DELIVERABLE FRONTPAGE

Project number: 243827 FP7-ENV-2009-1

Project acronym: LC-IMPACT

Project title: Development and application of environmental Life Cycle Impact assessment Methods for improved sustainability Characterisation of Technologies.

Deliverable number: D5.4

Deliverable name: Guidance document to finalise LCIA method development, based on the stakeholders consultation outcomes.

Version: 2

WP number: 5

Lead beneficiary: JRC

Nature: R[1]

Dissemination level: PU[2]

Delivery date Annex I: M33

Actual delivery date: 29/01/2013

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Comments: #

[1] Please indicate the nature of the deliverable using one of the following codes: **R** = Report, **P** = Prototype, **D** = Demonstrator, **O** = Other

[2] Please indicate the dissemination level using one of the following codes: **PU** = Public, **PP** = Restricted to other programme participants (incl. the Commission Services), **RE** = Restricted to a group specified by the consortium (incl. the Commission Services), **CO** = Confidential, only for members of the consortium (incl. the Commission Services)

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1. Executive summary

LC-IMPACT's main goal is to further develop robust impact assessment methodologies for LCA. Such ambitious goal entails, on the one hand, the need of scientifically robust methods and on the other hand, the need of wide stakeholder acceptance for ensuring the uptake and the actual utilization of the methods. This requires in-depth interaction with and feedback from the scientific community and the public stakeholders. Therefore an external consultation has been organised in order to get feedback from respectively domain experts and stakeholders concerning the newly developed LCIA methods within LC-IMPACT.

This document provides an overview of the consultation process and the main outcomes as guidance to finalise LCIA method development.

The *domain experts' review* has been organised in order to receive detailed comments on each deliverable and to give the possibility of discussing the comments in a workshop on the 6th of November2012. Eighteen domain experts, previously asked to provide detailed feedback on each task of the project, joined the researchers of LC-IMPACT. Together they have discussed the newly developed LCIA methods. The meeting was very fruitful. The main points analysed and debated were:

1. Trade off between precision and applicability
2. Completeness of scope
3. Testing of the validity of the models
4. Uncertainty analysis ("the spread around the outcome")
5. Recommended level of spatial detail (country level, watershed etc.)

The aim for the *on-line public consultation* was to get feedback back from LCA practitioners and end-users. The public consultation ran from the 22th of November until the 23th of December 2012 and was announced via the LC-impact website. As result of the on-line public consultation, we received comments by 7 stakeholders, mainly addressing aspects related to the WP1, on resources. The WP leaders have looked in to the comments and formulated a reply.

2. Public stakeholder consultation

An external consultation has been organised by EC-JRC and PRé in order to get feedback from respectively domain experts (i) and stakeholders (ii) concerning the newly developed LCIA methods within LC-IMPACT. Characterization factors and normalization factors were outside of the scope of this public consultation, due to the fact they were developed in a later stage of the project. The idea behind the experts and public consultation is that methods may be further improved and subsequently uptaken, following the recommendations given by the domain experts and stakeholders.

The consultation with domain expert (i) was aimed to have detailed feedback on each newly developed method within LC-impact to improve and finalise them.

The on line consultation (ii) was aimed to involved a broader range of actors providing written answers on the website of LC-IMPACT (www.lc-impact.eu). The emphasis of this round was to have feedback from end users, such as industry and policy makers.

3. Domain experts review and workshop

The domain experts' review has been organised in order to receive detailed comments on each deliverable and to give the possibility of discussing the comments in a workshop. The public consultation was announced at the SETAC conference in Berlin via flyer (see annex 7.1).

The template for giving comments will consist in an evaluation of models against the criteria defined in the EC-JRC 2010[3] framework document for each impact category. This has been coupled with a template for giving additional/specific comments, which can be found in annex 7.2 , in which the reviewers were asked to provide a summary of the review and the key issues, as well as a more detailed reporting for each method, entailing the assessment of:

- Completeness of scope
- Environmental relevance
- Scientific robustness & Certainty
- Documentation & Reproducibility
- Applicability
- Specific remarks (indicating page/line of the document)

3.1. List of appointed domain expert/reviewers

The aim of the expert consultation was in-depth interaction with and feedback on newly developed methodologies. The list of invited experts has been compiled with the support of the LC-impact partners. Eighteen domain experts were asked to review the interim deliverables and the methodologies. These experts were selected from the scientific community because of their knowledge in specific fields. In table 2.1 an overview is given of the domain experts whom participated in the expert consultation.

Table 2.1: List of appointed domain experts and assigned topic/WP

#	Name	WP	Topic	Mail	Institute
1	Bo Weidema	1	Abiotic Resources	bow@lca-net.com	LCA-NET
2	Ottar Michelsen	1	Land	ottar.michelsen@ntnu.no	NTNU
3	Jo Dewulf	1	Land + abiotic resources	Jo.Dewulf@ugent.be	UG
4	Ian Vazquez	1	Marine resources	ianvazquez2002@yahoo.es	TUDOR
5	Ole Eigaard	1	Marine Resources	ore@aqua.dtu.dk	DTU
6	Markus Berger	1	Water	markus.berger@tu-	TU Berlin

[3] EC-JRC. *ILCD Handbook. Framework and Requirements for LCIA Models and Indicators*; EC-JRC: Ispra, Italy, 2010; p. 112. Available online: http://lct.jrc.ec.europa.eu

				berlin.de	
7	Bradley Ridoutt	1	Water	Brad.ridoutt@csiro.au	CSIRO
8	Antonio di Guardo	2	Toxicity	antonio.diguardo@uninsubria.it	University of insubria
9	Nilima Gandhi	2	Metals and whole effluents	nilima.gandhi@utoronto.ca	University of Toronto
10	Matt Macleod	2	Tox:spatial differentiation	matthew.macleod@itm.su.se	ITM
11	Arno Rein	2	Pesticides	arnr@env.dtu.dk	DTU
12	Magnus Breitholtz	2	Whole effluents	Magnus.breitholtz@itm.su.se	ITM
13	Jyri Sepalla	3	Acidification and euthophication	jyri.seppala@ymparisto.fi	SYKE
14	Jaap Struijs	3	Acidification and euthophication	Jaap.Struijs@rivm.nl	RIVM
15	Marko Tainio	3	Fine particulate matter + ozone	marko.tainio@ibspan.waw.pl	Systems Research Institute
16	Enrico Benetto	3	Noise	enrico.benetto@tudor.lu	TUDOR
17	Guido Reinhardt	1-3	LCA methodology	Guido.reinhardt@ifeu.de	IFEU
18	Martijn Schaap	3	Fine particulate matter + ozone	Martijn.schaap@tno.nl	TNO

3.2. Reviews received by domain expert

Adopting the template in Annex 7.2, the experts provided their comments (full reports available as Annex 7.3). The comments were generally positive, addressing methodological and implementation issues. The main points were:

1. Trade-off between precision and applicability
2. Completeness of scope
3. Testing of the validity of the models
4. Uncertainty analysis ("the spread around the outcome")
5. Recommended level of spatial detail (country level, watershed etc.)

The reviews were distributed to WP and task's leaders in order to prepare the interaction with domain experts during the workshop.

3.3. Workshop in Brussels

On the 6th of November 2012 a workshop was held in Brussel to discuss the feedback of the domain experts (figure 2.2 for the program of the day). All domain experts whom were asked to give feedback on the developed methodologies participated to the workshop in Brussel with exception of Bo Weidema. All the participants of the day can be found in annex 7.4.

Figure 2.2: program of the 6th of November 2012

10.00 – 10.30	Welcome , Highlights and cross cutting issues		Mark Huijbregts
10.30	WP1 Resources	WP2 Toxicity	WP3 Other impacts
10.30 – 11.30	Land	Metals	Eutrophication
11.30 – 12.30	Water	Whole Effluents	Acidification
12.30 – 13.30	Lunch		
13.30 – 14.30	Marine resources	Organic chemicals	Fine particulate matter + ozone
14.30 – 15.30	Metals + Fossils	Organic chemicals	Noise
15.30 – 16.00	Summarize key issues	Summarize key issues	Summarize key issues
16.00 – 16.30	Coffee break		
16.30 – 16.40	Main outcomes WP 1		Stefanie Hellweg
16.40 – 16.50	Main outcomes WP 2		Ralph Rosenbaum
16.50 – 17.00	Main outcomes WP 3		Philipp Preiss
17.00	Closure		Mark Huijbregts

In the morning the members of the three work packages and the respective domain experts had separate sessions. These session were structured into 4 parts of each an hour, 1 part for each task. In the beginning, the respective task leaders presented the content of the deliverable. Afterwards, one or two reviewers presented the main points of critique, which were subsequently discussed. Every expert gave a presentation with their main point of critique. All the presentations of the experts can be found in annex 5.6. There was a lively and fruitful discussion in all the session. The minutes of the morning sessions are available as annex 7.5

In the afternoon, the outcomes of the different work packages were discussed in plenary. Small comments of the experts have been and will be dealt with directly by the LC-IMPACT team. The more critical comments will be discussed within the work packages and will be dealt with accordingly. All agreed changes will incorporated in the final deliverables.

4. On-line Public consultation

The aim for the public consultation was to get feedback back from LCA practitioners and end-users. To make public consultation more accessible for all stakeholders a summary for each newly developed methodology has been prepared by the task leaders. These summaries can be found here on the LC-impact website. All the summaries entail the following elements:

- List of authors and affiliations
- Overall summary
- What is the assumed environmental mechanism underpinning the methodology
- Cross-cutting issues discussed in general meetings as:
    - Regional differentiation
    - Documenting uncertainties, and if relevant the adopted perspectives
- Assessment of progress beyond the state of the art,
- The very most important references

The Public consultation ran from the 22th of November until the 23th of December 2012 and was announced via the LC-impact website. Furthermore the public consultation was announced in:

- The Pre LCA discussion list
- European Platform on Life Cycle Assessment
- The network of the LC-impact team members.

The participants of the public consultation were asked to make use of the template that can be found in annex 7.7.

