortunate to be able to make some preliminary study of them and I place some of these names here under the name whose type, by chance, best represents the present species, thus uniting the taxa under one name.

Unfortunately with this particular species there is an added complication: whereas the other species in the area turn out, from the work of Mitsuta (1986), to be either sexual or (probably) apomictic, the present species is clearly a highly cryptic complex consisting of a very similar sexual and apomictic taxon. The only difference I can detect as yet is that the apomictic specimens seem to have a slightly thicker and slightly less glossy lamina. Both retain the characteristic large, very rounded, somewhat glossy pinnae with obtuse 'shoulders' instead of an acroscopic basal auricle, similar to *C. macrophyllum*, but, unlike it, have more or less small teeth, at least along the more distal margins. They also both have many sori covering the lower surface in a regular pattern of rectangles as in *C. macrophyllum* and it is not always easy to see where *C. macrophyllum* ends and the

present species begins. Thus more research is required into the biology of this particularly difficult small aggregate, let alone the necessity to deal with its unfortunate overlying Chinese nomenclatural problem.

In my opinion the best available name for it at present is *C. nervosum* Ching & Shing, in Shing, *Act. Phytotax. Sinica Add.* 1: 46 (1965); synonyms: *C. hunanense* Ching & Shing, l.c.: 31, *C. shunningense* Ching & Shing, l.c.: 46, *C. kansuense* Ching & Shing, l.c.: 35, *C. kungshanense* Ching & Shing, l.c.: 36, *C. neocaryotideum* Ching & Shing, l.c.: 40, *C. yuanum* Ching & Shing, l.c.: 36, and ? *C. megaphyllum* Ching & Shing, l.c.: 40. The last could also be a slightly toothed *C. macrophyllum*; the last three were found to be sexual species by Mitsuta (1986) and the first three (including *C. nervosum*) were found to be probable apomicts.

13. Professor T. Reichstein, of Basel, thought several years ago that *Asplenium septentrionale* (L.) Hoffm. subsp. *caucasicum* Fras.-Jenk. & Lovis, in Parris & Fraser-Jenkins, *Notes Roy. Bot. Gard. Edinburgh* 38(2): 273–281 (1980), the diploid subspecies of *A. septentrionale*, could probably be occurring in the Himalaya along with the commoner subsp. *septentrionale*, as suggested by some specimens with smaller spores. But I did not list this taxon, pending further confirmation, as though plants I had noted in herbaria as being potential subsp. *caucasicum* show the diagnostic thinner and narrower lamina of the subspecies they do not really match the more obvious Caucasian and Turkish plant with its more divergent and better developed lateral pinnae and long teeth. But in 1992 Dr. Viane (Viane & Reichstein in *Flora Iranica*, in preparation) detected the hybrid between the two subspecies from its abortive spores among my new collections sent back to Prof. Reichstein from Pakistan from W. PAK (DR) (1 ½ miles S. of Lowari Village, Dir. C. R. Fraser-Jenkins sheet no., 32, plant K, 9/10/1990 (= TR 7412)). Thus specimens I had listed in my herbarium notes and have collected myself as potential subsp. *caucasicum* can now be accepted as most probably belonging to it. Distribution: AFG; W. Pak (DR, CH, SWT); E. PAK (GT, NB, NHZ, AK); IND (NK, EK).

14. Viane & Reichstein (in *Flora Iranica*, in preparation), have recently discovered *Asplenium ruta-muraria* L. subsp. *dolomiticum* Lovis & Reichstein, *Brit. Fern Gaz.* 9(5): 143 (1964), the diploid subspecies of *A. ruta-muraria* in Afghanistan. Other specimens I have seen and had cited along with probable tetraploids under subsp. *ruta-muraria* appear to belong here and doubtless there are more. Distribution: AFG; W. PAK (SWT); E. PAK (SHZ).

