

Interactive comment on “Phylogenetic support for the Tropical Niche Conservatism Hypothesis despite the absence of a clear latitudinal species richness gradient in Yunnan’s woody flora” by G. Tang et al.

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The work seeks to test the tropical conservatism hypothesis (TCH) proposed to explain the latitudinal diversity gradient in plants. The present work focuses exclusively on Yunnan, China and woody plants in particular. There have been many articles recently testing the very basic prediction that tropical latitudes contain older lineages than temperate lineages with the mechanism being that cold tolerance is 'difficult' to evolve. WE HAVE TO DISAGREE HERE. THERE ARE NOT THAT MANY PAPERS THAT HAVE TESTED THIS BECAUSE INCLUSION OF PHYLOGENETIC INFORMA-

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TION IS A RATHER RECENT PHENOMENON THAT HAS MOSTLY BEEN USED AT THE COMMUNITY ECOLOGY SCALE (FIELD PLOTS), BUT NOT AT REGIONAL OR OTHER BIOGEOGRAPHICALLY RELEVANT SCALES. I KNOW OF ONLY A FEW FROM AUSTRALIA, NORTH AMERICA AND ONE FOR SOUTH AMERICA, WITH NONE FOR ASIA. ADDITIONALLY, OURS IS THE FIRST TO PROPOSE DROUGHT TOLERANCE AS AN ADDITIONAL MECHANISM TO COLD TOLERANCE TO EXPLAIN THE OBSERVED LATITUDINAL PATTERN.

Typically such papers have measured the mean family age of species in the tropics versus the temperate. A related approach, and the one taken here, is to quantify the degree of phylogenetic clustering across latitude with the expectation of clustering in temperate latitudes due to recent diversification in temperate latitudes. PHYLOGENETIC CLUSTERING AT THIS SCALE MAY HAVE NOTHING TO DO WITH THE AGE OF THE LINEAGES INVOLVED. FOR EXAMPLE, GYMNOSPERMS ARE VERY WELL ADAPTED TO COLD CONDITIONS BUT REPRESENT A VERY OLD PHYLOGENETIC LINEAGE. HOWEVER, WHEN VIEWED OVER THE WHOLE PHYLOGENY, THEY FORM A CLUSTERED SET. PHYLOGENETIC CLUSTERING REPRESENTS OVER-REPRESENTATION OF SPECIES BELONGING TO CLOSELY RELATED LINEAGES WHEN COMPARED TO SPECIES SELECTED RANDOMLY FROM THE WHOLE PHYLOGENY. IN SOME SPECIAL CASES THIS MAY INDEED REPRESENT RECENT SPECIATION EVENTS, BUT IN GENERAL IT JUST REPRESENTS CLUSTERING OF SPECIES IN A LIMITED SET OF CLOSELY RELATED LINEAGES (WHICH MAY BE OLD OR YOUNG).

I have to say, despite my interest in this hypothesis and its general importance, I find the present test (and recent tests using family age) to be fairly uninformative. That is, they are weak tests of the hypothesis. AS SAID, OUR TESTS HAVE NOTHING TO DO WITH THE AGE OF LINEAGES. THEY TEST PHYLOGENETIC INFORMATION CONTENT (PD AND PDS), WHICH MAY BE AGE RELATED BUT DOES NOT HAVE TO BE, AND PHYLOGENETIC RELATEDNESS (NRI), WHICH DOES NOT

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HAVE TO EQUATE TO LINEAGE AGE AT ALL (SEE PREVIOUS COMMENT). THEY ARE INDEED NOT ACTUAL, EXPERIMENTAL, TESTS OF THE HYPOTHESIS. WE AGREE THAT SUCH TESTS WOULD BE VERY USEFUL AND POSSIBLE (APPLY DIFFERENT COLD AND DROUGHT REGIMES TO EXPERIMENTAL COMMUNITIES AND SEE HOW THEY RESPOND). HOWEVER, THAT DOES NOT MAKE MORE CORRELATIVE STUDIES IRRELEVANT. EXPERIMENTS HAVE TO BE BASED ON INFORMED DECISIONS ON WHICH VARIABLES TO TEST. APART FROM COLD TOLERANCE, OUR STUDY SUGGESTS QUITE STRONGLY (FOR THE FIRST TIME) THAT DROUGHT TOLERANCE CAN PRODUCE THE SAME PATTERN USING THE SAME GENERAL PROCESS OF HYDRAULIC FAILURE. I THEREFORE THINK THAT THIS PAPER IS HIGHLY RELEVANT, AND NOT PUBLISHING IT BECAUSE IT IS NOT A REAL EXPERIMENTAL TEST OF THE HYPOTHESIS, WOULD BE HIGHLY UNDESIRABLE.

Of course, we know tropical families are on average older, but this does not preclude alternative hypothesis. The phylogenetic clustering of lineages using NRI is an even less optimal test. For example, you could have a recent radiation that causes an actual higher richness in the temperate zone and still get 'phylogenetic clustering' in the tropics using a metric like NRI due to the age of the radiation. Thus, NRI by itself is not enough to really disentangle the mechanisms underlying latitudinal gradients nor is the mean family age. AS SAID ABOVE, NRI GENERALLY HAS NOTHING TO DO WITH LINEAGE AGE. IT JUST SHOWS THAT THE SPECIES IN A CERTAIN REGION ARE NOT A PHYLOGENETIC RANDOM SUBSAMPLE OF THE COMPLETE PHYLOGENY BUT A SET OF CLOSELY RELATED TAXA (THESE CAN BE OLD OR YOUNG). SINCE CLOSELY RELATED TAXA TEND TO HAVE SIMILAR TRAITS AND ENVIRONMENTAL REQUIREMENTS, PHYLOGENETIC CLUSTERING INDICATES THAT THE COMMUNITY CONSISTS OF A SUBSET OF SPECIES ESPECIALLY ADAPTED TO A SPECIFIC HABITAT (I.E. ENVIRONMENTAL FILTERING), WHICH IS ONE OF THE BASIC PREMISES OF THE TROPICAL NICHE CONSERVATISM HYPOTHESIS. IN THE TROPICS PHYLOGENETIC RANDOM OR OVERDISPERSED