The Management Board decided for to go for the commonly accepted review period of one months which is also a common consultation period used by the EC-JRC for recommendations and documents (policies, guidance etc). It was decided not to have a longer public consultation because of the tight schedule of the LC-impact project, all the methodologies needed to be ready by the end of April 2013. With preparing the methodologies for public consultation and giving the partner's time to review and incorporate the feedback from the public consultation a limited time was available.

4.1. Results of the public consultation

As result of the on-line public consultation, comments by 7 stakeholders have been received, mainly addressing aspects related to the WP1, on resources (see annex 7.8 for overview). The issues emerged from the consultation are: the relationship between newly developed methods and the related inventory data needed for applying the methods; the modelling of specific chemicals, such as metals; the need and the relevance of the whole effluent toxicity approach; and methodological issues related to resource modelling.

The replies to the comments by the WP and Task leaders are reported in the last column of the received comments (see annex 7.9 for the collided comments).

The number of respondents in the public consultation was disappointing. However, the choice of involving stakeholders in commenting the summary of scientific methodological development was an attempt to set a more trans-disciplinary setting. Usually, public consultations involve policy documents or standards; hence, the interest of stakeholder is higher. Besides, the LC-impact methodologies are quite complex and a certain level expertise on impact assessment is needed to assess the work. This is not always easy found through a public consultation, especially when you take in account that the field of impact assessment is quite small.

5. Involvement of experts

In all stages of the LC-impact project input from experts has been collected where possible. Numerous presentations and workshops have been given by the LC-impact partners at various conferences such SETAC and LCM Berlin. Feedback on these presentations was whenever possible used to improve the developed LC-impact methodologies.

Specifically relevant workshops were:

- A public workshop was organized to address the challenge for method developers and LCA practitioners how to deal with uncertainty issues in the context of Life Cycle Assessment (LCA). The workshop took place on January 20th 2012 in Zurich, Switzerland and was organized in two parts. Six platform presentations and lectures were held in the morning and two practical exercises were performed in the afternoon. The workshop was well attended by representatives of the consortium members (approximately 25 people joined) the workshop gave insight in how you could deal with uncertainty in the LCIA field.
- Participation by SIK in an international workshop in Italy with fisheries scientists to present the study and learn more about the state of knowledge. The workshop was a valuable exchange of knowledge
- A workshop at World Resource Forum on September the 19th 2011 by ETH, PRé and SIK. During this workshop the four types of resources which were further developed by LC impact, namely use of mineral and fossil resources, biotic resource, water and land were presented. An important part of this workshop was the dicussion with the participants on the issue of concern regarding resource use.
- A stakeholder workshop that took place on October 4th 2010 in Brussels regarding the development of an indicator for resource depletion. 11 experts, policymakers and representatives of industry discussed what kind of indicator they would prefer to use as a resource depletion indicator.

These workshops were deliverables of the work package for which they provided input so respectively WP 1 to 3.

6. Conclusions

The consultation with field experts was extremely beneficial for the refinement of the proposed impact assessment methods. It was fundamental both involving them in the workshop and asking to peer review- in a structured way- the methods during the development thereof. The area of improvement and refinement entail:

1. Trade-off between precision and applicability
2. Completeness of scope
3. Testing of the validity of the models
4. Uncertainty analysis ("the spread around the outcome")
5. Recommended level of spatial detail (country level, watershed etc.)

Looking back it would be recommendable for future projects to evaluate how to properly involved stakeholders in research project as their perception of the need of providing comments could be not very high.

7. Annexes

7.1. Flyer at SETAC conference in Berlin (May 2012) announcing the public consultation

The following is an excerpt of the newsletter distributed in May 2012, reporting the announcement of the workshop and the public consultation. The full text of the newsletter could be retrieved from http://www.lc-impact.eu/en/newspage/newsletter-may-2012.

Workshop announcement: Public consultation

LC-IMPACT's main goal is to further develop robust impact assessment methodologies for LCA. This ambitious goal requires in-depth interaction with, and feedback from the scientific community. Hence, an external consultation will be held in order to get feedback from domain experts and stakeholders, concerning the newly developed LCIA methods and characterization factors within LC-IMPACT. The basic idea is that methods and factors are further improved, following the recommendations given by the domain experts and stakeholders.

The Joint Research Centre (JRC) of the European Commission and PRé Consultants will be in charge to organize this consultation with scientific experts. In a second step following the expert consultation, on line consultation with written answers on the website of LC-IMPACT (http://www.lc-impact.eu/consultation) will be carried out. The emphasis of this second round will be to have feedback from end users, such as industries and decision makers.

The suggestions from the domain experts and stakeholders will give guidance to the researchers for the final stages of the development and execution of the work.

We cordially invite you to take part in this process, supporting the scientific enhancement of new methods for Life Cycle Impact Assessment.
If you wish to participate to the expert consultation, you may send an email to lc-impact@science.ru.nl for receiving further information and signing up.

For the broader stakeholder consultation, please refer to http://www.lc-impact.eu/consultation for more information.

7.2. Template expert reviewers

This template was designed for the domain expert acting as reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	
Date	
Reviewer	
Contact (e-mail)	
Contact (phone)	

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
Additional comments				
Suggestions for work package – or task leader:				

Environmental relevance

Irrelevant				Relevant
Additional comments				
Suggestions for work package – or task leader:				

Scientific robustness & Certainty

Weak				Robust

Additional comments
Suggestions for work package – or task leader:

Documentation & Reproducibility

Weak				Robust

Additional comments
Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable

Additional comments
Suggestions for work package – or task leader: -

Specific remarks (please indicate page/line in the document)

7.3. Combined feedback expert review

The combined feedback of the expert review are reported in the following pages

Template for reviewers of LC Impact
This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	**D 1.4 and D 1.2, Task 6 (seafloor impact)**
Date	October 22, 2012
Reviewer	Ole Ritzau Eigaard
Contact (e-mail)	ore@aqua.dtu.dk
Contact (phone)	+45 21154565

Summary of the review and key issues

Given the objective stated in the deliverable text - to take product-related quantification of the biological impact of fishing a step further than state of the art at the beginning of the project – progress has certainly been made.

A number of potential impact categories have been explored wthin four main groupings: Group 1) Wasted potential yield (WPY), F-overfishing and B-overfishedness, Group 2) Red list Index (RLI) and Vulnerable, Endangered and Critically endangered (VEC) discard to catch proportion, Group 3) a seafloor impact factor defined by *affected area*restoration time*, and Group 4) Mean Trophic Level (MTL) and Primary Production Required (PPR).

The impact categories from two of the above groupings (1 and 2) are assessed as directly applicable in seafood LCAs, whereas one group (3) has been assessed as potentially applicable (following further refinement), and one existing/established approach (4) has been assessed as not applicable in its present form, and furthermore is questioned with respect to its general reliability in providing guidance for better governance of the world's oceans.

I mostly agree with the above conclusions regarding the applicability of the impact categories examined. I.e. the immediate applicability of the group 2 categories (RLI and VEC), the potentials of the group 3 category (*affected area*restoration time*) and the lacking applicability of group 4 categories (MTL and PPR).

With respect to the immediate applicability of group 1 categories (WPY, F-overfishing and B-overfishedness, I am more uncertain. I see a lot of potential in the concept of defining and using an MSY related impact factor in seafood LCAs, but in its present form there are some scientific uncertainties of the WPY definitions that need to be resolved before this mid-point category can be applied directly in seafood LCAs. The scientific uncertainty pertains mainly to how the concept of fishing mortality (F) is perceived and used in the definition and calculation of WPY, which is somewhat different from the perception of F within traditional stock assement and fisheries management.

I have detailed my WPY reservations – and suggestions for ways forward – in the sections below relating to completeness of scope, etc., where I have also evaluated the other fisheries relevant impact categories explored in D 1.4 and D1.2.

Key issues

As I see it, the development of seafood LCAs should head in the direction of a producing a broadly applicable tool to simplify, compare and convey complex information of fishing impacts on the ecosystem for any given case study. By definition the construction of such a tool will require a very large degree of trade-off between minimzing the complexity reduction (information loss) and maximizing the operationality of the tool. I have a little concern whether the LCA framework, being such a well established methodology within non-renewable resource economy, is actually well suited (it might be too "rigid" in methodology) to accommodate the large biological variability of the renewable marine resources or "substances". If, however, these concerns are to be met by fulfilling the final deliverable objectives of an applicable LCA based impact assessment tool for fisheries, I believe the key is to strike the right level for the trade-off between precision and operationality – i.e. to define both meaningfull(informative) and applicable impact categories.

Completeness of scope

Incomplete				Complete
		X		

Additional comments

Given the objective stated in the deliverable text: to take product-related quantification of the biological impact of fishing a step further than state of the art at the beginning of the project, progress has certainly been made, and in the context of this (rather narrow) objective I would say the scope of the deliverables achieved - the definition of two directly applicable (Red List Index (RLI) + Vulnerable, Endangered and Critically endangered species (VEC) discard to catch proportion + two promising impact factors (potential wasted yield [WPY] and a seafloor impact factor defined by *affected area * restoration time*) - is complete.

If, however, broadning the objective somewhat, the impact categories brought forward are certainly stil relevant but can only be considered part of a complete scope for a seafood LCA. Two obvious (and relatively well established) impact categories to include are the ones immediately below and I suspect there a few more "easy to include" impact factors, which would make the scope more complete.

- Fuel consumption
- Emission of greenhouse gasses/global warming impact

Broadening the scope even further, for instance to align with the sustainbility concepts of the ecosystem based approach to fisheries management and of the CFP revision, it would also be relevant to consider whether the below listed aspects are necessary for a complete scope of seafood LCAs:

- *Social impact factors*
- *Economic impact factors*

Suggestions for work package – or task leader:

Investigate the possibilities of including impact categories of social and economic sustainability into Seafood LCAs *vis a vis* current efforts (e.g. in the CFP reform) of developing an ecosystem based approach to fisheries management.

Environmental relevance

Irrelevant				Relevant
			x	

Additional comments

The environmental relevance of the approach taken and the mid-point categories investigated is high and - as detailed above under completeness of scope - my suggestions for broadening the perspective of the seafood LCA methodology pertains mainly to investigating the possibilities to include social and economic aspects.

