DEVELOPMENTS IN HYDROBIOLOGY

The Diversity of Aquatic Ecosystems

edited by
Hendrik Segers and Koen Martens

Springer

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Aquatic Biodiversity II

Developments in Hydrobiology 180

Series editor
K. Martens

Aquatic Biodiversity II

Edited by

H. Segers & K. Martens

Royal Belgian Institute of
Natural Sciences, Belgium

Reprinted from Hydrobiologia, volume 542 (2005)

Springer

Library of Congress Cataloging-in-Publication Data

A C.I.P. Catalogue record for this book is available from the Library of Congress.

ISBN 1-4020-3745-7

Published by Springer,
P.O. Box 17, 3300 AA Dordrecht, The Netherlands

Printed on acid-free paper

Printed in the Netherlands

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H. Segers & K. Martens (eds), Aquatic Biodiversity II
DOI 10.1007/s10750-005-0886-x

The future of freshwater biodiversity research: an introduction to the target review

Hendrik Segers
Freshwater Biology, Royal Belgian Institute of Natural Sciences, Vautierstraat 29, 1000 Brussels, Belgium
E-mail: hendrik.segers@naturalsciences.be

During August 2003, an international meeting was organized to mark the handing over of editorial responsibilities over *Hydrobiologia* from Henri Dumont to Koen Martens. Such a milestone in the history of this important scientific journal with aquatic research as scope is a perfect opportunity to reflect on the future directions that this research should, or probably shall, take. Indeed, science and scientific interests not only evolve as a result of inherent drivers like the development of novel paradigms and the application of new techniques, but also from shifts in societal demands towards research: as the human impact, and reliance on natural resources increases, so does the need for a scientifically sound and sustainable management and conservation of these resources increase.

The Belgian Biodiversity Platform, an initiative by the Belgian Science Policy Office, offers scientists and other stakeholders in research a forum to exchange views on biodiversity issues, amongst other services (see http://www.biodiversity.be). Taking advantage of the August 2003 'Aquatic Biodiversity: Past, Present, Future' (Antwerp, Belgium) meeting, the Platform's forum on freshwater biodiversity initiated a discussion on the future direction of freshwater biodiversity research by inviting a target paper, and organizing a workshop on the topic. The following contributions in this section reflect the lively debate generated by this initiative.

In their target paper, De Meester & Declerck (2005) highlight several priorities in freshwater biodiversity research, in a way that takes into account both short-and long-term scales. Their plea for scientific rigor in research, whether fundamental or applied, is supported by all contributors, and reflects the conviction that only high-standard research combined with research management (van der Werf, 2005) can lead to reliable insights.

Interestingly, several of the contributions (e.g. Franklin, 2005; Gopal, 2005a, b) illustrate the need for improved communication at all scales, and understanding between scientists, policy makers and other actors in biodiversity issues. This was exactly the motivation of the Belgian Science Policy to initiate the Biodiversity Platform (see van der Werf, loc. cit.). However, finding an optimal balance between fundamental, bottom-up and science-based vs. applied, top–down or policy-oriented research remains difficult. Whereas De Meester & Declerck (loc. cit.) stress the importance and scientific merit of fundamental ecological research, Franklin (loc. cit.), Gopal (loc. cit. a), Lévêque & Balian (2005) and van der Werf (loc. cit.) highlight the high relevance of more applied research, with a strong focus on conservation needs, not only for society but also for science. Martens & Segers (2005), on the other hand, argue that a renewed effort towards fundamental and applied taxonomic research, as reflected by the Global Taxonomy Initiative (GTI) of the Convention of Biological Diversity (CBD), is called for. Clearly, this debate is by no means exhausted and will undoubtedly remain vigorous. However, it is reassuring to note that, whatever the position of the contributors, all defend the idea that the importance of both approaches calls for a well-guarded balance between them.

Lévêque & Balian (loc. cit.) argue that many ecosystems, especially in temperate regions, are historically relatively young hence immature, being inhabited by almost haphazardous assemblages of organisms. Ecologists should be aware of this, and accordingly should set realistic goals for research on, and the conservation of, such systems.

Further interesting thoughts on the valuation and economic value of biodiversity are developed by Dumont (2005).

While some contributions contain explicit recommendations for research priorities, some also reflect on the science policy and, more specifically, on the structure of research funding opportunities. De Meester & Decerck (loc. cit.) identify a bias in EU programs towards large networks of big institutes and large-scale projects. Whether this policy is justified or not is largely left in limbo, but they argue that national or regional funding agencies should counter the potential risks of this approach (loss of expertise in smaller consortia, few funding opportunities for truly innovative research) by developing an approach that is complementary to that of the EU. Sturmbauer (2005) largely agrees to this, but goes further in stressing the importance of a science policy that is independent from, and in some aspects even opposite to, the strategy of the EU.

