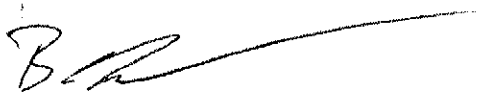
General Assessment:

Here, I review the Ph.D.-thesis written by Jan Klecka entitled "The role of species traits in predator-prey interactions and food web structure". The whole thesis is written in excellent English and the hypothesis, scientific background, methodology, analyses as well as the discussion and conclusions are well presented. The thesis topics are centred around species feeding interactions ecology and their impact on biodiversity and food web structure and implications for the stability of ecological communities. Moreover, the thesis spans from theoretical ecology and modelling over laboratory experiments to field studies. All parts of Jan Klecka's thesis are interconnected but also are able to be own projects that can be published separately. Two of the six manuscripts in the thesis are already published in international high quality journals. Moreover, three of the remaining manuscripts are already under review. The whole thesis is of excellent quality and high scientific standards. Including six first author manuscripts in one Ph.D. thesis is outstanding based on my personal knowledge and shows also the high quality.

In conclusion, the extent and content of the thesis meets the requirements of a Ph.D.-thesis and I therefore recommend to accept this thesis and award the doctoral degree.

Yours sincerely,



Dr. Björn C. Rall

Five Major Questions:

1) Paper 1; "Who eats whom in a pool? A comparative study of prey selectivity by predatory aquatic insects": You mentioned that the models you used to identify prey selectivity have the problem that they don't account for prey depletion over a 24h predation experiment. Moreover, prey might by differently consumed in one-predator one-prey trials (e.g. because of different body masses [imagine a 1mg predator might consume a 0.5 mg prey or 50 of a 0.01 mg prey to be satiated] (e.g. Brose et al. 2008)) making models that test selectivity without knowing that a-priori information additionally less trustworthy (even if they are today's standard procedures). There is only a very scarce source of studies using more mechanistically models to overcome that problem (Colton 1987; Kalinkat et al. 2011). In your case, you haven't measured the one-predator one-prey feeding strength what would have been a much too labour intensive work from my own experience. But there is a plethora of literature investigating functional responses from different predator prey combinations that where also summarized and in several meta-studies (see e.g. Hansen et al. 1997; Englund et al. 2011; Rall et al. 2012 for an overview).

   Could you please discuss if the methods and sources mentioned above would be applicable to your data and if you think that these methods might give a new interpretation to your results. Moreover, do you think that comparing mechanistic models as used in Kalinkat et al. (2011) might be a good alternative to methods like Manly's selectivity index or Pianka's overlap index, or might be a could base to benchmark those methods?

2) Paper 2; "Anti-refuge effect of aquatic vegetation for planktonic prey": In this paper you found a kind of habitat induced anti-refuge towards the planktonic prey, but only for the phytophilous predator. But to call it "anti-refuge" it might by necessary if the prey would "like" to use the introduced habitat as a refuge and the predator might not be able to follow in this refuge (e.g. Scheffer & De Boer 1995). To that reasons, I would not call your findings an "anti-refuge" effect but for example a habitat induced preference, because the alternative prey was less consumed.

   Do yo think that one-predator one-prey would lead to similar results for both prey types or do you think it would lead to different outcomes for each prey type?

3) Paper 3; "Foraging and vulnerability traits modify predator-prey body mass allometry: freshwater macroinvertebrates as a case study": You stated in the methods part that you avoided a complex habitat structure, sediment and refuge to focus on the size and the