Having said that, I do, however, also believe that the environmental relevance of the approaches taken has room for improvement. Without being an expert on the contents and wording of the EC's Marine Strategy Framework Directive (MSFD), I suspect that MSFD definitions of Good Environmental State (GES) of the marine environment and its suggestions for associated indicators could provide valuable guidance as to the definition and refinement of impact categories to be used in seafood LCAs.

Presumeably additional and potentially usefull mid-point categories for environmental impact - or improvements of the already developed impact categories - can be identified using the contents of the MSFD. This could for instance be methodology/suggestions to include of non-fish species (e.g. various invertebrates) in the RLI or VEC impact factors.

Ensuring that the actual categories, as well as the terminologies deployed, are compatible with the contents of the MSFD, will presumeably ease the way for a broader use of seafood LCAs in routine advice and fisheries management.

Suggestions for work package – or task leader:

Look to the Marine strategy Framework Directive (MSFD) for guidance/inspiration on appropriate descriptors and indicators (and impact factors) for good environmental status of the marine ecosystem.

Scientific robustness & Certainty

Weak				Robust
		X		

Additional comments

The impact categories examined in the deliverable are commented in turn below with respect to the scientific robustness and certainty of estimates.

The wasted potential yield (**WPY**) definitely has substantial potential as a meaningful and informative impact category in seafood LCAs, but in its present form it is not scientifically robust. There is a mis-match between the meaning of the term "fishing mortality" in the draft deliverable and the meaning of fishing mortality (F) in a traditional VPA-based stock assesment context. In the latter context F, refers to the fishing related part of the total instantanious mortality rate, Z, of the number of individuals of a given cohort (age class) of a spieces. The remaining component of Z is defined as the natural mortality (M). An good straightforward introduction to traditional stock assessment – and the meaning of F (as a part of Z) in traditional VPAs - is found in cooper (2012) from which a nice presentation of the relationship between Z and annual mortality rate is borrowed (appendix 1).

The VPA definition of F is also the traditional foundation for establishing biological reference points, such as Maximum sustainable yield (MSY). The establishment of MSY is based on yield per recruitment calculations for a range of age based fishing mortality levels. This has to be done on a species by species basis depending on specific growth parameters and natural mortalities.

This is where the main methodological weakness of the WPY factor comes in; the wasted potential yield from not conforming with Fmsy can only be quantified on a species by species basis because such a quantification requires yield per recruit curves, which are defined with species specific growth parameters. In other words, two stocks with the same F/Fmsy ratio will not necessarily have the same ratio of harvested yield/potentially harvested yield.

As I see it there are two ways forward: 1) re-defining the WPY impact category to conform with VPA fishing mortality and MSY concepts, which requires inclusion of yield per recruit information on a species basis, or 2) reconsider the WPY category in its nature and aim for an index or proxy for the wasted potential yield, which presumebly would require less work (and maybe enable a very simple F, B, Fmsy and Bmsy expression), but also provide more uncertain estimates. In this latter case it would be necessary to be very clear on the diverting definitions of fishing mortality.

The **RLI** and **VEC** impact categories are scientific robust in their definition, in addition to being simple and informative , but they do to some degree have a lack of certainty in their estimates. This uncertainty is, however, mainly a result of limitations in the input data used. As a consequence of this their applicability is also somewhat reduced. A "weakness" of both is that they are very similar and basically convey the same information. Therefore nothing much is gained by using both in the same LCA although one returns absolute values (VEC) and the other returns semiquantitive index values (RLI). With respect to choosing between calculating VEC in numbers or weight discarded per kg of catch, I would suggest numbers, as I expect that the red list component of discard is made up of several species of very different sizes ranges.

The suggested seafloor impact category ***affected area*restoration time*** - and the underlying method for estimating swept area and impact intensity for various MarLIN habitats affected - is fairly straightforward and scientifically fairly robust. Swept area is, however, not just "swept area" in terms of seabed impact. Demersal fishing gear (including trawl gear) has very variable degrees /severity of bottom contact depending on the species targeted. Assuming equal impact per square meter swept of e.g. a beamtrawler and a sand eel trawler is a crude simplification and some differentation with respect to gear type presumeably has to be made. Likewise the effective gear width will vary substantially with gear type and vessel size and this variation has to be addressed somehow, if reliabele estimates of seafloor impact are to be produced.

Presumeably the intensity (spatial distribution) of effort also needs to be defined in terms of scale of resolution/ proportion of subarea (MarLIN habitat type) actually affected. For example a given MarLIN subarea may be estimated to be exposed to a yearly fishing effort (total swept area) corresponding to 3 times the total area, but in reality the total effort is very spatially concentrated resulting in that one fourth of the total subarea is being swept 12 times and three fourths are not swept at all.

At present there are no "off the shelf" solutions to the methodlogical improvements pointed at above, but several research projects aiming to improve assessment of seafloor are currently ongoing (e.g. EU-FP7 Benthis)

I agree with the deliverable conclusions; that the estimates of the two potential impact categories, **MTL** and **PPR** of catches and discards, will be associated with substantial uncertainty. I am concerned with reliability of estimates for two primary reasons: 1) because discards of fisheries - and consequently the data feeding into the impact assessment - are not very well documented or monitored, and 2) because the assumption of changes in the trophic levels (and PPR) of catches being mainly a result of overfishing is not straightforward; are we in fact experiencing a global decline in trophic level of the fish stocks reflected in the catches? or is it rather a shift/expansion of fishing effort towards exploiting new species at lower trophic levels, which were not exploitable previously due to e.g. technological constraints? and what about variation in regulations and environmental impacts at a regional scale?

As for the RLI and VEC, commented above, the MTL and PPR are very similar and basically convey the same information. In that sense they are to some degree mutually exclusive as impact categories in the same LCA.

Suggestions for work package – or task leader:

It is recommended that the current use of the fishing mortality concept is aligned with the traditional definition used in fisheries management and that the WPY expression in the deliverables text (equation 1) is revised to better confrom with the MSY concept. Two approaches for moving forwards are given above.

Furthermore it is recommended that in the coming development of the WPY and the seafloor impact categories contact is made to participants in the two ongoing FP7 EU research projets, MYFISH and BENTHIS, where guidance and synergy effects will most likely be found.

Documentation & Reproducibility

Weak				Robust
				X

Additional comments

Documentation and reproducibility of how WPY, F-overfishing, B-overfishedness, RLI, VEC, MTL, PPR and seafloor impact categories are defined and estimated, seems robust.

As described above, under uncertainty, the same cannot be said for many of the data feeding into the calculations of the impact categories. This is particularly a concern with respect to discard information, but also many of the other impact categories have issues of limited data quality and coverage, meaning that the data requirements for reproducibility in estimates across fisheries, are not always easy to meet. Examples are the many stocks lacking MSY data and the many fishing areas and vessels where seabed information and effort location is only available on rather crude scale. Improvement of the input data situation would also improve the reproduciblity of impact category estimation across the fisheries to be compared.

Setting out to obtain better discard estimates or to define MSY reference points for additional fisheries is, however, beyond the scope of the deliverable LC impact project. The current input data situation is a condition to accept - not to change - and within the framework/mandate of the LCA impact project, documentation and reproducibility of the suggested categories is robust.

Suggestions for work package – or task leader:

None

Applicability

Not applicable				Applicable
			X	

Additional comments

In theory the examined impact factors are easily applicable across a range of fisheries. In reality, limitations in availabilty and reliability of input data (catches, discards, stock parameters, effort, seafloor/habitats and red lists) to a large degree restricts a broad applicabilty.

As discussed above, under reproducibility, the issue of limitations in input data is a working condition of the effort to meet the objectives of D1.4. and D1.2. My impression is that the development work undertaken so far has been realistic with respect to the imitations of input data and has pusuied a “best possible praxis” approach, searching for impact categories with reasonable information levels and realistic data requirements. Within scope of the LC impact project I find the applicability of the suggested impact categories good.

Suggestions for work package – or task leader:

Put faith in a large number of ongoing efforts to improve monitoring/data collection of the environmental impact of fisheries and of the marine habitats themselves.

Specific remarks (please indicate page/line in the document)

The general definition of the fishing mortality (foot note on page 10) is different to the one traditionally used in traditional fisheries management and cohort-based virtual population analyses (VPAs) as it takes place in e.g. ICES.

Table 3.3 on page 34 gives a category "RE" which is not explained in the table caption

Appendix 1. Presentation of the relationship between instantanious mortality rate Z (=F+M) and annual survival & mortality rate (from Cooper, 2012: http://www.seagrant.unh.edu/stockassessmentguide.pdf)

Instantaneous Mortality Rates

The total instantaneous mortality rate (*Z*) equals the instantaneous natural mortality rate (*M*) plus the instantaneous fishing mortality rate (*F*). If scientists have estimates for *M* and *F*, they can calculate both annual mortality and annual survival using a table such as the one below.

Total Inst. Mortality Rate (Z)	Annual Survival Rate	Annual Mortality Rate
0.0	100 %	0.0 %
0.1	90.5 %	9.5 %
0.2	81.9 %	18.1 %
0.3	74.1 %	25.9 %
0.4	67.0 %	33.0 %
0.5	60.7 %	39.3 %
0.6	54.9 %	45.1 %
0.7	49.7 %	50.3 %
0.8	44.9 %	55.1 %
0.9	40.7 %	59.3 %
1.0	36.8 %	63.2 %
1.5	22.3 %	77.7 %
2.0	13.5 %	86.5 %
2.5	8.2 %	91.8 %
3.0	5.0 %	95.0 %

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D1.2
Date	21/10/2012
Reviewer	Ian Vázquez-Rowe
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Contact (phone)	+(352) 691-242334

Summary of the review and key issues

Marine spatial planning and global positioning sytem (GPS) technology are used in this deliverable to improve the computation of seafloor impact ("land use") in LCA. Prior studies had limited the analysis to the computation of the swept area (Ziegler et al., 2003; Vázquez-Rowe et al., 2012). One single paper has also attempted at linking swept area to a specific location through GIS (Nilsson and Ziegler, 2007).