In all, it was a gratifying experience to witness the wide range of views expressed during the debate. It would be naïve to think that a single workshop could resolve the problems raised, as a continued and adaptable effort is required to answer many of the needs identified. However, I sincerely hope all contributors will agree that the workshop discussions, and this target review, can be instrumental to the development of a science policy that answers appropriately to the challenges posed by the present-day biodiversity crisis.

Acknowledgements

I gratefully acknowledge the support of the Belgian Science Policy Office to the organization of the workshop and target review, and Koen Martens and Springer publishers for the opportunity to publish this target review in *Hydrobiologia*.

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Hydrobiologia (2005) 542:1–9
H. Segers & K. Martens (eds), Aquatic Biodiversity II
DOI 10.1007/s10750-005-0887-9

Opinion paper (target review)

The study of biodiversity in freshwater habitats: societal relevance and suggestions for priorities in science policy

Luc De Meester* & Steven Declerck
Lab. Aquatic Ecology, KULeuven, Ch. De Beriotstraat 32, 3000 Leuven, Belgium
*(*Author for correspondence: Tel.: +32-16-323708, Fax: +32-16-324575, E-mail: Luc.DeMeester@bio.kuleuven.ac.be)*

What should be the priorities in biodiversity research in freshwater habitats? The present target review builds upon a discussion text on this issue that was written upon invitation by Hendrik Segers. The text served as a preparation for a discussion session during the workshop 'the future of biodiversity studies in freshwater habitats', held in honour of Henri Dumont in Beveren, August 2003. The present text is influenced by some of the more striking remarks and ideas that have been formulated during this discussion session, which inspired us to consider different viewpoints. We gratefully acknowledge Hendrik Segers for his invitation to write the discussion paper, the attendants of the workshop for the interesting and lively discussion, and Dr Gopal for giving an impressive and realistic outline of the problems faced by biodiversity research in developing countries. We also want to acknowledge financial support from EU-project BIOMAN EVK2-CT-1999-00046, which got us fully involved in the field of biodiversity research.

In the following text, we first set the scene by positioning biodiversity research within the broader framework of measures and strategies to deal with the biodiversity crisis and sustainable development. We subsequently give an outline of the aspects we think are important from a purely scientific point of view, but with attention for the generation of relevant scientific information needed for the safeguarding of biodiversity. Finally, we discuss a few aspects related to science policy, with special attention to the tension between academic and applied science. Even though some statements are inspired by the discussion session during the workshop, we like to stress that this text mainly reflects our personal opinion. The first version of this opinion paper did not contain any references; during revision, we were asked to include approximately 10 key references.

The relevance of biodiversity research for the management of the biodiversity crisis

How do we cope with the biodiversity crisis, and how can science contribute to its management? One of the basic problems we are facing is that, on a global scale, political decisions are most often inspired by economic motivation (e.g. export), irrespective of improved laws and scientific knowledge. Such decisions have dramatic consequences for biodiversity. During the workshop, Dr Gopal in his lecture 'nicely' illustrated this overwhelmingly sad situation, and illustrated with many examples that scientific knowledge on biodiversity often does not have any power against the laws of economy, where natural resources and the richness of biota are often the victim of the trade-off between protection and the argument of 'feeding the poor'. So we can ask the question: is science on biodiversity necessary and of highest priority?

No doubt, the argument of 'feeding the poor' is a very strong one. It is sometimes abused in the sense that it is used as a synonym for economic development in terms of export which does not always feed back to the benefits of the majority of the poor, local people. If feeding the poor is done in a non-sustainable way, it is clear that it will only help for a short time, and that it may worsen the situation on a longer term, precisely because natural resources have been destroyed. Nevertheless, in practice, the potential long-term benefits of biodiversity conservation are most often ignored, because of the trade-off with the immediate need

for the exploitation of natural resources. The sad thing, so often exemplified, is that natural resources are too heavily exploited and that their actual value becomes clear only after they have been destroyed. One then starts to realize that their destruction only temporally supported needs, needs that, however, most often remain unresolved...

No doubt, science on biodiversity can be useful in this context. There is both a need for a good biodiversity science and for good biodiversity scientists (see further). It should, however, also be stressed that many of the problems we are faced with can actually be solved without any additional science on biodiversity itself. Solving the most crucial of problems involves political decisions, which implies a strong motivation to imply true sustainable development combined with political courage. Second, more than data on biodiversity itself, there is a need for technical solutions to the problems. As Henri Dumont formulated it in his final comment during the discussion, we need ideas and solutions to reduce the ecological footprint of people. As the global size of the human population is growing, there is a reduced potential maximal footprint that can be allocated to each person. Reducing population growth combined with technical solutions to live a decent life without having too high an impact on the environment is the only way out. In essence, this often requires technical, engineering solutions. So political courage and technical solutions for the dilemmas we are faced with are the highest priority. However, good scientists on biodiversity, as good environmental scientists in general, are badly needed as they provide data that keep politicians and engineers focused on these issues.

