Review of Michael F. Fay, Jonathan R Bennett, Kingsley W. Dixon and Maarten J. M. Christenhusz. 2010. Parasites, their relationships and the disintegration of Scrophulariaceae sensu lato. Curtis's Botanical Magazine 26 (4): 286-313.

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This review was the first in a series of eight papers devoted to parasitic plants with a focus on their cultivation. Its purpose was to provide an update to the changing ideas about their phylogenetic relationships. In style and organization it appears to have been modeled after two book chapters by Nickrent (2002) and updated from some information gleaned from the Parasitic Plant Connection web page and primary literature. Many of the classification concepts derive directly from the APG (Angiosperm Phylogeny Group) classifications (APG II 2003, APG III 2009). The authors make a disclaimer in the conclusions saying "In a piece of this length, it is not possible to do justice to the weird and wonderful biology of the parasitic plants in all their diversity…". It seems to this reviewer that 27 pages should be sufficient room to provide a good review of current knowledge, but unfortunately this review was compromised by a number of factors. Taken as a whole, the authors provided information for all parasitic plants and in many cases cited recent molecular studies that have advanced understanding of relationships. But the devil is in the details, thus the numerous factual errors, omissions, misconceptions and misspellings are distracting and erode confidence in the accuracy of the information. For example, in the very first paragraph, *Phoradendron* is placed in Loranthaceae – a century's old concept that was long ago abandoned in favor of the independent family Viscaceae (also supported by many molecular studies). This error is compounded further (p. 298) when we learn that APG lumped Viscaceae into Loranthaceae. That is not true, as they lumped it into a broadly defined Santalaceae.

The discussion of myco-heterotrophic plants is confusing. First heterotrophs are broken down into two categories, myco-heterotrophs and haustorial parasites. But then the authors state (p. 288) that myco-heterotrophs "are more properly referred to as parasites." This is misleading. Yes, myco-heterotrophs do obtain nutrients from vascular plants, but this is indirect as they are actually fungal parasites. For this reason, I find it important to make the distinction. Why? When one is asked "how many times did parasitism evolve in flowering plants?", the answer would be 12 times when referring to strict haustorial parasitism or at least 22 times if myco-heterotrophs are included. If one asks "do monocots form parasitic associations?" the answer is "no" for haustorial parasites and "yes" when including myco-heterotrophs. Nowhere in this review does one read about the evolution of parasitism, how many times it occurred, etc. In this context, the critically important work by Feild and Brodribb (2005) on *Parasitaxus* was missed, thus its unique trophic mode is not properly discussed. The list of orders of haustorial parasites (p. 291) is missing Malvales (for Cytinaceae). Apodanthaceae is not unplaced (see below).

For the treatment of the individual parasitic plant orders, numerous errors were detected and they will simply be listed below to allow readers to make necessary corrections.

Piperales. The first molecular study to show that Hydnoraceae were related to Aristolochiaceae was Nickrent et al. (2002). This original work should be cited, not APG III (2009). There is no point in continuing to propagate an old concept, such as a relationship with Annonaceae and Rafflesiaceae.

Saxifragales. Why is there a question mark after the family name Cynomoriaceae? The statement that they are "often placed in Santalales" requires a citation.

Malpighiales. Regarding the "30 species" within Rafflesiaceae, what is meant by "although some may be undiscovered due to their ephemeral nature"? The statement that *Rafflesia* "lives on *Tetrastigma* and other genera" is not true. *Rafflesia* has been found ***only*** on *Tetrastigma*. Has anyone ever proposed that Rafflesiaceae are related to its host, Vitaceae? The poorly constructed sentence could lead some to think that Davis and Wurdack (2004) suggested this (which they certainly did not). Flowers in *Rafflesia* can be perfect as well as imperfect. No stamens are formed but anther sacs. Information on flower sexuality in *Rhizanthes* needs to be updated with information from Bänziger et al. (2007). And finally, ex situ conservation should not be considered impossible. It has been done successfully a few times in the past (see Nais 2001) and hopefully will be perfected in the future.

Santalales. The criticisms about previous molecular phylogenetic studies of Santalales being compromised because of poor taxon sampling, i.e. lack of Santalaceae in Malécot and Nickrent (2008) and Olacaceae in Der and Nickrent (2008), are unfounded because ingroup and outgroup taxon selection was appropriate for the questions being asked in each of those papers. More to the point, if the authors wanted a global analysis of Santalales, they should have chosen Vidal-Russell and Nickrent (2008) which included representatives of all families, but this citation was missed. Thus, it relationships with and among families are well understood for this order, sufficiently so that it has been reclassified based on these data (see Nickrent et al. 2010).