Completeness of scope

Incomplete				**Complete**
				X

Additional comments

The final section of deliverable 1.2 completes an ongoing work at SIK to report land use impacts in marine ecosystems. I would say that the future perspectives will include an evaluation of the direct damage that the gears exert on the seafloor and synchronizing marine land use impacts with those that are being developed for terrestrial production systems.

Suggestions for work package – or task leader:

Environmental relevance

Irrelevant				Relevant
				X
Additional comments				
Suggestions for work package – or task leader:				

Scientific robustness & Certainty

Weak				Robust
				X
Additional comments				
Suggestions for work package – or task leader:				

Documentation & Reproducibility

Weak				Robust
			X	

Additional comments

For reproducibility comment, please check the following section

Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable
			X	

Additional comments

-The use of this impact category is not only useful for fisheries, but its use can also be extended to other marine issues, such as coastal building, tourism, marine constructions, etc.
- However, it remains unclear how the IC deals with differing levels of damage on the seafloor. For instance,

Suggestions for work package – or task leader:

- Difficulty in obtaining this type of very specific data will be a constraint to the applicability of this specific methodology. Therefore, it would be interesting to guarantee a high level of transparency for other future research studies.

Specific remarks (please indicate page/line in the document)

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D1.2 Land
Date	Oct. 19, 2012
Reviewer	Jo Dewulf
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Summary of the review and key issues

At page 7, the goal is set for methods for the assessment of land use on ecosystems services, biodiversity and human health.

Subsequently, a number of chapters are elaborated. In chapters 1 and 2, biodiversity is covered, but in two different ways. The first chapter brings a spatially differentiated approach based on species richness. It results in characterization factors (table 1.2) per land type use. In general this is widely applicable for 8 types of land use. Opportunities to improve are further refinement within the land use types as there can be quite different practices within one land use type, e.g. annual crops.
It is unclear to the reviewer to what extend this subpart of the LC-IMPACT is differentiated from the UNEP/SETAC "Operational Characterization Factors for Land use Impacts on Biodiversity and Ecosystem Services" project.

In chapter 2, biodiversity is covered again but now based on a functional diversity base instead of species richness. This is a rather new approach from a scientific point of view, but its potential for implementation is far more narrow as it (1) covers only land occupation and not transformation; (2) it is geographically limited to America; (3) land use types (table 2.1) do not include artificial areas (urban area). Unlike chapter 1 with table 1.2, it misses a comprehensive overview of the characterization factors.

Chapter 3 focuses on climate change impacts induced by wood extraction. Although it is an interesting scientific elaboration, it may be questioned if it can serve as a general method for land use impact: (1) only one particular land use type is under study: forests. (2) the environmental impact considered are rather the impact on global warming as a result of emissions, that subsequently results in health damage and biodiversity effects. A proper definition of the targeted cause-and-effect chain, especially in order to differentiate with other environmental impact categories like global warming, is needed. One should be careful in double counting in this way.
Technically, (1) it is not so clear if greenhouse gases like N_2O have been considered systematically: is this checked with forestry specialists? Section 3.3: impact is quantified per m³ wood: is this the best base/unit for land use? At the same time, if this wood enters the market "on a global scale" (see title of chapter): this wood for sure is then substituting other products: a consequential approach would be appropriate.
In this chapter the way the characterization factors are calculated is not so clear.

Chapter 4 deals with damage costs due to erosion. Erosion was not mentioned explicitly as one of the impacts to be assessed. However in the course of the text, in particular further on in Chapter 5: introduction, it becomes clear that it can be considered as one of the factors affecting the ecosystem services. Again this chapter is not as generic as chapter 1, as it only concentrates on one land use type: agriculture crops.
Technically: section 4.2: why is the characterization factor per yield of crop ($Y_{x,s}$) ? Is this a good base? In other words: is erosion linear with crop yield? Has this been checked with erosion/agriculture specialists? Cost model is used: is this the proper base to quantify ecosystem services, as it may quantify economic losses in agriculture, i.e. agricultural practices ending up in higher level of erosion versus agriculture ending up with lower level of erosion, rather than benchmarking erosion induced by agriculture versus erosion observed with the potential vegetation state. Figure 1 (legend not clear): characterization factors for wheat globally: does this make sense? Checked with agricultural specialists? If so: wheat in combination with green manure and in some crop rotation?
Similar as with Chapter 3, the impact is quantified per product: kg crop (Figure 2): is this the best base/unit for land use?

Chapter 5 also deals with erosion, where it becomes clear that this is one of the five ecosystem goods and services. Also here the presented work is not applicable globally for land use impacts as it specifically looks to one specific type of land use: agricultural crops for energy production.
From a scientific point of view, the approach is innovative as it makes use of the emergy concept. Two points of concern can be raised however. It falls back on numbers from Odum (p 76). It can be questioned if the mechanisms considered by Odum to quantify the emergy quantity are transferable to the objective in this work. Secondly, the authors considered soil erosion but left out the soil formation (p 94). In making an overall erosion balance, it is questionable if soil formation can be simply omitted.
Technically: section 5.2.2 (p 82): ecosystems with higher NPP values are considered more valuable: is this a proper assumption? What is the base? Section 5.3.1 is confusing: what is now assessed: erosion regulation potential as one of the 5 ecosystem services? Or is it biotic production potential? Or is it (abiotic or biotic) resource depletion? Or is it ecosystem quality? See also table 5.2: were resources and ecosystem quality in the scope of this chapter?

In chapter 6, the impact of fishing on seafloor is studied. It is not so obvious if this subject fits fully in the scope of "land use". This is especially the case if you look back to the previous chapters: sea floor was never studied. Nevertheless, it is not so clear what exactly the authors like to assess: human impact? Ecosystem services? Biodiversity?

Overall, it can be concluded that chapters 1 and 2 are most generically applicable, i.e. characterizing impact of land use on biodiversity. This is also the most obvious and direct link. Impacts on ecosystem services and human health are far more difficult. The authors give the impression that effects of a land-based product is assessed, rather than the land use as such. This comes clear from some characterisation factors that are based on the products obtained from the land use. For these, the authors have brought some specific characterization factors that may have applications in particular studies; however double-check with agricultural/forest specialists is recommended.

Completeness of scope

Incomplete				Complete
		x		

Additional comments

See general comments

Suggestions for work package – or task leader:

See general comments

Environmental relevance

Irrelevant				Relevant
			x	

Additional comments See general comments
Suggestions for work package – or task leader: It would be useful to have guidance for the practitioner to know in what case what methodology is advised. E.g. should I characterize biodiversity based on chapter 1 or based on chapter 2?

Scientific robustness & Certainty

Weak				Robust
	x		x	

Additional comments Robustness and certainty fully depend on the chapter
Suggestions for work package – or task leader: See environmental relevance: it would make sense to inform the potential user on the robustness and certainty of the specific methods. See also ILCD

Documentation & Reproducibility

Weak				Robust
			x	

Additional comments

Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable
	x		x	

Additional comments

Applicability is different for the different methods proposed.

Suggestions for work package – or task leader:

Again, the potential user would benefit from some guidance with respect to the specific applicability of the specific methodologies.

Specific remarks (please indicate page/line in the document)

See general comments

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D 1.2
Date	19 October 2012
Reviewer	Ottar Michelsen
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Summary of the review and key issues

Completeness of scope

Incomplete				Complete

Additional comments

According to the document "The goal of Task 1.1 is to develop operational and scientifically sound methods for the assessment of land use on ecosystem services, biodiversity and human health. Characterization factors for land use impacts have been developed with specific attention for ecosystem services as well as biodiversity which have hardly been assessed within the framework of Life Cycle Assessments." Further it is stated that "The ILCD currently does not recommend an endpoint method for life cycle impact assessment of land use. Therefore research efforts of D1.2 mainly focused on impacts on biodiversity, human health damage and additional costs. Direct as well as indirect pathways were assessed."

Impacts from land use raise a lot of questions and based on the document, it is not possible to see how all of these are addressed and thus the focus in the work. To mention a few of the most important; land use and land use changes have a range of impacts. Here biodiversity (primarily vascular plants, chap 1), functional diversity (chap 2), changes in carbon stocks (chap 3), erosion and soil properties (as ecosystem services, chap 4 and 5) and impacts on seafloor (chap 6) are addressed. No doubt, these can be highly affected by LULUC, but these are not the only ones. Why are these selected and not others? It is not likely it will be possible to cover all, the list of potential ecosystems services that could be affected is massive, but still it would be interesting to see more about the selection process. I assume this is described in the research plan, but I did not find this information. How many impacts from LULUC should be included before the assessments are regarded as 'complete'?

A second important point that should be mentioned here is the possibilities to have a more common format on these impacts from LULUC; common reference situation and basis for characterization factors. The reference situation in most cases is potential vegetation, but in chapter 3 changes in present harvest is used, i.e. present state, not potential state, is used as reference. For assessing CFs, both relative changes to a regional value (e.g. biodiversity) and global maximum values (soil depth) are used. IF, and if is probably the right word, different impacts from LULUC should be harmonized to e.g. ease data collection, visualize trade-offs between different impacts from LULUC etc, this seems problematic when reference situation and basis for calculating CF is not consistent over the different impacts from LULUC. A question is thus if this question should have been treated on a more generic level in the project.

A third question is the danger of double counting for some of the proposed approaches. When changes in species diversity is assessed and related to a specific land use, e.g. agriculture including use of chemicals, the use of chemicals will in most cases also be assessed in an LCA and open a possibility for double counting of the effect of these.

The proposal on impact on seabed seems to be on an early stage in development.

As a conclusion – completeness is a question of the actual scope.

Suggestions for work package – or task leader:

Address the questions addressed above – reference situation, basis for characterization factors.