qualitative traits microhabitat use, feeding mode, foraging mode, activity of the prey and escape ability. Results from the SMA-statistics suggest that only body mass effects as well as feeding mode (suctorial vs. chewing) have significant effects on preferred body masses of the prey and variability is not significant. In contrast, the trait based food web analyses suggests more complex results on prey mortality only excluding prey escape ability and microhabitat use. In general, movement speed is correlated to body size (Peters 1983) so even good escaping prey (maybe due to better reaction times) might be not able to escape finally without any hiding possibility. Moreover, habitat structure affect the refuge effect differently in dependence of the body masses of the predator and the prey (Vucic-Pestic et al. 2010b) and might decrease the overall feeding performance (or increase mortality) on a large scale of prey densities (Vucic-Pestic et al. 2010a). Please discuss if additional habitat structure that might have provided refuges for the prey would have changed your findings. Do you think that PPMR, prey body mass vs. predator body mass and variability would have been even more sharp as they already are or do you think (with your experience on the biology of the prey species) escape ability and microhabitat use would also be significant estimators for mortality?

4) Paper 4; "Niche model and size structure of food webs": You used three different niche model variants to model size dependencies on the occurrence of feeding links. The body mass based niche model shows the broadest variance in PPMR (Fig 2A), especially for the largest predators. In nature, predators and prey can have a high diversity of different mass ratios, e.g. large cats as leopards prey on animals that are approximately equally sized or spiders are capable of larger prey whereas some specialists as filtrators (e.g. whales, *Daphnia*, unicells) have preferred prey that is extremely smaller than themselves. Meaning that the body mass based niche model would include this differences and maybe the pure allometric model would exclude them. However, if the body mass based niche model allows always the feeding of all prey sizes for each predator would be also not realistic. The two-traits model seems to be the solution to cope this problem, however the allometric model still performs best compared to empirical data. Why is it like this, do you think that the studies of Brose et al. (2006) and Nisbet et al. (2011) do not cover other foraging traits but sheer predator-prey pairs; what do you think, based on your experience on your laboratory experiments should be the superior model to predict food web structure?

5) Paper 5; "Body mass dependent dispersal and feeding constraints drive food web assembly": In this study you investigated how food web assembly is driven by abundance and migration rates. Your model assumes that migration out of a source patch is simply driven by the

product of species density N and the dispersal rate D. Please discuss if other abiotic and biotic drivers might be responsible for the willingness of a species (or at least some individuals) to leave one patch and migrate elsewhere.

Minor Questions & Concerns:

1) The numeration of the "list of papers and author's contribution" doesn't fit to the occurrence of the papers later in the thesis. Paper 2 and 3 are exchanged.
2) Paper 2: Why do you use a GLM on $\log_{10}$ transformed data? Isn't it then a simple ANOVA with factorial variables?
3) Why do you use the log ratio, what other tests would have been possible (see review (Berlow et al. 1999)) and what are the pros and cons of the different possibilities?
4) Paper 5: I think boxplots (Fig. 2) without a statistic are informative but the statistics could be more sophisticated if you would use glmm's accounting for the autocorrelation through time and with a binomial family for ratio data. I guess it would be a nice addition to the paper before submitting it to a journal.
5) Paper 5: There are different assumptions how abundance is driven by allometry. Assuming b_n as 1 doesn't take the trophic level into account as nicely shown in a field study by Timothy Meehan (2006). Moreover, analyses of statistical methods showed that the slopes of -0.75 to -1 for b_n are an artefact and must be reduced by approximately 1 (White et al. 2008). I guess adding this to your analyses would not change the qualitative output of your studies, but it would be a t least worth it to discuss.
6) Paper 4: Fig 3c) shows the same variances for OLS and SMA. Is this a coding error in R? Or is this a true result what would be surprising.
7) Paper 4: I guess it would be nice to have figure 2 also overlaid with empirical data to get an impression how empirical distributions are. I guess it would be a nice addition to the paper before submitting it to a journal.
8) Paper 4: You presented SMA and OLS all in the main manuscript. As you suggested in this and other papers (and this is also my opinion) SMA is the better analyses. Presenting both is somehow over-technical. I guess the readability of the paper would gain if the OLS regressions go into an appendix. That would allow you to focus on the biological and not on

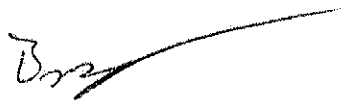
the technical results.

