

Figure 1 shows the plant in flower (10 Oct 1993), and you can also see clear evidence of vegetative reproduction. This occurs spontaneously and often.

I have never observed more than one flower per plant at any one time. My plants flower in all seasons except winter. As is the case with many *Pinguicula* spp. the corolla persists for a few days only while the remainder of the flower, including the glabrous or almost glabrous peduncle, persists for several weeks.

The leaves exhibit no movement since trapping is passive. Glands seem fewer on the leaf surfaces compared to other pinguiculas. Also, fewer prey seem to be trapped compared to other pinguiculas in my collection. Figure 2 shows three leaf studies using my field microscope.

Early in the spring of 1994 I noticed an unexpected germination of *Byblis liniflora* in a pot with *P. rotundifolia*. I decided to leave it there to see if the two species could co-exist, and as Figure 3 indicates, they certainly seem able to do so.

***DROSERA FILIFORMIS* Raf.: ONE SPECIES OR TWO?**

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INTRODUCTION: THE PROBLEM --

We are all quite familiar with the two expressions of *Drosera filiformis* Raf. The familiar northern plant has leaves 8-25 cm long with prominent red glands and occasional red leaf pigment, is known from coastal plain sandy wet areas with its main distribution running from Long Island through New Jersey, although the plant is found as far north as Maine and Massachusetts, and as far south as a few locations I know of in Columbus and Brunswick Counties, North Carolina, and disjunct populations on the shores of some lime sinkhole lakes in Bay County, Florida. There have been reports of the plant in South Carolina, but there are no supporting herbarium sheets and it has not been seen in more recent surveys. The interesting disjunct populations in Bay County, Florida are a separate problem to be tackled in a later article.

The well-known southern expression of thread-leaf sundew has leaves about 25-35 cm long and these are entirely green. The plant seems restricted to the Gulf coastal regions. I am not aware of any northern colonies, although I suppose one day someone will come across an anthocyanin-free form of the northern plant!

The problem seems to be what to call these things. The northern thread-leaf sundews are variously called *D. filiformis*, *D. filiformis* var. *typica* and *D. filiformis* var. *filiformis*. The southern Gulf coastal green plants are either *D. filiformis* var. *tracyi* or *D. tracyi*. The confusion boils down to two main factors: 1) Whether the northern and southern thread-leaf sundews are separate species or sub specific variations of one species, and 2) Changes in the ICBN while this problem has been argued.

It is worthwhile looking at some of the history of the thread- leaf sundew(s) in North America, but I am afraid that will not provide all the answers and one or two aspects are a mystery.

Drosera filiformis was described botanically by the colorful field botanist Constantine Samuel Rafinesque (1783-1840) in 1808 (Med. Repos. NY. 5:360) and he listed the plant as a North American boreal, and it had red glands, so it is clearly the northern expression. Nearly a century later, we come to questionable aspect number one.

Diels (1906) in the Droseraceae issue of *Das Pflanzenreich* listed and described *D. filiformis* Raf., then made a formal botanical description with specimen history of var. *tracyi* under the species. The type specimen for this was one collected by J.M. Macfarlane near Ocean Springs, Mississippi, and he was listed with several additional specimens. After the epithet var. *tracyi*, Diels placed the following Latin phrase in parentheses: (Macfarlane msc. sub titulo speciei). The problem is, what does this mean? Particularly, what is "msc." an abbreviation for? The rest of the phrase is straight forward, and if we make an assumption that "msc." is the equivalent of the more modern "ms." for manuscript, then the phrase is "Macfarlane in manuscript under the title species". This would mean that Macfarlane intended to describe the plant as a species, had a manuscript in preparation or prepared but not published, and Diels scooped him by describing a varietas, but giving a polite nod to Macfarlane in the process! Another possibility was suggested in correspondence to me by Ms.P.M. Eckel, namely that Diels might have meant for "Msc." to be an abbreviation for miscellus, or "mixed", possibly alluding then to a number of mixed plants of various origin on a single herbarium sheet, both northern and southern plants.