Environmental relevance

Irrelevant				Relevant
			X	

Additional comments

As stressed in most of the chapters, the issues dealt with are of high importance for environmental performance of production systems and are such highly relevant for LCA that claims to be a holistic tool. I welcome more research in this field and recognize the need of methodological development. It is also important to realize that if methods are to be implemented in LCA and used on a regular basis by practitioners, there have to be some simplifications that in most cases will reduce the environmental relevance. Still, there are some questions I feel is not really addressed in depth here.

Is species diversity, and in particular vascular plant species diversity a good measure on biodiversity? Chapter 1 actually shows in a very good way the challenge of using alpha diversity as indicator for biodiversity. To move forward, abundance data for more taxonomic group from more places of the world is needed, but it is unlikely that there will be an overwhelming access to such datasets in the near future. So, are indicators chosen in such a manner that what is intended to be assessed actually is assessed?

For both chapter 1 and 2 the process of using plot data and relate these to a regional reference value, and use these for assessing CFs on ecoregion level is somewhat unclear. It is not clear to me how close a reference point (natural or close to natural vegetation) need to be to be regarded as a reference point. In chap 2 the impact of this choice is to some degree discussed by using different data as reference.

In chapter 2 the focus is on functional diversity, but due to the process of selecting/creating functional groups, it is not clear what functions are actually assessed. The idea behind functional diversity is primarily that one species can replace others if they disappear, i.e. that functions are not necessarily lost even if species are. It is thus very surprising for me that CF in some cases are higher for FD than SR – this should then be understood as the impact on functions is regarded to be higher than the impact on species. How can this be explained?

In chapter 1 it is shown that the taxonomic groups are so broad that different sub-groups respond significantly different (in particular vascular plant vs mosses) that there is a danger that the selection of groups actually masks impacts? In chap 2 it is possible to discuss the choice of categories included in the construction of functional groups. What happens if more functional traits are included or some excluded? How sensitive would the results be? E.g. reproductive potential for mammals seems to be a potential trait, similar pollination strategy for plants or type of photosynthesis (C3/C4/CAM)?

In chapter 3 it is a focus on climatic impacts from changes in logging. For a complete assessment, also other impacts than changes in C stocks should be addressed, such as albedo and evapotranspiration, in order to avoid giving misleading results.

Suggestions for work package – or task leader:
Increase the focus on what is actually assessed and what is not.

Scientific robustness & Certainty

Weak				Robust

Additional comments
In general, data availability for most methods are low, as also pointed out, and the robustness is consequently rather low as well. Based on the presented material, it is not possible to really say to what degree this influences the proposed CFs. A question I do not find addressed is the use of average data for huge areas; ecoregions or countries. In the chapters where both countries and grid cells are used, it is clearly documented that the use of average data masks some severe impacts. It is thus questionable if average data should be recommended as some sort of default value when there obvious are cases with significantly higher impact. It is at least relevant to raise the question of whether worst case data should be used if not detailed data is available, not average data. A possible scenario is that companies etc. reporting based on such CFs would prefer average data to avoid detailed and worse values…
Suggestions for work package – or task leader:

Documentation & Reproducibility

Weak				Robust
			X	

Additional comments

Data sources and assumptions are overall well described.

Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable

Additional comments

These are new methodological proposals. Some, as in chapter 1, can be regarded as improvements of existing methods, while others are new. The applicability has to be shown through case studies. In chapter 5 a case study is included, but I am still left with a 'so what' feeling. What is the implication of the findings; should present practice be changed, should production be relocated? This is also a valid question for findings in other chapters. Should the results in chapter 3 be interpreted as wood harvest should be increased in South-America, Canada, Malaysia, Philippines, … but decreased in USA, Russia, Scandinavia, New Zealand… ?

For some of the chapters, I miss data tables in addition to the figures. In chap 3 it would be useful to have the data on assessed changes in CO_2 to the atmosphere.

Suggestions for work package – or task leader:

Case studies to test applicability.

Specific remarks (please indicate page/line in the document)

Page 65 – refers to table S1 – as far as I see this is not included. Page 67 – I guess it is ($/kg crop), not /kg soil as written in the text.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D 1.3
Date	22 October, 2012
Reviewer	Bradley Ridoutt
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Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments

This work is well situated in the context of previous research and makes important new contributions regarding:

1) Impacts of consumptive water use on inland Ramsar wetlands of high significance, and
2) The estimation of ET (evapotranspiration) by PNV (potential natural vegetation)

The latter will simplify assessment of the reduction in water flows *to water resources* as a result of land use change/management which leads to an increase in ET compared to PNV. That said, this is probably not a common situation. Agricultural lands typically have lower annual ET compared to the complex natural ecosystems they replace.

What could be suggested for future research is a focus on modeling impacts associated with a reduction of ET, meaning that flows from the land base are increased. These impacts might include increased levels of erosion, waterlogging, flooding, salinisation and even perturbations in the water cycle which might affect precipitation patterns and hence water availability. The need for close connection with researchers working on impact assessment of land use is highlighted as it is not yet agreed where such impacts should be modeled.

Furthermore it would be helpful to clarify if the green water method is also intended to be used in contexts where precipitation is harvested, e.g. dams, reservoirs, rainwater harvesting systems

associated with buildings and other urban infrastructure. Regarding the wetland method, the researchers themselves identify opportunities for future research including the assessment of additional species groups, such as mammals, consideration of coastal wetlands where saltwater ingress occurs as the consequence of upstream freshwater use, as well as further temporal delineation. In addition, I would add the desirability of modeling the *positive* biodiversity aspects from human created wetlands, such as rice fields.
Suggestions for work package – or task leader:

Environmental relevance

Irrelevant				Relevant
			X	
Additional comments The wetland method is highly novel, fulfilling a well-defined gap in life cycle impact assessment of water use, and the environmental relevance is very high. The initial focus on birds is justified and the method developed represents a major advancement. The green water method, which has less novelty, addresses the situation of an increase in green water consumption which, I believe, is not so common. The green water method is appreciated, but the environmental relevance is not so high.				
Suggestions for work package – or task leader:				

Scientific robustness & Certainty

Weak				Robust
			X	

Additional comments

For the wetland method the scientific approach is considered sound and uncertainty regarding data quality is discussed. A more precautionary approach could perhaps be considered with respect to the proportion of return flow to soil which is assumed to replenish ground water. Regarding model uncertainty, please see comment below.

For the green water method, I was surprised to see no reference or use of the widely known and used (approx 400 citations) model of Zhang et al. (**WATER RESOURCES RESEARCH, VOL. 37, NO. 3, PAGES 701-708, MARCH 2001**).

Review by a ground water hydrologist is suggested if this has not already occurred. The connectivity between surface and groundwater is complex. I wonder whether the 3 situations modeled are sufficiently comprehensive [i.e. 1) SW withdrawal affecting SW-dependent wetlands, 2) direct SW withdrawal (i.e. from the wetland itself) affecting GW-dependent wetlands, and 3) GW withdrawal affecting GW-dependent wetlands], or whether high connectivity between SW and GW in some cases means:

1) GW withdrawal can alter SW flows and subsequently SW-dependent wetlands
2) Indirect SW withdrawal (i.e. not from the wetland itself) can alter GW recharge and subsequently GW dependent wetlands

I do not have the hydrological expertise to comment further, hence the suggestion for review by an expert in this field (most likely from outside the LCA community).

Suggestions for work package – or task leader:

Documentation & Reproducibility

Weak				Robust
				X

Additional comments

Both methods are well documented and have high capacity for being reproduced by another researcher.

Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable
			X	

Additional comments

Characterization factors for consumptive water use impacts on inland Ramsar sites of significance are provided, thereby enabling practical application. That said, more guidance would be welcomed regarding how far upstream from a wetland it is suggested to assess water use.

Regarding the green water method, the excel file in the Annex for Chapter 5 was not able to be assessed. However, no problems with applying this method are anticipated. As already mentioned, the major issue is the lack of subsequent impact assessment methods for situations where land use reduces ET compared to PNV and drainage and runoff are increased. This is a topic recommended for future research.

Overall, it would be helpful if the authors were to provide some additional commentary regarding the coherence of the new life cycle impact category indicators with existing end-point oriented life cycle impact assessment methods (EcoIndicator99, Recipe, etc)

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

Firstly, I wish to congratulate the researchers for their efforts and the important contribution which has been made to LCIA.

I appreciate greatly the endpoint-oriented modeling approach which has been adopted. I am witnessing a proliferation of proposed new midpoint indicators – most notably in the area of land use impact assessment. What concerns me is that these new methods are really only useful for making comparisons between products, systems and technologies on the basis of each indicator in an isolated way. These new midpoint indicators are not providing the ability to assess the relevance of the environmental impacts relative to other impacts, meaning that interpretation is extremely difficult and the assessment of tradeoffs virtually impossible.

To enable the assessment of water use impacts relative to other environmental impacts the endpoint approach described in Deliverable 1.3 is appreciated. The authors themselves note the difficulty in harmonizing the endpoint indicators (p. 10). This is not regarded as a flaw, but a consequence of the complexity of the task undertaken. However, further consideration on how to arrive at harmonized endpoint indicators would be valuable.

Other minor comments
In the wider LCA community, the term water use is increasingly being used to describe both consumptive and degradative (i.e. changes in water quality) aspects. As Deliverable 1.3 relates specifically to consumptive water use, it might be helpful to avoid the more general term (i.e. water use) and refer to *consumptive water use* throughout.

The definitions on page 8 were a bit odd: 1) Green water is defined as water consumption…, 2) It is not clear whether rainwater harvested is included in the definition of green water (this type of water is relevant in many LCA studies, but is not traditionally covered by the concept of green water introduced by Malin Falkenmark which referred to soil moisture), 3) The definition of water consumption is circular, i.e. water consumption = the use of water is a consumptive way…

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D 1.3
Date	24 October 2012
Reviewer	Markus Berger
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Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments

Many gaps in life cycle impact assessment of water use are identified and tackled. Especially impacts on wetland ecosystems are addressed comprehensively. The study quantifying biodiversity impacts from salinity in a coastal wetland, provides an interesting first step. However, in terms of "completeness of scope" it has to be mentioned, that the focus is on a particular Spanish wetland. Global characterization factors are so far missing and results cannot be transferred easily. In a similar way the method estimating green water consumption of potential natural vegetation provides the (very important) basis for impact assessment of green water consumption by enabling the determination of net green water consumption. However, a proper impact assessment scheme is still lacking which limits the "completeness".