It should be very clearly stated that it is not a good idea to wait to implement proper political decisions and the search for technical solutions because the 'data on biodiversity are incomplete'. Biodiversity researchers should not promote the need for their data to the extent that the lack of data can be used as an excuse not to act now. Courageous political decisions and solutions to deep-cutting dilemmas between economical development and the use of natural resources (e.g. in agriculture, forestry, fisheries) should be implemented as efficiently as possible. Neither 'applied' nor 'academic' science in biodiversity is going to solve the problem. In the meantime, however, science on biodiversity can continue to contribute significantly by providing data that allow to fine-tune and improve the efficiency of the implementation of policies and technical solutions. So the lack of better data should not prevent action, but the collection of better data should be a high priority in order to improve the efficiency of action as soon as possible. We think it is important that this distinction is made.

Scientists have a bad track record in the sense that they often promote their own scientific endeavour, in an effort to attract money for their research group. Within their field, they are the obvious experts, and if they claim that their field is important, who can argue against that? Admittedly, it is realistic to say that scientists sometimes over-emphasize the importance of their own field, inspired by the way funding agencies operate. This is a bad situation, as this may feed the growing perception among non-scientists that scientists only want to promote their own position. Consequently, doubt arises about whether their claims need to be taken seriously. It is clear that many people actually believe that major environmental crises (e.g. global climate change, the biodiversity decline, pollution problems) are perhaps not as bad as depicted by scientists. This is inspired by the combination of the existence of controversies among scientists – a normal scenario for scientists but somewhat difficult to grasp for many non-scientists ('if global climate change is a fact and so overwhelmingly important, how can there be so much discussion and uncertainty about it?') – and the 'suspicion' that scientists overemphasize the problems. Many policy-makers also have this feeling, which is actually one of the reasons why they so strongly emphasize policy-driven research. Unfortunately, this emphasis on policy-driven research may just make things worse, as it stimulates scientists to emphasize the importance of their field of expertise in solving societal problems, and actually selects for scientists that are good in public relations. By itself, it is good for scientists to be good in public relations, but it should be realized that people that are good scientists and good communicators ànd good managers and good politicians at the same time are exceedingly rare...

Science: biodiversity in freshwater habitats

Let us first state that, purely from an ecosystem services to society point of view, the study of freshwater habitats is of utmost importance. Freshwater habitats are among the most threatened and valuable ecosystems we have. Although there is a continued need for describing patterns of biodiversity in freshwater systems, many descriptive studies have already been carried out, so we feel that there is a need for more encompassing and novel approaches. Important and innovative science can be conducted by, for instance, focusing on the issues listed below. In general, we would strongly advocate research that either develops one of the following issues in great detail or combines several of them for a specific model habitat type. The reader will notice that the structure is so that several issues are stressed repeatedly but viewed from different angles – we see this as an illustration of the fact that these issues are indeed important, and that there are different aspects to it. The order in which we deal with the different issues is not reflecting their importance. Rather, there is an underlying logic from more descriptive issues to more mechanistic approaches to the study of biodiversity. Obviously, the list is not exhaustive – after all, it is a discussion text. Our text no doubt is influenced by the fact that we see biodiversity in an ecological context. As such, we did not focus on taxonomic research, even though it is clear that proper taxonomic knowledge is important in collecting the data needed for the questions below.