Balanophoraceae. The "circular structure at ground level" has been called the volva, thus this terminology should be used. Stating that the "floral morphology is unclear" does not get to the main point, i.e. that flowers have undergone reductions in size and complexity, thus compromising comparison to more typical flowers.

Loranthaceae. The problematic first sentence was mentioned above. The family as it is properly recognized contains 73 (not 80) genera and 988 (not 1400) species. The majority of this section discusses genera such as *Arceuthobium*, *Phoradendron*, and *Viscum*, erroneously included in this family (they are in Viscaceae). That *Viscum* "lends its common name to the whole family" is totally confused – "the mistletoe family"? Mistletoes occur in five different clades. Which one is ***the*** mistletoe family? With regard to medicinal compounds derived from *Viscum album*, Mann (1915) is cited. Instead of this ancient reference, why not the more recent book "Mistletoe. The Genus *Viscum*" edited by Arndt Büssing (2000) that provides a vast amount of information on the pharmaceutical properties of *Viscum*.

Olacaceae. Even in the traditional sense, Olacaceae s. lat. had 30 (not 21) genera and 170 (not 120) species. The statement that "a number of species are parasites in heathlands" may be true of some *Olax* species but not necessarily the whole family. It curious why Malécot and Nickrent

(2008), which was cited in the Santalales section (above) was not mentioned at all here. That paper provided the molecular data supporting the reclassification of this poly- and paraphyletic family (Nickrent et al. 2010). This "family" is incredibly diverse in vegetative, floral, and fruit morphology, hence this very brief summary does not do it justice. Some mention should have been made that both autotrophic and heterotrophic members occur in Olacaceae s. lat., i.e. it is in this group that parasitism evolved.

Opiliaceae. The number of genera (11) was omitted. The molecular phylogenetic work did not show a closer relationship to Loranthaceae, Misodendraceae and Schoepfiaceae. It is sister to a group of clades from the former Santalaceae and Viscaceae.

Santalaceae. The "family" s. lat. contains 37 (not 44) genera and 531 (not 875) species. It curious why Der and Nickrent (2008), which was cited above, was not mentioned here with regard to molecular data that supported the reclassification of this poly- and paraphyletic family (Nickrent et al. 2010).

Ericales. Nickrent et al. (2004), that examined all members of "Rafflesiaceae" s. lat., showed Mitrastemonaceae to be part of this order. That citation should be used instead of APG III (2009). Also, the spelling of the generic name should be *Mitrastema* (not *Mitrastemon*) according to Meijer and Veldkamp (1993); however, a proposal to conserve the name *Mitrastemon* was recently published after this work (Reveal 2010).

Lamiales. Orobanchaceae. The review by Tank et al. (2006) as well as Schneeweiss et al. (2004) should have been listed with the series of others addressing realignment in this group. The genus *Epifagus* is misspelled "Epiphagus" several times. Despite the claim on p. 304, there currently does not yet exist a "new classification" of Orobanchaceae. The papers that are cited all had incomplete taxon sampling, so a global phylogeny that could serve as the basis for a revised classification remains to be completed. The size of the newly defined Orobanchaceae is not provided to the reader (90 genera, 1811 species).

Solanales. Convolvulaceae. *Cuscuta*. This brief treatment makes no mention of the molecular evidence that firmly placed Cuscuta in Convolvulaceae (Stefanovic and Olmstead 2004). This is important to show that Cuscutaceae should no longer be recognized or used.

Apodanthaceae. Nickrent et al. (2004) discussed ambiguity as the the placement of this family. Nuclear ribosomal DNA gave Malvales and mitochondrial matR gave Cucurbitales. Later work (see PPC web site) showed that the *Pilostyles* rDNA data were from contamination, and new sequences agreed with the matR data in the Cucurbitales placement. Thus, this family is no longer unplaced in the angiosperm phylogeny. The genus *Berlinianche* is misspelled "Berlinianthe". The common hosts to *Pilostyles* in the USA, *Dalea* and *Psorothamnus*, are not mentioned along with *Astragalus* and *Daviesia*.

Summary

The purpose of this chapter was to provide an update to the changing ideas about parasitic plant phylogenetic relationships. Although information is provided for all groups, and in many

cases recent molecular studies that have advanced understanding of relationships are cited, the goal was compromised because of numerous factual errors, omissions, misconceptions and misspellings that confuse the reader and erode confidence in the accuracy of the information. To this reviewer's knowledge, none of the authors are specialists in parasitic plants, thus this work would have benefitted greatly if it had been reviewed by such an expert. Given that the theme of this issue of Curtis's Botanical Magazine was cultivation of parasitic plants, it also would have been useful for this review to have consistently incorporated culture information for the various groups.

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