Suggestions for work package – or task leader:

Limitations mentioned above are addressed in the individual chapters. May be they should be made more transparent in the summary as well.

Environmental relevance

Irrelevant				Relevant
			X	

Additional comments

By focusing on wetlands, which are both extremely relevant and threatened from an ecological perspective, this work addresses an important topic and gap in LCA. However, as shown in the results of the two case studies, impacts from area loss in the Peru wetland and from salinity increase in the coastal Spanish wetland seem to be of lower relevance compared to biodiversity damages from other environmental interferences. This can either reflect reality or be a consequence of methodological shortcomings in terms of the new methods underestimating or other methods overestimating ecosystem damages. Another possible explanation would be that endpoint results obtained by different methods cannot be aggregated or compared as they contain different assumptions or focus on different species and different endpoint effects (growth inhibition, deaths, etc.).
The work estimating global green water consumption of PNV is urgently needed as it provides the first usable tool to quantify net green water consumption – even though a proper impact assessment scheme is still lacking.

Suggestions for work package – or task leader:

If it is a finding from this research that land loss and salination impacts resulting from water consumption in wetlands are of minor importance compared to other environmental interferences causing biodiversity damage, this should be clearly mentioned. This does not mean that the work is not relevant. In contrast, it would be good if other scientists could "close this chapter" as there might be more relevant issues on which research should focus. If this conclusion is not justified, it should be explained why ecosystem damages of water consumption in wetlands are so small compared to other impacts.

Scientific robustness & Certainty

Weak				Robust
		X		

Additional comments

In this category I need to differentiate between the different chapters. It is an inherent problem that those methods developing endpoint damage factors (chapter 1-4) suffer from uncertainties as many assumptions have to be made and statistical regressions are not always significant. Further, the two case studies (chapters 3 & 4) are more robust as they assess damages for a concrete wetland and a particular situation. In contrast, chapters 1-2 take a global perspective where even more assumptions and generalization is required. Thus, uncertainties cannot be avoided but are addressed in a clear and transparent manner. If possible, uncertainties are quantified and sensitivity analyses were conducted – this is very well done!
Green water consumption of PNV is determined in a robust manner and also here uncertainty estimates are provided enabling reliable results. In contrast to the high scientific robustness on the inventory level, the framework for impact assessment (which is not the focus) is rather weak as no direct impacts of green water consumption are quantified and the proposal to use characterization factors for blue water consumption instead is not justified (see section "specific remarks" for further explanaition).

Suggestions for work package – or task leader:

In most chapters, uncertainties are addressed in an adequate manner. Only the impact assessment framework suggested in chapter 5 is not really justified in my opinion. I think this is not even necessary, as this work provides great information allowing for the proper determination of net green water consumption on a global level. So I suggest focusing on the inventory level in this paper rather than providing a questionable impact assessment framework. May be it would be helpful to provide guidance on how the evapotranspiration of the current land use type can be determined. This would complete the inventory challenge of determining net green water consumption flows.

Documentation & Reproducibility

Weak				Robust
		X		

Additional comments

In general the entire deliverable is documented well allowing for a good understanding of the basic ideas of the methods as well as the results and discussions. However, some methodological steps are explained very briefly making it difficult to really understand methodological details for scientists not involved in the development. Examples are provided in the section "specific remarks".
The supporting information usually support the understanding of the methodologies developed but seems a bit long in chapter 2 (new version). In chapter 3 more links should be given to the SI in the main text.
Moreover, the position of the case study in chapter 3 is misleading as in the abstract it is claimed to be the test of the method developed in chapter 1 and 2. However, this is not true as the methodology is not the same and this study was performed before. So it is rather the foundation on which the work of chapters 1 and 2 is based.

Suggestions for work package – or task leader:

In terms of documentation of the deliverable, chapter 3 should be included in the report but moved before chapters 1 and 2. If the report should serve as a guidance document for LCA practitioners and scientists, this basic case study is not really needed and a reference would be enough.

Applicability

Not applicable				Applicable
			X	

Additional comments

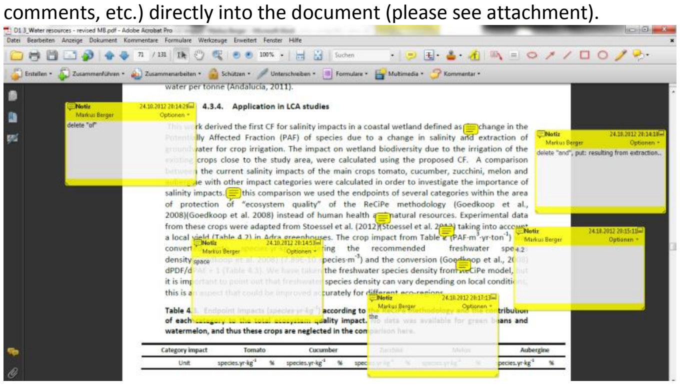
Fate factors developed in chapter 1 are available for wetland on a global level. Corresponding effect factors are so far provided for Spain only, which limits applicability of course. As soon as effects factors are available on a global level too (in progress), this method is perfectly applicable in LCIA. Chapter 3 shows a case study in Peru which served as a basis for the method developments – so applicability is not an issue here. The case study concerning salination in coastal wetlands (chapter 4) is an interesting first step but so far not applicable as results cannot be transferred directly to other wetlands. Chapter 5 allows for a global determination of net green water consumption by providing evapotranspiration figures for PNV, which can be subtracted from ET of the current land use type. Thus, applicability is perfectly ensured on the inventory level - only a proper impact assessment scheme is missing.

Suggestions for work package – or task leader:

I strongly recommend publishing characterization factors and PNV green water consumption figures by means of GIS shape files or in Google Earth Layers. This supports and simplifies applicability drastically and proofed to work well in methods of Pfister et al. (2009) or Boulay et al. (2011).

Specific remarks (please indicate page/line in the document)

As there is no line numeration in my document, I included many specific comments of minor relevance (spelling mistakes, editorial comments, etc.) directly into the document (please see attachment).



In the following, specific comments of MAJOR importance are mentioned along with suggestions for improvement:

1) It is stated that only consumption but not withdrawal of SW and/or GW leads to impacts in SW/GW-fed wetlands. This is based on the assumption that non-consumptive withdrawal is discharged back to the wetland. However, according to the general definition of non-consumptive water use, the discharge needs to occur within the same watershed – not-necessarily within the same wetland. Hence, for the first 4 chapters a new and more precise definition of water consumption is needed, which defines water consumption as evapo(transpi)ration, product integration, or discharge into seawater or **into areas outside the originating watershed.**

2) Chapter 1 (new version send by Francesca): Some concrete, numerical, results should be shown in the abstract.

3) Chapter 1 (new version send by Francesca): The wetland is modeled as a cone but no rationale is given. It should be justified, why a cone reflects reality better than other geometric figures like, e.g. cyclinders.

4) Chapter 1 (new version send by Francesca): The wetland cone is modeled in two ways – using the wetland area and the water surface as base of the cone. Using the surface water area and an average depth implies a virtual cone filled with water. This assumption is justified as a reduced volume leads to a decreased base area indicating the loss of wetland area. However, using total wetland area as base of the cone implies a virtual cone consisting of water and soil. So a reduced volume of water can probably not be directly translated into a reduced base area indicating the loss of wetland. I assume that effects of soil and porosity need to be taken into account. This needs to be addressed or at least discussed.

5) Chapter 1 (new version send by Francesca): In equation 4, the precipitation is added to $Q_{in,\ assessment}$. Can you explain this? Actually the precipitation is already included in $Q_{in,\ assessment}$ according to equation 2.

6) Fate factors of up to 86.5 $m^2\ yr/m^3$ appear extremely high. Can this really reflect reality?

7) Chapter 1 and 3: It should be mentioned that the water balances (equation 2 and 3.1) are used to determine $Q_{in,\ modeled}$. After understanding the full method this is quite obvious but I needed some time to figure out why you actually do this… It could also be mentioned that this balance is used as it appears more robust than models like waterGAP.

8) Chapter 2: It should be reflected in the title, that effect factors are available for Spain only (unless this chapter is updated with global factors anyway.)

9) Chapter 2: Equation 2.1 is quite complex and it should be explained how you developed this relationship. By trial and error? Is it a general equation in biodiversity assessment?

10) Chapter 2: The fate factors, provides a unit of m^2 of wetland lost. So according to traditional endpoint modeling, I would expect that the effect model provide something like PDF/m^3_{lost}. This is not the case as A_{new} and $A_{original}$ are parts of the equation. Furthermore, a change from 10 to 9 m^2 would lead to different results than a change of 1000 to 999 m^2. Here some extra explanation is needed.

11) Chapter 2: There is a weighting of the potentially affected fraction of species based on the rarity score. This sound reasonable but (I think) not really consistent with other methods assessing biodiversity damage from other environmental interferences (e.g. EI 99). This should be checked and if true mentioned, that results may not be directly comparable to other biodiversity damages.

12) Chapter 2: I don't understand the last three lines on page 27…

13) Chapter 2: Some of the correlations (2.3.2) should be shown in the supporting information

14) Chapter 3: In the introduction it is mentioned, that Pfister et al. (2009) developed a global method assessing impacts on terrestrial ecosystems but that impacts should be assessed on a local level. Pfister and colleagues use $NPP_{wat\text{-}lim}$ as an indicator for ecosystem vulnerability, which is local as well. So why do you consider this method as global and not local?

15) Chapter 3: Please explain how you came up with these complex equations. By trial and error? Are these default relations? How did you develop them?

16) Chapter 3: On page 40 (last lines) it is mentioned, that ISRx "is determined for each scenario x as a function of the groundwater level." What kind of function? Can you show an equation?