9) Paper 4: I couldn't find out if the data that goes into Fig. 3 is based on individual regressions of each web or on an overall regression. Can you clarify this?

10) Paper 4: In Fig. 7 it would be also nice to find a boxplot with empirical results if Naisbit et al. (2011) provide their data, the data of Brose et al. (2006) is freely available in the ecological archives (Brose et al. 2005). I guess it would be a nice addition to the paper before submitting it to a journal.

Looking forward to a stimulating discussion.

All the best,



Björn Rall

References:

Berlow, E.L., Navarrete, S.A., Briggs, C., Power, M.E. & Menge, B. (1999). Quantifying variation in the strengths of species interactions. *Ecology*, 80, 2206–2224.

Brose, U., Cushing, L., Berlow, E.L., Jonsson, T., Banasek-Richter, C., Bersier, L.-F., et al. (2005). Body sizes of consumers and their resources. *Ecology*, 86, 2545–2545.

Brose, U., Ehnes, R., Rall, B., Vucic-Pestic, O., Berlow, E. & Scheu, S. (2008). Foraging theory predicts predator-prey energy fluxes. *J. Anim. Ecol.*, 77, 1072–1078.

Brose, U., Jonsson, T., Berlow, E.L., Warren, P., Banasek-Richter, C., Bersier, L.-F., et al. (2006). Consumer-resource body-size relationships in natural food webs. *Ecology*, 87, 2411–2417.

Colton, T. (1987). Extending functional response models to include a secondnd prey type: an experimental test. *Ecology*, 68, 900–912.

Englund, G., Öhlund, G., Hein, C.L. & Diehl, S. (2011). Temperature dependence of the functional response. *Ecol. Lett.*, 14, 914–921.

Hansen, P., Bjornsen, P. & Hansen, B. (1997). Zooplankton grazing and growth: Scaling within the 2-2,000-mu m body size range. *Limnol. Oceangr.*, 42, 687–704.

Kalinkat, G., Rall, B.C., Vucic-Pestic, O. & Brose, U. (2011). The Allometry of Prey Preferences. *PLoS ONE*, 6, e25937.

Meehan, T.D. (2006). Energy use and animal abundance in litter and soil communities. *Ecology*, 87, 1650–1658.

Naisbit, R.E., Kehrli, P., Rohr, R.P. & Bersier, L.-F. (2011). Phylogenetic signal in predator-prey body-size relationships. *Ecology*, 92, 2183–2189.

Peters, R.H. (1983). *The ecological implications of body size*. Cambridge University Press.

Rall, B.C., Brose, U., Hartvig, M., Kalinkat, G., Schwarzmüller, F., Vucic-Pestic, O., et al. (2012). Universal temperature and body-mass scaling of feeding rates. *Phil. Trans. R. Soc. B*, 367, 2923–2934.

Scheffer, M. & De Boer, R.J. (1995). Implications of spatial heterogeneity for the paradox of enrichment. *Ecology*, 76, 2270–2277.

Vucic-Pestic, O., Birkhofer, K., Rall, B.C., Scheu, S. & Brose, U. (2010a). Habitat structure and prey aggregation determine the functional response in a soil predator-prey interaction.

Pedobiologia, 53, 307–312.

Vucic-Pestic, O., Rall, B.C., Kalinkat, G. & Brose, U. (2010b). Allometric functional response model: body masses constrain interaction strengths. *J. Anim. Ecol.*, 79, 249–256.

White, E.P., Enquist, B.J. & Green, J.L. (2008). On estimating the exponent of power-law frequency distributions. *Ecology*, 89, 905–912.

Posudek disertační práce Jana Klečky:

The role of species traits in predator-prey interactions and food web structure

Předkládaná práce sestává z šesti článků, napsaných na velice dobré úrovni. Dva z nich jsou již publikovány ve velice dobrých časopisech: *PLoS ONE* (IF_{2011} = 4.092) a *Ecological Indicators* (IF_{2011} = 2.695), v obou je aplikant prvním autorem, další pravděpodobně vyjde v *Journal of Animal Ecology*, což vše dohromady samo o sobě dostatečně vypovídá o kvalitě této práce. Další dva články jsou odeslány do tisku a jeden je prezentován jako manuskript.