15. *Asplenium trichomanes* L. subsp. *inexpectatum* ["*inexpectans*"] Lovis, *Brit. Fern Gaz.* 9: 155–156 (1964), has been discovered anew from the Himalaya by Viane & Reichstein (in *Flora Iranica*, in preparation) in my new collections sent from Pakistan from E. PAK (AK) (Bursala, S. of Muzaffarabad, Azad Kashmir. C. R. Fraser-Jenkins sheet no. 209, 2/11/1990 (= TR 7589)). As has been pointed out by others (Lovis, pers. comm. 1970, Janchen, 1966, Kramer, pers. comm. 1992), the subspecific epithet was published with an incorrect termination meaning "not expecting", while it was intended to mean "not expected", so has been changed here accordingly, as has the stem of the name, which has an "s" in latin.

16. What I had called *Asplenium trichomanes* subsp. *orientale* Lovis, ined., on the basis of earlier Himalayan collections of mine identified by Lovis, must be only a part of the more or less continuous variation of subsp. *quadrivalens* D. E. Meyer in Asia. Having

found that there are nearly always fertile intermediate forms and even a tendency for both forms to occur on the same rhizome when the luxuriant forma “*orientale*” occurs, it is no longer meaningful to attempt to draw a line between it and normal subsp. *quadrivalens*. I therefore now include it within subsp. *quadrivalens*.

17. The species I had excluded as *Asplenium yunnanense* Franch. (following Reichstein’s earlier nomenclature, pers. comm.) has now been found by him (Viane & Reichstein in *Flora Iranica*, in preparation) to be conspecific with *A. glenniei* Bak., in Hook. & Bak., *Syn. Fil.*, ed 2: 488 (1874), from Mexico, where I have also collected it. It is corrected here in order to prevent the synonym, *A. yunnanense*, from getting into the literature of the subcontinent in error and for the sake of continuity. This is another interesting example of a Mexican/Himalayan connection.

18. The taxon I had referred to under 161, *Asplenium* sp., near *A. kukkonenii* Viane, Rasbach & Reichstein, from SWT in Pakistan (top of the Shangla Pass, Swat. C. R. *Fraser-Jenkins* sheets no. 55–59, 16/9/1990 (=TR 7435–7439)), which I had thought quite distinct in the field, is under further study by Viane & Reichstein (see *Flora Iranica*, in prep.), who have tentatively suggested it may only be a wider-fronded population of *A. tenuicaule* Hayata due to its somewhat small spores, of *A. kukkonenii*.

19. The species I had referred to under 166, related to *A. aitchisonii* Fraser-Jenkins & Reichstein, was first discovered in old herbarium material by Viane & Reichstein (in *Flora Iranica*, in prep.) and is a new species about to be described by them from Gol near Skardu in NB. While in Karachi herbarium, I detected a second collection of it, previously unidentified, from Ayeenabad, east of Altabad near the main bend in the Hunza river on the new Karakoram Highway to Gulmit, collected by S. I. Ali, W. Sugong, Tahir Ali & G. Ke 3462, 7/10/1989. My less reliable identification was confirmed by Viane & Reichstein and I was able to visit the place for Prof. Reichstein shortly afterwards. After a very long climb up above, returning in failure, I was about to depart when I found it in an irrigated meadow under a rock not 100 yards from the Karakoram Highway. However, Dr. Viane has pointed out the fact that my material does not exactly match their new species (including Ali *et al.* 3462), but has narrower, less-dissected pinnae. It is therefore under further study by Viane & Reichstein to see if it is not some other member of the group of *A. daghestanicum* Christ, *Vestn. Tiflis. Bot. Sada (Mon. Jard. Bot. Tiflis)* 6: 24 (1906), (to which *A. aitchisonii* also belongs), hitherto undetected in the area.

