BOTANY 940
Question for Antonelli & Sanmartín 2010 and Pennington et al. 2010

What is phylogenetic nice conservatism (Pennington paper page 13786)?
Given the criticism of limited statistical value for the lineage-through-time plots, what other methods should be used to test the major hypotheses for Neotropical plant diversification?

How should we proceed when hypotheses of diversification overlap (e.g. Andean uplift, climatic fluctuations, and postulated island chains between South and North America; p 410)?

Methods

The Pennington states it uses a dated phylogeny yet has no fossils in its dataset or analysis. It uses the dating of Legumes from a 2005 paper in Systematic Biology which uses a number of fossils. The dated crown node for the lineage that leads to "Cyathostegia mathewsii" is dated at 58.6 Mya, the Legume crown node at 59.0 Mya. Is it valid to use the fossil dating from another paper ? Are there criteria for such use? How long can this "leash" be? It is about 47 million years in this case.

Considering the variation in topology between the ITS and matK trees, why use one over the other? Because the nice northern and southern clades in the ITS tree tell a cleaner story? What about a combined tree?

Natural History

I'm curious about the pollinator and disperser/dispersal agent of Cyathostegia mathewsii. One paper (Lewi 2003, The floral scent of Cyathostegia mathewsii (Leguminosae, Papilionoideae) and preliminary observations on reproductive biology) indicates that beetles might be a possible pollinator. I'm curious as to what other species might be involved, and how this coupled with the uplift of the Andes might lead to population differences and increased speciation. Do different populations of C. mathewsii have the same pollinator(s)? How are the fruits or seeds dispersed? Which has a greater effect on speciation: limitations in pollination and dispersal or geographic factors (obviously, pollination, dispersal and geography are related).

It would be interesting if the Pennington paper had touched on the pollinators and dispersers of *Cyathostegia mathewsii* (if they are known) and how they affect its isolation. It is obvious that the Andean terrain has isolated the various populations but the pollinators of *C. mathewsii* are apparently unable to fly (or walk?) from one population to another, preventing gene flow. We assume that most legumes are animal-dispersed so the disperser apparently cannot travel between populations either. These aspects of *C. mathewsii*'s natural history should be studied to gain a better understanding of why (in addition to topography) the populations have remained isolated.

Would you please go over some of the basic biology of *Cyathostegia mathewsii*. How

are seeds dispersed? And how far are they dispersed? Also, they mention isolation of populations. Have any population genetic studies been conducted on this group? What are the pollinators?

Results & Conclusions

The results seem very clean with all of the regions forming nice monophyletic groups (except the paraphyletic Apurimac). How common is SDTF throughout the Andes? Is the author's sampling representative of the total range of C. matthewsii, or is it possible that they picked locations that were just separate enough to tell this clean story?

The authors said that lineages in SDTF have a different diversification history with older species and lineages. However, the table 1 shows that in general the crown ages of the MMF lineages are older o similar to SDTF lineages. Their conclusions are based on two figures (figure 2 and figure S1) where that idea seems to be very clear. However, I do not understand the biological meaning of divide the crown age by the number of species in the lineage. Do you think more data for MMF lineages will support the authors' idea?

Don't you think the affirmation that SDTF contain mostly old species and lineages could be biased by the groups that were surveyed (just genera that matched the biomes were used)?, I think it should be complemented with phylogenies of groups present in the different biomes, then proceed to determine to which biomes the basal or derived taxa belongs to.

The authors states that the seasonal dry tropical forest are only open to groups previously adapted to these conditions (they associate conditions with the same kind of forest). There are other groups that also deal with some degree of drought though its habitat are not SDTF (i.e. some epiphytes like certain genera of bromeliads and orchids), several of them having prove to be very plastic about their growing conditions, can these groups also be an input to the composition of the flora of these places?