I am inclined at this point to go with the first, the "manuscript" interpretation. Unfortunately, Msc. is not a Latin abbreviation that I or anyone I have queried can find. It is unlikely that Macfarlane had a mixed northern/southern herbarium sheet since the type used by Diels from Macfarlane's collection at Ocean Springs would most likely have only the Gulf coastal plants. Then, if Macfarlane did have a species description manuscript, what happened to it?

I think I can safely say it was not published, not even in relatively obscure venues such as *Bartonia* or the occasional papers from the University of Pennsylvania Botany Department. Maybe in 1906 he sighed, buried the manuscript, and decided to go with Diels' variety. Academic botany was probably no less political then as now, and Macfarlane had been tapped to do the Sarraceniaceae for *Das Pflanzenreich*, which appeared two years later in 1908. But Macfarlane was also a proud, stubborn man (Edgar Wherry in personal correspondence to me), and perhaps he did not bend and there is a published manuscript somewhere after all.

So, where does the combination *D. tracyi* as a species come from? We see it listed in various publications today and some flora writers seem to prefer it. The usual attribution for it is Macfarlane in L. H. Bailey's *Standard Cyclopedia of Horticulture*, 1914 edition. I referred to my copy of that work, and sure enough on p1077 is listed *D. Tracyi* Macfarlane. There follows a few lines of English horticultural description. Macfarlane did the entire treatment for sundews for this horticultural work. What is there is certainly not by any stretch an adequate "botanical publication" of a new species combination, even in those days, so one would think then that *D. tracyi* is *actually a nomen nudum-illegitimate*. That is, unless something else happened between 1906 and 1914 of which we are not aware. Considering the searching that I and others have done in CP literature in general, until someone comes forth with an acceptable botanical publication of *D. tracyi* by Macfarlane, I am inclined to regard the combination as illegitimate. To mention a few, though certainly not all, botanists who have recommended *D. tracyi*, Shinnors(1962) gave a line or two of garbled reasoning I cannot follow, and Godfrey and Wooten(1981) also prefer the combination. I do not know if either they or others have looked into or evaluated the purported "description" in Bailey(1914).

The next information is in a 1944 paper by Wynne. Many CPN readers have referred to this paper which is well done. She mentions that Macfarlane proposed" the species *D. tracyi* in 1914 in Bailey, indicating that she did not accept his paragraph on this as publication, but then she lists it in the synonymy of the southern plants. She explained that she preferred Diels'

var. *tracyi* because of the very minor differences between the northern and southern plants, and made the combination var. *typica* to cover the northern plants. However, ICBN rules indicate that if a sub specific plant is described, the plants of the type of the species automatically bear the same epithet for a sub specific name. Thus, the northern plants are more properly var. *filiformis* if the southern plants are var. *tracyi*.

In summary for this first section, the only acceptable names at present for the two expressions of *D. filiformis* are var. *filiformis* and var. *tracyi*. *D. tracyi* is a nomen nudum, and someone would have to properly publish this if they wished to designate it a species. Which brings us back to our original question: Are we dealing with one or two species?

SOME OBSERVATIONS--

Considering the morphology of the plants, Wynne(1944) noted that aside from color and leaf length, there were no differences between the northern and southern plants of sufficient discontinuity to warrant separate species designation. I concur after examining many plants over the years. Early on, I thought there might be a useful difference in the anther lobes of the stamens. If you examine these with magnification, you will see that each anther is two-lobed. These seemed to be quite separate and parallel in the red plants, but joined into an inverted V in the green plants. After examining still more plants in different seasons and locations, I concluded that while most red plants had two-lobed anthers, and most green plants had inverted joined V anther lobes, there were too many exceptions (about 20%) in each case and I now discount this.