17) Chapter 3: On top of page 41 you state that "the amount of infiltrating water determines the change in water level and surface area of the wetland (A_x). The resulting groundwater levels for the scenarios are than compared to the base year." Without having read chapters 1 and 2, it is very difficult to guess that this has been done based on the cone equations. They are shown in the supporting information but should be shown in the main text or at least a link to the SI should be provided.

18) Chapter 3: If you compare damages from water consumption to biodiversity impacts from other environmental interferences (3.3.3) the concrete numbers should be mentioned, which show that impacts are actually relatively low. This is an important message and referring to the SI only is not enough here I think.

19) Chapter 4: In section 4.2.4 the effect factor is determined showing "the effect of salinity for various endpoints". Isn't this a strong inconsistency if for some species growth inhibition and for others death is considered as an endpoint? The point at the cause effect-chain is very different I would say.

20) Chapter 5: In the introduction on page 68 you say that "The difference in these green water flows represents the lack of recharge of groundwater and surface water run-off (so called blue water) and can therefore be assessed by characterization facors (CF) for water consumption". This is a fundamental assumption and no evidence is given that this is true. I have doubts that the additional evapotranspiration of 1 m³ of soil moisture leads to an equal lack of 1 m³ of blue water. This strongly depends on the local conditions and it may also be that only local soil moisture content is decreased and that blue water availability is not affected at all. As mentioned in the previous sections, this assumption has to be justified or changed. In my opinion this is not even relevant for this paper. I suggest focusing on providing robust green water consumption figures of PNV rather than touching a totally different topic of green water impact assessment. May be guidance on how evapotranspiration of the present land use type can be determined would be helpful for practitioners, which would allow for determining net green water consumption.

21) Chapter 5: In section 5.2.2 the site generic equation 5.1 is calibrated by means of AET_i estimations derived from equation 5.2. However, approach 2 can only be applied for those areas with natural vegetation. So how did you determine $k_{opt,i}$ for those areas currently used by human activities, where no comparison of approach 1 and 2 is possible?

22) Chapter 5: In section 5.2.4.1 you mention "…and that, at the same time, this plant green water consumption does not lead to intensify blue water shortage in surface and groundwater bodies downstream. This is reflecting that the overall effect of cultivation leads to increased blue water availability. Thus, we deemed green water consumption to be of marginally environmental relevance in humid areas. Accordingly, $d(WC_{green})$ was set equal to zero and the consumption of green water is ignored in the LCA study."

    In my opinion, several points get mixed here. You are establishing an LCI in this section and, thus, the pure elementary flows should be listed without any judgment of their impacts – this is the task of the impact assessment phase and not of the inventory. The second thought that is included in the section cited above is the issue of negative net green water consumption flows which are likely to occur, when e.g. forests are transferred into agricultural land in e.g. Central Europe. You should discuss how you want to deal with these credits. On the one hand they are justified as green water increases. On the other hand a green water increase due to lower evapotranspiration leads to decreased precipitation elsewhere. So the role of natural vegetation on the global water cycle cannot be ignored. The next question, which is also addressed in the paragraph, is how the altered land use and green water consumption influence blue water availability – which is a study on its own I would say.

23) Chapter 5: Even though it is obvious, I would mention in the inventory section (5.2.4.1) that $WC_{green,system}$ and $ET_{PNV\ opt,i}$ are geographically differentiated flows.

24) Chapter 5: In the life cycle impact assessment section (5.2.4.2), three options are suggested:
    a) Assessment without characterization (CF=1): This is a pure inventory quantity and doesn't make sense in the impact assessment stage
    b) Using CFs for blue water consumption: I think this is not valid, as there is no evidence, that 1 m³ of green water consumption leads to 1 m³ of blue water consumption (see comment 20))
    c) Weighting water consumption by a function of natural availability: This sound reasonable, but why do you take ETPNV as an indicator? Wouldn't the ratio of ET and P be more reliable and consistent with the scarcity indicators used for blue

water consumption assessment?

25) Chapter 5: Figure 5.1 should also highlight those areas which were not considered in your work (P/ET>0.75). Otherwise it looks like water consumption of PNV would be zero in many areas like Northern Europe or parts of the US.

26) Chapter 5: Would it be possible to show results also on the country level? The regionalization you chose makes more sense from a scientific perspective of course but often data are available on the country level in the LCI only.

27) Chapter 5: I don't understand the second paragraph of the discussion (5.4, p. 76).

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D1.4/Task 1
Date	21/10/2012
Reviewer	Ian Vázquez-Rowe
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Summary of the review and key issues

Section 1 of D1.4 tackles one of the main milestones in the improvement of the adaptability of life cycle thinking to fisheries. The use of stock assessment data to actually include them in the assessment in terms of overfishing and overfishedness is definitely an important step forward in improving the usefulness of LCA in, for instance, policy making, rather than simply using stock assessment data to interpret the results obtained in other environmental dimensions (e.g. Ramos et al., 2011). Nevertheless, a main constraint I see in the applicability of the methodology is the fact that mass is used as the unit of assessment, when I have serious doubts whether this allows performing comparability between different species. However, I do acknowledge that discussing this issue with Andreas at St. Malo, he agreed on the need to improve the methodology in these terms. Nonetheless, taking into consideration current policy making protocols in the EU, in which each fishing species is analyzed independently regardless of the marine web interactions, the methodology can offer great advantages at a CFP level, since it would integrate stock assessment in LCA, without disregarding the other environmental dimensions which have been pointed out as relevant in previous LCA studies.

Section 2 of D1.4 presents a clear introduction to the use of mean trophic levels (MTLs) in fisheries, and refers to robust scientific publications to note the risks and weaknesses of using this approach. The analysis on how the data obtained in terms of MTL and PPR in the Kattegat constitute an interesting perspective, especially since the constraints of using them as stock assessment methods may give rise to misinterpretations. A more precise definition of how different MT evolution patterns through time may be interpreted in differing ways is needed. Moreover, the link of abrupt changes in MTL through time should be analyzed also in terms of change in the management system of the fishery (i.e. regional and national restrictions, but also in terms of changes in TACs and quotas due to the Common Fisheries Policy).

Section 3 of D.1.4 evaluates the appropriateness of the Swedish Red List of marine fish species linked to fisheries management advice, as well as applying the selected approach to attribute impacts from incidental catches to a specific seafood product. While the idea is novel, I think that the clarity of this section must be improved substantially. For instance, the main hypothesis at the end of the introduction refers directly to the impacts from "incidental catches". According to FAO (FAO Fisheries Report No. 547, 1996; Kelleher, 2005), incidental catches is a concept "used in the context of rare incidents or events such as catches of marine mammals, turtles or seabirds." Therefore, it is quite difficult to follow since in the Methods section the author's state that their objective is to "study the amount of red-listed fish species discarded per kilo landing". Discards, according to FAO are defined as the "portion of the total organic material of animal origin in the catch which is thrown away or dumped at sea for whatever reason, dead or alive". I have also seen that different organizations (i.e. European Commission, NEAFC, etc) may have small changes in the definition of these concepts with respect to those used by FAO, which makes their use confusing in the international community. Therefore, I would suggest the authors make a thorough revision of all the concepts used in this section that may give room to misinterpretation, consider their substitution or redefinition.

Completeness of scope

Incomplete				Complete

				X

Additional comments

Section 1) This section is very complete and presents the problem of overfishing and overfishedness from an integral perspective, analyzing the importance of including it in LCA studies.

Section 2) I would say that there is room for discussion in terms of TACs and quota restrictions. When the landing patterns for the aggregated fish species falls sharply and abruptly in the 1992-1996 period, it seems quite probable that there are also factors linked to the CFP, especially taking into account that in 1992 the CFP was revised, that may influence the results. I think this issue deserves some further analysis in order to understand whether policy-making is also influencing the results that are being obtained, and therefore, becoming a further source of uncertainty.

Section 3) It is an interesting perspective that is offered in this section, since it highlights the need to shift from a target stock perspective to a marine biodiversity approach when dealing with fisheries management. However, Section 1 of this deliverable still maintains a highly target stock oriented assessment. Therefore, I would suggest the authors take advantage of this section to analyze also how the methodology in Section 1 could be improved in the future.

Suggestions for work package – or task leader:

Environmental relevance

Irrelevant				Relevant
				X

Additional comments

The environmental relevance of all three sections is very high, since these elements all add up in creating a more integrated approach for future LCA studies. It remains as a future perspective to evaluate from a methodological perspective which would be the weighting that fishery specific impacts should have in life cycle thinking and how to balance the focus on a specific stock assessment and the influence on marine biodiversity.

Suggestions for work package – or task leader:

Scientific robustness & Certainty

Weak				Robust
			X	

Additional comments

Section 1) The scientific robustness of the overfishing impact category is certainly limited by the unit selection to report the results. The use of a mass unit does not reflect the real effort that the ecosystem has performed to support a specific stock. Therefore, the direct comparability between species when applying this impact category is very limited. For instance, it is not the same to overfish 50 kg of European pilchard, with a trophic level of *circa* 3, as to overfish 50 kg of hake (trophic level *circa* 4.5).

Therefore, the current development of the impact category is very aimed to enhancing policy support, since it would provide policymakers with a suitable tool to integrate stock assessment with other environmental issues (e.g. climate change, acidification, etc), but always from an individual species perspective, which has been the perspective used by the EU to date.

However, the development of this impact category towards an ecosystem effort perspective, through the biotic resource use, or other available approaches, such as emergy, will allow an integrated assessment of multi-species fisheries and the direct comparison between species.

Nevertheless, I would like to point out that I have already had the chance to briefly discuss this particular concern with Andreas at the conference last month in St. Malo. I perceived that the authors are well aware of the current limitations of the method they propose, but also of the wide potential it may develop in the future.

Section 2) Issues such as illegal fishing (IUU), the influence of policy-making (TACs and quota restrictions) are also factors to be taken into account when calculating the MTL and PPR based on catch. The authors have made a discussion on how discards may also affect the results, but it would be interesting to include these two aspects as well, despite the limitations they infer, since IUU are very difficult to quantify and detect.