- An *ecosystem, multigroup or multitrophic* approach: instead of focusing on just one group of model organisms, it can be highly revealing to study patterns of biodiversity at different trophic levels simultaneously: are the patterns related, what are the consequences for quantifying biodiversity at a macro-level and in terms of nature conservation (Declerck et al., in press). Obviously, the study of bacterial diversity may here shed a novel and strongly different light on patterns generally reported.
- *Attention for often-ignored habitat types*: Most studies in the world are being carried out on typical ecosystems such as deep lakes, or streams and rivers. There is a need of attention for specific types of habitats that are often ignored in classical monitoring studies. Typical and understudied habitats are small ponds and pools, ephemeral waters, ephemeral swamps, ponds and lakes in floodplain areas, ... These often smaller systems can also be profitably used as model systems to study specific aspects (e.g. connectivity, metapopulation dynamics, time stress, organism specific dispersal capacities (e.g. Rundle et al., 2002).
- *Attention for often-ignored components of the biota*: By including taxonomic or (better) functional groups that are often ignored, one can contribute to a better understanding of patterns of biodiversity and its consequences. In freshwater systems, such neglected components could be the microbial community in sediments, in the periphyton, and in the plankton. Furthermore, the resting stages of various groups that are present in sediments are an important component to be studied. Resting propagule banks tend to integrate population or community variability in time and space, and may contribute to the improvement of biodiversity assessment techniques (Vandekerkhove, 2004). In addition, resting egg banks are of immediate interest to nature conservation because they may be a prerequisite for the re-establishment of rare or 'extinct' populations following restoration measures. Note that these aspects have also direct relevance to the framework of the Water Directive, in that they may provide better indicator systems to monitor water quality than the ones suggested at this stage. Especially for standing waters, this may be an important contribution. A special and, in our opinion, important issue is the potentially evil side of diversity: it is quite important to engage in studies on the biodiversity of pathogens in relation to the diversity of the habitat, other biota, etc.
- *Genetic diversity*: There is a general misconception that genetic diversity is less important to nature conservation than species diversity. Often, studies on genetic diversity are considered to be of scientific interest only. In contrast, one should realize that genetic diversity and capacity for evolution are central to species survival. Studies on genetic diversity may reveal the underlying processes that determine taxon diversity in natural habitats. It should not be

concluded that this is only interesting from an evolutionary point of view: micro-evolution is only possible when sufficient genetic diversity is present, and given the rapid human-induced changes in our environment, the capacity to evolve in a short term will be crucial to the survival of many populations and species (Frankham et al., 2002). Without insight in the genetic aspects of diversity, we will never be able to understand the loss of taxon diversity or at least not be able to exclude purely genetic effects. It has also direct bearing on conservation practices: if habitat fragmentation leads to population extinction, it is very important to know whether this is due to smaller population sizes per se or by a reduced dispersal and gene flow, because the measures to be taken to mitigate the problem are different. Studies focusing on genetic diversity in addition to taxon diversity are therefore highly wanted. Importantly, recent studies have shown that the use of neutral genetic markers to infer evolutionary potential can be highly misleading. Neutral genetic markers can be very important to study population structure and infer historical processes. For the study of evolutionary potential, however, it is essential that studies are being conducted on ecologically relevant traits. Such quantitative genetic studies are laborious and difficult, but highly needed. The development of methods (e.g. QTL studies, the development of genetic markers for specific traits) can also be highly rewarding in this context.

– *Auto-ecology of specific focal organisms*: For specific organisms that are either rare or functionally important, it may be worthwhile to conduct an encompassing study on its auto-ecology, including studies on habitat requirements, action radius and dispersal capacity, competitive strength, population genetic structure, etc. Such studies can be important in a context of strongly fragmented landscapes. A similar endeavour may also be very useful for more common species that may be considered good model systems for a broad category of freshwater organisms.
– *Determinants of diversity*: A good understanding of the factors and mechanisms that determine diversity is a major scientific challenge and is essential for the development of efficient conservation strategies. Extant patterns of diversity in ecosystem types should be well documented. These patterns can be related to the abiotic environment, community architecture and the landscape context, and be used for the generation of hypotheses. Descriptive studies, however, seldom allow inferring causality. The next step should, therefore, consist of hypothesis testing. This requires well-designed and replicated large-scale experiments in the field (e.g. enclosures) or in the laboratory (mesocosms).
– *The landscape context*: Recently, the relative importance of local and regional factors in determining genetic and taxon diversity is receiving increasing attention (Shurin & Allen, 2001; Cottenie et al., 2003). This issue has strong bearing on nature conservation, since restoration of local conditions may or may not be enough to restore biodiversity, depending on the degree to which regional factors are limiting. Freshwater habitats are among the most attractive model systems to study the effects of landscape context, connectivity, habitat size, etc., because in general, the limits of the habitat are very well delineated in the terrestrial landscape. Corridors, dispersal highways, dispersal bottlenecks and dispersal barriers are easily identified in the field, at least when dealing with obligatory aquatic organisms. An important aspect of the regional setting is the potentially negative aspect of connectedness. It is generally assumed that corridors and landscape connectivity are good for nature conservation. Yet, there are negative aspects to it, such as the spread of exotic species, diseases, etc. Studies encompassing those aspects too would be very welcome.
– *Functional diversity*: It is not enough to study patterns of biodiversity in isolation. What does biodiversity mean in terms of system resilience, ecosystem functioning and ecosystem services to society? It is of the highest priority that well-designed experiments are carried out to determine functional consequences of biodiversity (Giller et al., 2004). What is the importance of taxon or functional diversity for ecosystem functions like productivity, litter decomposition, nutrient cycling, or trophic interactions? Such