20. A recent paper (Viane, Rasbach, Reichstein & Schneller, *Candollea*, in press) contains a new subspecies of *Asplenium adiantum-nigrum* L. from western Asia, the Himalaya, China, E. Africa and La Réunion, known as *A. adiantum-nigrum* subsp. *yuanum* (Ching) Viane, Rasbach, Reichstein & Schneller, op. cit. In addition to the specimens they cite I have seen material from: AFG; W. PAK (CH, DR, SWT); E. PAK (NHZ, SHZ, AK, WP); IND (PCH, WK, NK, EK, SUD). It is a slightly narrower-fronded plant with more obtuse segments than in *A. adiantum-nigrum* subsp. *adiantum-nigrum* and can be recognised quite easily when it reaches more or less extreme forms. At one time Reichstein had identified it in herbaria as *A. pseudolanceolatum* Fomin, *Vestn. Tiflis. Bot. Sada (Mon. Jard. Bot. Tiflis)* 12: 8–10 *et t.* (1908), before he and I (in Demiriz 1981), found that that name is a synonym of what is now called *A. cuneifolium* Viv. subsp. *woronowii* (Christ) Viane, Rasbach, Reichstein & Schneller, *loc. cit.*, and therefore referred to it instead as an unnamed tetraploid taxon intermediate between subsp. *woronowii* and subsp. *adi-*

antium-nigrum, which we intended to publish later. Both Reichstein and I have long been aware, however, that fertile intermediates occur between subsp. *yuanum* and subsp. *adiantum-nigrum* and these are now interpreted by Viane et al., in press, as being the result of introgression between the two; they also claim to have established their separate identity, which has not in fact been proven at all, it being, as they admit, impossible to do so by means of cytological investigation of hybrids. It is only following the original assumption they make about the presence of postulated genomes from diploid ancestral species that they then appear to have demonstrated the separate origins of the two by a process of somewhat cyclical logic. In fact, the name subsp. *yuanum* itself was originally a mere Chingian spurious redescription of *A. adiantum-nigrum*, a species quite uncommon in China and at the time unknown to him, hitting on this form entirely by chance. Its subsequent reduction to a variety can normally be read as Ching's nearest approach towards sinking one of his redundant names. Be this as it may, it remains possible that the interpretation of Viane et al. could be correct, but in view of the publication of their paper it must now be said that there also exists a quite different and still perfectly tenable explanation. This alternative may perhaps fit the morphology of the derived allotetraploids even better, or is at least equally possible. Instead of introgression to explain the intermediates, subsp. *yuanum* may merely represent one extreme of clinal variation within *A. adiantum-nigrum*, widely connected to it by less extreme forms and not worthy of subspecific rank. Since seeing it in the Caucasus and Turkey it has long seemed to me that subsp. *woronowii* would be a far better fit, morphologically, for the ancestral diploid that really gave rise to *A. adiantum-nigrum* subsp. *adiantum-nigrum* itself (but also, of course, to subsp. *yuanum*), rather than *A. cuneifolium* subsp. *cuneifolium* Viv., *Fragment. Flor. Ital.* 16: t 18 (1806), which has been suggested as its ancestor so far, following Shivas (1969). Thus hybrids between subsp. *adiantum-nigrum* and subsp. *woronowii* would obviously show the same bivalent formation at meiosis as in those between subsp. *yuanum* and subsp. *woronowii* because they are the same thing and it cannot necessarily be interpreted as providing evidence about a postulated subsp. *cuneifolium* and subsp. *woronowii* genome. In other words, subsp. *woronowii*, like *Dryopteris caucasica* (A. Br.) Fraser-Jenkins & Corley, *Brit. Fern Gaz.* 10(5): 221–231 [“1972”] (1973), is another Caucasian “missing” diploid for a widespread and well known species. The imitation of subsp. *woronowii* by subsp. *yuanum* can be compared with the similar if less marked situation in *D. filix-mas* (L.) Schott and indeed imitation of ancestral diploids can be rather frequent where taxa occur sympatrically. In the Caucasus and Turkey *D. filix-mas* grows sympatrically with its ancestral diploids and shows a slightly wider range of variation towards both, but particularly towards the range of *D. caucasica*, than it does further west where *D. caucasica* is absent; while in the west of Britain it is commoner to find it imitating *D. oreades* Fomin, than in central Europe where *D. oreades* is absent. Thus some forms of subsp. *adiantum-nigrum* could merely be imitating subsp. *woronowii* to a greater or lesser degree in the Pontic-Caucasian-Hyrcanian region and further east, but less so in the west. The discovery by Viane et al., *loc. cit.*, of subsp. *woronowii* in the Sayanskiy Khrebet in the Russian Altai, sub *A. sajanense* Gudosch. & Krasnob., *Animad. Syst. Herb. Univ. Tomsk.* 84: 1–3 (1967), is also relevant in respect of more-or-less sympatricity. Although subsp. *cuneifolium* is evidently genomically homologous with subsp. *woronowii*, as can be seen from bivalent formation in the hybrids studied by Viane et al., *loc. cit.* (including subsp. *yuanum* × subsp. *woronowii*, first identified and discovered by me in Turkey), when taken in conjunction with those studied by Shivas, (1969), it remains possible that it may never have played any actual part itself in the origin of subsp. *adiantum-nigrum*. But instead it could be a most suitable ancestor for the