Autor se ve své práci zabývá rolí vlastností druhů v interakcích dravec-kořist a ve struktuře potravních sítí, především na vodním hmyzu. Metodicky jde o kombinaci modelového přístupu pomocí simulačních modelů, laboratorních a terénních pokusů.

Dizertace je opatřena úvodem, který stručným a jasným způsobem uvádí čtenáře do problematiky, a závěrečnou kapitolou věnovanou závěrům práce, v níž jsou dosažené výsledky stručně sumarizovány. I zde je sympatický koncizní styl doktoranda.

Sám fakt, že dva z šesti předkládaných článků prošly tvrdou recenzí v prestižních časopisech naznačuje, že je těžké těmto článkům nyní něco dalšího vytknout. Zbylé čtyři jsou dle mého názory též velice dobré úrovně. Matematické výsledky, které jsem byl na základě dizertace schopen ověřit, jsou dle mého názoru technicky správné a bez chyb.

Spíše zde proto uvádím pár myšlenek, které s předkládanou prací souvisí a jimž by bylo v diskusi při obhajobě vhodné věnovat nějaký čas, čímž by též kandidát prokázal mimo jiné i své schopnosti vědecké disputace. Jde o tyto otázky:

1. Na str. 4 uvádí autor, že pozitivní korelace mezi složitostí potravní sítě a stabilitou jejího rovnovážného bodu je možná následkem nenáhodnosti struktury této potravní sítě. Domnívá se uchazeč, že toto je skutečně kauzálním vysvětlením, tj. že se toto tvrzení dá nějak dokázat, nebo to spíše intuitivně plyne z toho, co autor uvádí na str. 3-4?
2. Je vždy skutečně nutné, aby rovnovážné stavy v potravní síti byly skutečně stabilní? Co by se stalo, kdyby byly všechny nestabilní a systém by "přeskakoval" z jednoho nestabilního stavu do druhého? Mohlo by dojít k dlouhodobému přežívání systému?
3. Na str. 20 a 27 říkáte, že jste při review vyloučili studie zabývající se intraguild predation a kanibalismem mezi predátory. Tyto dva jevy jsou velice časté mezi terestrickými predátory, obzvláště v případě nedostatku jejich běžné kořisti - to citujete u vodních druhů na str. 24. Jak je to tedy u akvatických dravců? Jaký byl důvod vyloučení těchto jevů z vašich úvah?

K práci mám drobné stylistické poznámky:

a. "List of papers", správně je minulý čas od "run" psán "ran". Objevuje se to zde dvakrát, i když jinak je úroveň angličtiny celé práce velice dobrá.
b. "List of papers" vs. "Contents": články II a III jsou zde citovány v opačném pořadí, což působí poněkud divně.

c. Str. 3: "stability of large complex food webs may be stem from" je asi překlep, ne?

Jak jsem již zmínil výše, mé otázky jsou míněny spíše pro povbuzení diskuse, v žádném případě nesnižují vědeckou kvalitu předkládané práce. Kritizované překlepy jsou též maličkostmi.

Z autorovy publikační činnosti, vysoce nadprůměrné pro doktorského studenta, proto zcela jednoznačně vyplývá, že Jan Klečka prokázal během svého doktorandského studia schopnost samostatně vědecky pracovat a publikačně zpracovat získané empirické poznatky.

Závěr: Jan Klečka jednoznačně prokázal schopnost samostatné vědecké práce a splnil veškeré požadavky kladené na doktorandské studium. Jednoznačně doporučuji přijetí jeho disertační práce k obhajobě.

Praha, 20. února 2013

Prof. RNDr. Pavel Kindlmann, DrSc.