Let us consider seed morphology in our subject plants as well as North American *Drosera* as a group. Wood (1966) agrees with the value of this and feels that seed morphology is useful in distinguishing species worldwide. With a dissecting microscope, one can appreciate seed size, shape and surface sculpturing. Wynne(1944) did the definitive seed work on the North American species and provided a nice chart of seed drawings in her paper, and these are also available in Britton and Brown(Gleason, 1952). have found these seed morphologies to be quite useful, accurate and consistent. The useful point here is that the seeds of the red plants and the green plants of *D. filiformis* are identical among a gallery of quite easily distinguished seeds. Wynne (1944) also noted this, and we have another point in favor of one species.

Another useful tool to help us would be to look at some simple hybrid studies. While hybrids among North American *Drosera* spp. are easy to produce and are often found in nature where the species are sympatric, all resultant hybrids of the North American species (and indeed, most in the world) are sterile; that is, not capable of further sexual reproduction (Wood, 1955, 1960; personal observations). So an examination of some aspects of hybrid plants between our red and green expressions may be helpful.

Pursuing this line was made easier by the fact that more than a decade ago, Joe Mazrimas produced a hybrid between these two expressions and it was published as a horticultural cultivar in CPN (Robinson, 1981). This cultivar was named *Drosera filiformis* (California g.) 'California Sunset', and I will refer to it informally as "Sunset". Readers will recall that there are differences between naming and publishing hybrids botanically as a group, and publishing a hybrid as a cultivar. The cultivar is selected as an index plant among all the initial cross progeny as the best example, then propagated vegetatively to avoid variation attendant to sexual reproduction. All the other original hybrid progeny are destroyed.

Joe was kind enough to furnish freshly collected pollen and seedpods(which he normally would destroy!) for my little study. first did a stain of pollen collected from

the red and green plants, and "Sunset". I used lactol phenol cotton blue and noted 90-95% stainability of both the red and green plant pollen, and 80- 85% stainability in "Sunset" pollen. This indicates that all three pollens were quite viable, the hybrid only slightly less so. As a control, I stained pollens from hybrids of widely accepted species, e. g. *D. filiformis x intermedia*, and found less than 5% stainability, indicating non-viable pollen. If the pollen viability indication of the accepted sterile hybrids of recognized species of droseras is so low, and the "Sunset" hybrid pollen viability so high, then this would seem to be another point in favor of one species.

But let's go one step further in simple experiments that anyone can do. Let's see what is in those mature "seedpods" of "Sunset" that Joe so kindly sent me. Opening them and examining the contents under the dissecting microscope, I noted abundant full seed identical to that pictured by Wynne (1944). For the final proof of non-sterility of the cultivar hybrid, I sowed the seed and had better than 90% germination, after which I destroyed the seedlings, of course! This would then indicate that the hybrid "Sunset" is a hybrid between less than species since hybrids of all the other accepted, clear-cut species are sterile.

IN CONCLUSION...--

These results provide support for the concept of the red northern and the green southern expressions of *D. filiformis* being sub specific rather than separate species. I personally accept the variety designations rather than a concept of separate species.

As an aside, one might ask why use variety instead of subspecies. Briefly— and you may wish to consult standard references for more detailed discussions of this— variety (or more properly *varietas*) is an older more established botanical concept. Surprisingly, the designation "subspecies" did not come into play very much at all until the turn of the century, and then it was more or less suppressed by classical botanists, and those who would be recognized by these folks! More modern systematists cautiously consider a different sub specific system supported by Western American botanists that there be a hierarchy of slots beneath species, the highest of these levels being subspecies (preferably these are geographically separate one from the other), then varieties under subspecies, and forms under varieties. Strictly under this system, you cannot have a variety unless there is 3 subspecies in which it is included. But in our *D. filiformis* varieties we have no subspecies overhead. Ideally, then, the two varieties might best serve us by being changed to subspecies. However, you cannot simply say "Okay, let's call them that then." "Since this is a matter of preference and interpretation, you would have to— you guessed it—publish the new levels in the formal botanical sense. Many would say it makes little difference and do not waste the trees making paper to do this.

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