clearly distinct "serpentine form" of *A. adiantum-nigrum* (see Sleep. 1983 *etc.*), which is best known as *A. adiantum-nigrum* subsp. *corunnense* (Christ) Riv.-Mart., *Lazaroa* 8: 423–425 ["1985"] (1986). This taxon is far more consistently distinct than subsp. *yuanum* and though reminiscent of subsp. *cuneifolium* can always be readily separated from it even on gross morphology, as pointed out by Sleep and as she and I could easily see together, more recently, from my material from Norway and other material from France *etc.* (confirming here the presence of subsp. *corunnense* in those countries). Although I am not prepared to come to any positive conclusion without more evidence, it seems to me that the difference between subsp. *cuneifolium* and subsp. *woronowii* is most probably the reason for the difference between subsp. *corunnense* and subsp. *adiantum-nigrum*, whereas subsp. *yuanum* may be no more than clinal variation within subsp. *adiantum-nigrum* arising from imitation of subsp. *woronowii*, the latter being the true parent of subsp. *adiantum-nigrum*. The least that can be said at this stage is that this is equally likely to the rather definite, as if only choice made by Viane *et al.*

However, I am glad to say that it seems likely that an answer may be at hand before too long, coming out of the exciting work of J. Vogel and Dr. Mary Gibby just beginning at the BM on isozyme studies within the group. I await their judgement, but in view of the as yet unproven and hypothetical background for the separation of subsp. *yuanum* and its wide morphological overlap with subsp. *adiantum-nigrum* it is not accepted here at present, pending their further research.

22. I can now confirm that my material of *Loxogramme* from KAT in Kashmir belongs to the distinct species, *L. porcata* Price, and not to *L. involuta* (D. Don) C. B. Presl as I had thought. The type of *L. involuta* (BM!) represents a somewhat narrower-fronded species.

22. The plants I had tentatively referred to *Lepisorus morrisonensis* (Hayata) H. Itô and *L. sesquipedalis* (J. Smith) Fras.-Jenk., which latter Dr. M. J. Zink, of Zürich (pers. comm. 1992), has pointed out that I cited incorrectly as "(Wall. ex J. Smith) Fras.-Jenk.", all appear to belong to the same species, which has characteristically dark scales with narrow, pale edges and sometimes untoothed, but usually more or-less minutely toothed apices of the scales. The correct name for this species appears to be *L. morrisonensis*, to which *L. bicolor* (Takeda) Ching, *Bull. Fan Mem. Inst. Biol.* 4(3): 66–68 (1933), probably also belongs, pending cytological investigation. The Himalayan plants and *L. bicolor* have not yet been investigated cytologically, but *L. morrisonensis* from the Far East has been reported as tetraploid. In any event, *L. bicolor* was not properly considered by Ching in relation to the earlier name, but was kept separate by him latterly, as became his almost automatic practice of preserving synonyms for their own sake.

