Review of the Ph.D. thesis of **Mohamed Ali Abdussalam M. Kotela** entitled

The Use of Insects for Monitoring Global Changes

The Ph.D. thesis of Mohamed Ali Abdussalam M. Kotela consists of three papers, from which two are unpublished manuscripts and one is accepted in a leading ecological journal.

The aim of the thesis was to analyse long-term data on population dynamics of nocturnal insects (moths) and to investigate mechanisms, which underlie the revealed population trends. Besides habitat conversion in the study area, climate is thought to be responsible for the long-term changes in species diversity and abundance. The main presumption is that populations of moths, as representatives of ectotherms, could be sensitive to even slight increase in temperature and fluctuating precipitation. It is hypothesised that species, which share similar life-history and ecological traits response in similar way to the environmental changes. The present study also attempt to summarise temperature limits for development of wide range of insect taxa and investigates general rules, which could explain the species differences.

All three papers are very well written and easily understandable to even those ecologists (like me), which are not too familiar with the model taxa and studied processes. The unique data of long-term population trends are accompanied by additional meteorological data, data on life histories and the third manuscript summarises published data on species thermal window suitable for their development. These large data-sets enabled to perform sophisticated analyses, which are able to identify most relevant explanations of the observed variances and trends in the data. The analyses are brilliant and, what I have to highlight here, interpretations of the results are very clear and appropriately careful. It is obvious that such an extensive work must be based on a collaboration of a larger team. However, I have no doubts about the significant contribution of the applicant to all the three studies presented.

My main criticism concerns the introduction of the thesis, which I found not very comprehensive. Though the methodology of long-term monitoring of insects is very interesting, the title and aims of the thesis imply that the reader will learn more about the response of biota (or just ectotherms) to ongoing global changes. I imagine that the introduction will be more focused on the synthesis of this actual problem. This would show more clearly the capability of applicant to review critically current knowledge about this phenomenon.

Specific comments to particular manuscripts:

MS #1:
The first two manuscripts are based on unique, long term data on population changes of moths monitored in Prague suburb. I think the only weakness of these data is that they come only from one

locality (and I understand that hardly possible to obtain from more), which makes general conclusions to some extent difficult.

1. The ecological classification is based on habitat preferences of caterpillars. Do the adult moths (which were monitored) share the same preferences?
2. I think you use two types of classification – habitat generalist/specialists and then finer categorization of habitat preferences. It would be useful to explain their connection in methods and use these terms more clearly throughout the text and mainly in abstract (which is in a way the most important part for most of the readers).
3. Why do you predict that habitat generalists would reflect more sensitively changes to of abiotic factors? You tried to demonstrate this but I can imagine that these predictors only correlate with other, biologically relevant causes. For instance, generalist species with open structure of populations (many of them are migratory) could response to changes of habitats (e.g. succession) on a larger scale than only at the study locality; they also could for instance respond more specifically to changes in use of herbicides? Could you discuss it shortly?
4. Although you discuss adequately the possible influence of habitat changes at the study locality, you left out one possible methodological problem. Large areas were built-up within the range of the light trap during the study period. I think the light pollution increased accordingly and it might have an effect on some specific groups of moths, e.g. those sedentary habitat specialists, which were attracted by closer sources of light or those species with highest sensitivity to light spectra used in street lamps?
5. I think more information on light-trap sampling should be included – was it done continuously?
6. I think the mean annual temperature is basic, but not very useful variable (with its variance of only 0.2°C for all years) for understanding of biological processes standing behind population trends. For example, increasing temperature could positively affect larval development, but on the other hand, higher winter temperatures could affect negatively the survival of dormant stages. I think it would be also worthwhile to analyse not only the general abundance of species in particular years, but also start and duration of the season when particular species on wings?
7. You state that you did not find any significant changes in temperature and precipitation. How did you test it?

MS #2:

The second manuscript tries to examine the relation of population trends of moths to their life history traits. Despite a lot of work has been done, only few predictors have been identified as responsible for similar population dynamics in species groups with similar traits. However, it is concluded that most of the species´ populations are density-dependent.

1. I think it would be fine to explain to readers, which important mechanisms are responsible for this density-dependence in moths – resource overexploitation, predation, diseases?
2. As far as I understand, Bulmer´s value and population fluctuation are inter-correlated because the fluctuation is already incorporated in Bulmer´s value. Is therefore relevant to test their relationship?

3. You state in the third paragraph of the Discussion (p. 75) that significant density dependence in population dynamic appeared as a rule, independently on population trend. But the Bulmer´s value has sense only in populations with no trend. Can you explain this mismatch, please?

MS #3:
In my opinion, the third manuscript is the most matured from all the three. It already underwent strict reviews in Functional Ecology journal done by much more experienced referees than I am. I think the most important result of this study is very strong empirical evidence of the theoretically predicted 20 °C range of the thermal window in insects and probably for all ectotherms. However, surprisingly to me, you were able to explain only about 14% of the variability of data on interspecific differences, from which only 0.6% could be attributable to ecological traits of particular species. Therefore, what do you think about that to what extent could be your conclusions generalized for insects? Second, do you have an idea what other (biologically relevant) variables or what kind of bias by thermal window estimates could theoretically explain some of the rest of the 86% of variability among taxa?

In conclusion, I am convinced that the present thesis demonstrates the scientific competence of the applicant and I recommend it for acceptance.

In Prague, 27 August, 2008

Ondřej Sedláček