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COMMUNITIES (AS OPPOSED TO CLUSTERING) ARE FOUND BECAUSE ALL SPECIES CAN CO-EXIST AND IF EXCLUSION OCCURS IT IS BASED ON COMPETITION, I.E. SPECIES WITH SIMILAR TRAITS AND ENVIRONMENTAL REQUIREMENTS OUTCOMPETING EACH OTHER RESULTING IN PHYLOGENETICALLY LESS RELATED SPECIES COMPOSITIONS THAN EXPECTED. THEREFORE NRI IS ESPECIALLY WELL SUITED TO TEST THE TROPICAL NICHE CONSERVATISM HYPOTHESIS!

If we are to truly progress and test the TCH we must move beyond such simplistic (ALLOW US TO DISAGREE ;-)) analyses that do not robustly support or reject the hypothesis. The paper would be much better if some more detailed information about climatic niches and physiology was included. Otherwise, we simply have yet another paper saying tropical lineages are phylogenetically widespread and older. We know this and another paper pointing this out isn't progressing our science. AS MENTIONED ABOVE, MORE EXPERIMENTAL ANALYSES WOULD INDEED BE VERY USEFUL, BUT THIS DOES NOT MEAN WE SHOULD GIVE UP ON PUBLISHING INFORMATIVE CORRELATIVE ANALYSES, AS THESE WILL FORM THE BASIS FOR A CAUSAL EXPERIMENTAL APPROACH. ALSO, FOR MOST SPECIES INCLUDED IN THIS STUDY, THE PHYSIOLOGY HAS NEVER BEEN STUDIED SO FOR THE MOMENT, GIVEN THE AVAILABLE INFORMATION, A CORRELATIVE APPROACH IS THE MOST FEASIBLE.

I found the focus on only Yunnan to be a major methodological weak point when considering the latitudinal species richness gradient. Yunnan is extraordinary in its climatic diversity, but that does not mean it is sufficient to serve as an adequate laboratory for studying latitude and species richness. OUR CHOICE FOR YUNNAN IS ACTUALLY THE STRONGEST POINT OF THIS PAPER AS THIS ANALYSES DETECTED A POSSIBLE ALTERNATIVE, OR ADDITIONAL MECHANISM SUPPORTING THE TROPICAL NICHE CONSERVATISM HYPOTHESIS THAT WAS NOT YET FOUND BEFORE. ADDITIONALLY, THIS STUDY SHOWED THAT EVEN WHEN THERE IS NO

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CLEAR LATITUDINAL SPECIES RICHNESS GRADIENT, THERE CAN STILL BE AN UNDERLYING PHYLOGENETIC SIGNAL IN SUPPORT OF THE TROPICAL NICHE CONSERVATISM HYPOTHESIS. BOTH FINDINGS ARE NEW AND UNEXPECTED AND WOULD NOT BE KNOWN IF WE HAD NOT CHOSEN FOR YUNNAN.

Lastly, I'm not quite sure where this paper fits into the scope of this journal which is typically concerned with ecosystems processes and biogeochemistry. It seems a rather strange venue for the work and I can't help but wonder why it would be submitted here and not to a macroecological or biogeographic journal. It might be outside the scope of the journal. WE COPIED THE FOLLOWING 'AIMS AND SCOPE' FROM THE JOURNALS WEBSITE: XXX "With the above objective, Biogeosciences covers the following fields: Biodiversity and ecosystem function; Evolutionary ecology; Environmental microbiology; Biogeochemistry and global elemental cycles; Biogeochemistry and gas exchange; Biomineralization, microbial weathering and sedimentation; Interactions between microbes, organic matter sediments and rocks; Biogeophysics; Earth system sciences and response to global changes; Industrial Biogeosciences & Technology Paleogeobiology, including origin and evolution of life, evolution of the biosphere, sedimentary records, and the development and use of proxies; Astrobiology and Exobiology." XXX IT CLEARLY STATES 'BIODIVERSITY AND ECOSYSTEM FUNCTION' (WHICH WE COVER BECAUSE OUR PHYLOGENETIC ANALYSES ARE LINKED TO SPECIES TRAITS AND RESOURCE REQUIREMENTS AND HOW THIS LINKS TO DIVERSITY), ONE COULD EVEN ARGUE THAT WE ALSO COVER 'EVOLUTIONARY ECOLOGY' BECAUSE OUR ANALYSES ARE BASED ON HOW COMMUNITIES ARE PHYLOGENETICALLY STRUCTURED AND THUS MAY INFORM ON SPECIATION PROCESSES AS WELL.

In sum, I found the results to be something any botanist could predict, the results do not

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advance our understanding of latitudinal gradients, the work does not strongly support or reject any major hypothesis and the work is too geographically restricted to have great meaning. WE STRONGLY DISAGREE WITH THIS CONCLUSION AS SHOULD BE CLEAR FROM OUR COMMENTS ABOVE.

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