23. I have recently detected *Marsilea strigosa* Willd., *Car. Linn. Spec. Plant.*, 14, 5: 539 (1810), a European Mediterranean species which was formerly a Soviet "pseudo-endemic", from a previously unidentified collection from eastern Afghanistan (Dasht-i-Nar, S. E. border, 3000 m. *H. Freitag* 1448, 19/7/1967. KASSEL!). It could therefore also occur in westernmost Pakistan, but would have been overlooked under *M. minuta* L. if sterile, when most *Marsilea* species are inseparable. The sporocarps are paler, crowded, markedly more closely sessile than in *M. minuta* and more hairy with more or less insignificant horns. In the present specimen the leaves are not erose even though it is a small fertile plant presumably from more or less dry ground. I am most grateful to Dr. E. Launert, of London, for confirming my tentative identification of this plant. He has

also confirmed my identification of what I had been calling *M. minuta* from the area as being correct.

24. On checking the spores of the putative hybrid, *Cheilanthes bicolor* × *C. dalhousiae*, which I reported on the basis of frond morphology (as I had not had access to a microscope while travelling), Dr. Viane and I found them to be abortive, though some also appear good and are being tested for viability by Mr. C. J. Brotherton, of Sedgley near Dudley, England, who is an expert grower of the difficult cheilanthoid ferns. This confirms the hybrid nature of this single plant I found within the population of the parents, which is therefore described here as new:

Cheilanthes* × *vermae Fras.-Jenk. & Viane, **hybr. nov.** (= *C. bicolor* × *C. dalhousiae*). *Planta hybrida, morphologia frondium intermedia inter illam parentium praesumptorum, frondes subdimorphae deltatae-pentagonales, ambitus similis illi Cheilanthidis bicoloris, sed pinnula infera basiscopica pinnae inferae non valde longior quam pinnula proxima. Stipes paleas dispersas angustas usque ad medium vel fere totam longitudinem gerens, sed rhachis fere destituta et costae omnino paleis destitutae. Sporae plerumque abortivae sed cum sporis ut videtur normalibus mixtae. Holotype:* India, Jammu, Rajaori District, shortly below Adhkumari, between Katra and Vaishnodevi temple, Vaishnodevi mountain, N. E. of Jammu, southfacing rocks besides path, c. 1300 m. *C. R. Fraser-Jenkins* sheet no. 436 (= TR 7816), 28/11/1990 (BM).

This hybrid, the first yet described in Asian *Cheilanthes* of the subgenus *Aleuritopteris*, is named in honour of Profesor S. C. Verma of Panjab University, Chandigarh, who has carried out excellent detailed work on breeding systems in the genus and was the first person to detect hybridity in *Aleuritopteris*. I have also collected another two specimens of this hybrid (1 km W. of Aiju, 13 km W. Of Joginder Nagar, Mandi to Dharmsala, Himachal Pradesh, India, 1200 m. *C. R. Fraser-Jenkins* 6682, 1/9/1977 (BM) and rocks by nullah 2 miles N. W. of and below Kempty Fall, 11 miles below & N. W. of Mussoorie, N. of Dehra Dun, Uttar Pradesh, India, *C. R. Fraser-Jenkins* s. n., 27/1/1990 (Herb. CRFJ, temporarily at BM)), but in the first the sori are not quite mature. In keeping with its almost unique morphology, which is obviously intermediate between the two species, Prof. Dr. E. Wollenweber of Darmstadt has found the first specimen to be chemically different from *C. bicolor*, which it is otherwise closest to, and a full report is intended to be made by us on the flavonoid chemistry of Asian *Aleuritopteris* in relation to their taxonomy, as revised by me, at a later stage (Fraser-Jenkins & Wollenweber, in prep.). Distribution: IND (PCH).

25. In 1977 I unknowingly collected *Cheilanthes* × *kurdica* Rasbach & Reichst., *Webbia* 37(1): 58 (1983) (= *C. acrostica* (Balbis) Tod. × *C. persica* (Bory) Mett. ex Kuhn), thinking it was *C. persica*. I collected it en route on a solo drive from Oxford to Islamabad: Afghanistan, Kabul Province, c. 20 km. E. of Kabul in deep gorge of Kabul River, rocks beside road E. of main descent into gorge, c. 1800 m. *C. R. Fraser-Jenkins* 6182, 21/7/1977 (BM). For some reason I did not check the spores, but Prof. Reichstein detected this specimen from its abortive spores a number of years ago and will include it in Viane & Reichstein (in *Flora Iranica*, in prep.).

26. I have recently detected in my collections from Kashmir a new hybrid *Diplazium* with abortive spores, which shows marked features of both *D. frondosum* and *D. spectabile*, among which it was growing. It is not similar to *D. torrentinum*. This is now described as: *Diplazium* × *kashmirianum* Fraser-Jenkins, **hybr. nov.** (= *D. frondosum* × *D. spec-*

tabile). *Planta hybrida, morphologia frondium intermedia inter illam parentium praesumptorum. Stipes paleas fuscas anguste lanceolatas paulo rigidas exsertas numerosas gerens. Plerumque segmenta ordinis tertii magna ut in Diplazio frondoso, plus minusve sine lobis, plus minusve rectangularia sed apices acutiores; autem par segmentorum inferorum in quaque pinnula lobatum lobis plurimis profundis anguste regularibus in dimidio inferiore segmentis similibus illis Diplazii spectabilis, apices horum segmentorum obtusi vel rotundati. Sori ubique plus minusve breves ad costulas juxtapositi nec ad margines accedentes. Sporae abortivae.* Holotype: India, Jammu, Kathua, by cliff in large, deep, forested stream gully facing north, 9 mi. N. W. of Kadd on road to Bani, ca. 43 mi. N. of Basohli on road north to Bhaderwah, 1490 m. C. R. Fraser-Jenkins s.n., 19/12/1990 (BM). Distribution: IND (KAT).

27. A rather luxuriant specimen of *Asplenium septentrionale* I collected in W. PAK (DR) and sent back to Prof. Reichstein from Pakistan was examined by Dr. Viane and found to have abortive spores. Other material from the locality is normal subsp. *septentrionale* while other plants again appear more similar to subsp. *caucasicum*. This hybrid can only be between the two subspecies and has been described as *A. septentrionale* n. subsp. *fraser-jenkinsii* Viane & Reichstein, *Botanica Helvetica* (1992). It can only be reliably detected by its abortive spores and is probably quite widespread but easily overlooked; if it often shows hybrid vigour, which appears to be so with this specimen, that may help to reveal its presence. The lamina is not as wide as in a well developed plant of subsp. *septentrionale* and is slightly more delicate, similar to well developed Pontic-Caucasian plants of subsp. *caucasicum*. The material is from: Pakistan, N. W. Frontier Province, Dir District, 1 ½ miles below and south of Lowari village on E. side of road to Dir, S. side of Lowari Pass, rocks above slope above road, c. 2700 m. C. R. Fraser-Jenkins sheet no. 32, plant K (=TR 7412, 9/10/1990 (BM)).

28. Viane & Reichstein (in *Flora Iranica*, in prep.) will describe the new nothosubspecies of *Asplenium* × *javorkae* Küm. I reported from Afghanistan. Prof. Reichstein had also discovered it previously in Crete, but not formally described it (see Reichstein et al. 1973), and I had overlooked it because it had not been listed in Reichstein's comprehensive summary of *Asplenium* hybrids (Reichstein 1981, 1982).

29. Several more specimens of *Asplenium trichomanes* n. subsp. *lusaticum* (D. E. Meyer) Lawalrée have now been detected from their abortive spores by Viane & Reichstein (in *Flora Iranica*, in prep.) among my recent collections sent to them from Pakistan. There may also be more collections from outside the area of *Flora Iranica*, but I have not yet had the opportunity to examine microscopically the collections I sent to them. Add to distribution: W. PAK (DR, SWT).

30. Viane & Reichstein have detected from its abortive spores a quite unexpected hybrid within the *Asplenium yoshinagae* Mak. group in my recent collections from IND (KAT) sent to them. The plant does not look as if it involves a species from outside the complex and it is thus possible that there are two members of the complex occurring together there. Among normal *A. yoshinagae* subsp. *indicum* (Sledge) Fraser-Jenkins I did notice a few plants that were slightly more compact in their pinna-lobing, with slightly wider, more fused lobes. Further study is clearly necessary to find out what is happening in this group in the Himalayan region. The hybrid is from: India, Jammu, Kathua District, 4 miles S. of Bani on road to Kadol, Basohli to Bhaderwah, on mossy rocks in first N.

facing stream nullah south of the main Sewa river, 1410 m. *C. R. Fraser-Jenkins* sheet no. 50 (= TR 7430), 20/12/1990 (Herb CRFJ, temporarily at BM).

31. When listing the small handful of Sino-Himalayan species that extend westwards and reach the Pamirs, Tien Shan, or further into the fringes of the European floristic region I did not include any members of the genus *Asplenium* (see Viane & Reichstein in *Flora Iranica*, in prep.). A few extra species from other genera should now be added to the list, including some Soviet-central-Asian “pseudo-endemics” which are more widespread than at first realised and are actually Sino-Himalayan species:

Selaginella aitchisonii Hieron. – Tien Shan and elsewhere in Sinkiang. *Ophioglossum reticulatum* (inc. *O. bucharicum* and ? *O. mironovii* Sumn., *Animad. Syst. Herb. Univ. Tomsk.* 17(1): 1 (1945)) – Uzbekistan. *Gymnocarpium fedtschenkoanum* – Tadzhikistan. *Asplenium nesii* Christ – Tien Shan (in PE!). *Asplenium tienshanense* Ching, in Ching & S. H. Wu, *Act. Phytotax. Sinica* 23(1): 4–5 (1985). A Chinese species near to *A. nesii*, not present in the *Flora of Pakistan* area – Tien Shan.

Asplenium fontanum (L.) Bernh. subsp. *pseudofontanum* (Koss.) Schneller & Reichstein – Turkestan, Uzbekistan and Tadzhikistan.

Asplenium altaense (Kom.) Grubov, *Not. Syst. Herb. Inst. Bot. Acad. Sci. S.S.S.R.* 20: 33 (1960). A Chinese species in the *A. laciniatum* D. Don group, not present in the *Flora of Pakistan* area (see Viane & Reichstein in *Flora Iranica*, in prep.) – Altai.

Asplenium tenuicaule – Altai (see Viane & Reichstein in *Flora Iranica*, in prep.).

Asplenium daghestanicum – China and, at least its aggregate, in N. Pakistan (Hunza) (see Viane & Reichstein in *Flora Iranica*, in prep.); also in the E. Caucasus (including relatively recent material obtained at my request by Dr. A. M. Askerov, of Baku, with the help of Dr A. Takhtajan, of Leningrad, for study by Reichstein) and in the Tibesti in Chad.

Asplenium xinjiangense Ching, in Ching & S. H. Wu, *Act. Phytotax. Sinica* 23(1): 8, t. 1 (caption to t. 2) (1985). A Chinese species very close to the preceding one and presumably under study by Viane & Reichstein, not present in the *Flora of Pakistan* area – Tien Shan (PE!).

Asplenium pekinense Hance ssp. *pekinense* – Tien Shan (PE!).

32. Another “pseudo-endemic” from Soviet-central-Asia is a European element from west Asia occurring in Turkestan and western Afghanistan. It belongs to *Asplenium lepidum* C. B. Presl subsp. *haussknechtii* (Godet & Reuter) Brownsey and has been recognised as *A. lepidum* subsp. *haussknechtii* var. *samarcandense* (Koss.) Brownsey, *J. Linn. Soc. (Lond.), Bot.*, 72: 261 (1976). This particular form seems to represent only a handful of rather extreme plants which appear to me to be no more than local variation within subsp. *haussknechtii* in the climatically most extreme part of its range, with intermediates to the normal subspecies as might then be expected. I do not think this taxon is worthy of formal recognition, which is in slight contrast to, but otherwise in agreement with Brownsey.

I am most grateful to Dr. R. L. L. Viane, of Gent and Professor Dr. T. Reichstein, of Basel, for permission to include details of their work on *Asplenium* (specifically acknowledged in the text) in advance of the publication of *Flora Iranica*. I am also most grateful for Prof. Reichstein's continued financial support enabling me to work abroad and thus continue my botanical career.

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