It would be interesting to know also if any changes in the spawning or depth patterns of the different species have been detected throughout the analysed timeframe, in order to know if these patterns may also be a source of uncertainty. Section 3) As mentioned earlier, the main constraint that I detected in Section 3 is the lack of precision relating to some specific concepts, such as discards, by-catch or incidental catch.
Suggestions for work package – or task leader:

Documentation & Reproducibility

Weak				Robust

Additional comments

Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable
				X

Additional comments

Section 1) I think that the use of the impact category for overfishing will provide very useful information in terms of policy-making. The use of LCA in fisheries assessment had been limited to eco-labeling, business support or academic research, but with the inclusion of stock assessment issues its appropriateness in terms of policy support is enhanced.
The method will be very useful for TAC and quota appointments by the European Union (EU).
Additionally, it may pose an interesting point of junction in terms of economic, social and environmental sustainability, and may be an important starting point for the development of LCSA in seafood production systems.

Section 2) The applicability of this methodology, under my opinion, is going to be constrained by the availability and quality of data for interdecadal timeframes in the different analyzed fisheries. However, this study proves, in the same way as previous studies, that adequate datasets can be aggregated, not only at a worldwide scale, in line with publications such as Pauly et al. (1998), but also at a regional or fishery level. Nevertheless, recent studies have shown that different trends in MTL can show different states of exploitation of a specific fishery, especially in those cases in which the fishery is dominated by one or a cluster of resembling species in terms of MTL.
Finally, the inclusion of scenarios in which to model the effect of IUU or discards would be of added value to the project.

Section 3) Unfortunately, the applicability of Section 3 is highly constrained by the amount of discard inspections that are taken into account and on the specific studies that have been elaborated for endangered species. However, studies like this one provide useful information in order to minimize in future studies the effects of lack of data quality or availability.

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

Section 2) I think a slight discussion on these three mechanisms that have been identified regarding "fishing down the marine web" should be analyzed: fishing down, fishing through, and increase to overfishing. These scenarios correspond to a specific chronosequence of fishery evolution over time. An interesting article has recently been published analyzing the different patterns:

Foley, CMR, 2013. Management implications of fishing up, down, or through the marine food web. Marine Policy, 37, 176-182.

Section 3) Paragraph 5 of the Discussion section:
Please note that Vázquez-Rowe, Moreira & Feijoo, 2012, recommend the use of a BRU approach to attribute impacts from discards. The use of a mass based perspective, which, as you mention in this paragraph, may lead to misinterpretations, is only suggested as the approach to be taken whenever data regarding the composition of the discards is not available.

I would like to point out that according to the FAO definition of discards, these "are not a subset of by-catch as the target species is often discarded". Therefore, I would appreciate a clear differentiation between the *discards* concept and the *by-catch* concept, since they are frequently overlapped in literature studies.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D.1.5
Date	2012.10.21
Reviewer	Bo Weidema
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Summary of the review and key issues

Completeness of scope

Incomplete				Complete
		x		

Relatively to the topic of abiotic resources the methodology developed aims at estimating the future costs related to current resource dissipation using the marginal cost increase of extraction as basis. As this is applicable to all abiotic resources, the methodological scope is relatively complete. However, two issues are missing:

1) The marginal cost increase in the resource rent, i.e. the part of the resource cost not related to extraction costs but related to pure scarcity in an economic sense, cf. Hotelling's rule.

2) Any additional direct impact on human health or nature from the reduction in availability of abiotic resources that are critical to these endpoints, such as a possible reduction in availability of food for the poor, resulting from a decrease in phosphorous availability.

The scope is outlined in Figure 2.1. However, this Figure lacks a clarification of the differnece between current activities and future consequences. Also, metal extraction should be both inside AND outside the dotted box. The arrow from metal demand to secondary metal production should be reversed, since secondary metal production currently is shown as a dead-end consequence rather than reflecting the causal influence that it has on the demand for extraction.

Suggestions for work package – or task leader:
Point out the limitation in scope, unless this is addressed elsewhere.
Change Figure 2.1 to reflect the actual causality.

Environmental relevance

Irrelevant				Relevant
			x	

The endpoint is highly relevant relative the impact category that it is intended to cover. The impact category as such is of low relevance relative to other impact categories.

Suggestions for work package – or task leader:
None

Scientific robustness & Certainty

Weak				Robust

Terminology
Although it is clear from the draft deliverable that the object of concern is resource dissipation, i.e. the loss of resources from the economy resulting in a net extraction requirement after taking into account recycling, the term "resource use" is used for this, without a clear definition what is meant by this.

Inconsistency of the chosen midpoint
While Chapter 2 and 3 clearly states that "there is no constant mid- to endpoint factor that can be applied to all metals" it is anyway proposed to use the midpoint of "% ore grade decrease per kg material extracted". The choice of this midpoint is unjustified, since the whole purpose of a midpoint impact category is to represent an environmentally relevant impact characteristic that is **additive** for the individually characterised elementary exchanges. There is no justification provided that the change in ore grade is additive across metals, which is also the reason that the midpoint is impossible to relate to the endpoint. Rather than the relative change in ore grade, it seems that the **absolute** change in ore extracted per kg metal would be a more relevant and additive midpoint, or even more generally: The absolute change in extraction effort, which of course would bring the midpoint closer to the endpoint.

Use of average mines instead of marginal
The calculation of the characterization factors etc. are made for all three deposit types (porphyry, sediment-hosted and volcanogenic massive sulfide) although only one of these deposit types is the marginal one (porphyry), and therefore relevant in this context. Since the

calculated values are summed in proportion to the output, the porphyry deposits contribute with 80% to the average, so the error made here is limited, but conceptually this is a major flaw.

Unjustified use of average modeling

In addition to the marginal modeling, which is normally applied in LCIA, the use of an average approach is also shown, with the argumentation that "The advantage of the average approach is that it adopts a long-term perspective, focusing on what society ultimately wants to avoid from a resource extraction point of view" referring to Huijbregts et al. (2011). It is not good scientific practice to make such a reference without pointing out that this viewpoint was challenged in the same journal (by the current reviewer) and that the criticism has not been countered with appropriate scientific arguments. Besides its lack of scientific justification, the main problem with the average approach – in the specific context of abiotic resources – is that it would induce a behavior already now identical to the result if all resources were close to exhaustion. Such a behavior would of course be highly irrational.

Incompleteness of cost items for cost increase

The calculation of the surplus costs include only cost increases in mining and milling, i.e. not in beneficiation. It seems that it would be more correct to include cost increases of all activities until the pure metal.

Introduction of discount rates directly in the LCIA modeling

Discount rates of 3% and 15% are introduced for the calculation of the future surplus costs (which effectively means that it is not the future costs that are calculated but the net present value of the future costs). Introduction of a discount rate would be equally relevant for all other impact categories where the impacts do not occur instantaneously (e.g. global warming, acidification, eutrophication, etc.) as well as for economic costs of activities in the inventory that are placed in the future (e.g. the use phase and waste handling of long-lived products), so introducing this only for resource depletion is not justified. If done for one impact category, discounting should therefore be done consistently for all other impacts as well that do not occur at the present time. Introducing discounting in the calculation here appears to be done exclusively to avoid setting a fixed time horizon (since discounting implicitly introduces a time horizon after which impacts become insignificant). This appears to be related to another problem of the method, namely the inadequate accounting for future substitution between resources and technologies (see below). If technological substitution was adequately included in the methodology, this would provide a time horizon at which the specific resource was no longer extracted with the current technology and therefore discounting would not be needed for this purpose. It may also be interesting to note that without discounting, the accumulated marginal cost increase is equal to the absolute cost difference between the current costs and the costs at the point of introducing the ultimately substituting back-up technology, where the supply enters steady state, as pointed out by Stewart & Weidema (2005), which would therefore provide a simplification option for the calculation.

Scenario descriptions

The description (p. 27) of the hierarchist position as a 'business-as-usual' scenario is not in line with the description of this position in the original works by Hofstede and Hofstatter. The hierarchist position is rather a regulation-intensive position with substantial global cooperation.

It may be relevant to mention here the description of the three positions in the context of inventory scenario development in Chapter 6.9 (Table 6.3) in B P Weidema (2003): Market information in life cycle assessment. Copenhagen: Danish Environmental Protection Agency. (Environmental Project no. 863).

Inadequate inclusion of substitution and technology development

While substitution and technology development is included in the scope and method (as described e.g. in Figure 2.1), it is unclear how this in practice is included in the data basis and calculations. There is only a reference to Kapur (2005) that this has been included, but how the future copper demand is influenced by substitution (relative to a(n egalitarian) scenario of no substitution) is not made explicit. In chapter 3 the future fuel demand is modelled on the basis of the IPCC scenario groups, again without explicit mentioning of the role of development of renewable energy sources in these scenarios. The inclusión of substitution and technology development thereby becomes more of a postúlate than a documented causal factor. A more explicit inclusión of substitution would enable a simplified modelling without the need for premature inclusión of discounting.

Suggestions for work package – or task leader:

Improve the terminology (“use” vs. “dissipation”).

Remove the description of the midpoint, which is unjustified , irrelevant, and inconsistent.

Use data for marginal mines rather than average mines.

Include cost increases of all activities until the pure metal.

Include susbstitution and technology development more explicitly in the methodology and calculations, and thereby avoid inclusion of discounting directly in the characterization. Improve the description of the scenarios accordingly.

If you want to include the description of the average modeling, make the weaknesses and inherent value choices of this approach more explicit.

Documentation & Reproducibility

Weak				Robust
			x	

The justification for introducing regional modeling (on p. 27) is unclear, since metals are traded on a global scale. It is also unclear whether it is raw metal (less relevant) or final metal demand (more relevant) that is modeled here.
Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable

No further comments
Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

On p. 13 (and p. 30), the ultimate reserve of copper is given as 1300 Gton, while in Table 1.1, the total is approx. 300 Gton. One of these values must be wrong?

The last paragraph on p. 18 seems to be out of line with the data and description given in the immediately preceeding paragraph.

On p. 18, the marginal CF is given as $3 \cdot 10^{-12}$ %.kg^{-1}, which is then explained as $3 \cdot 10^{-6}$ % per 10^{6} kilogram (kiloton). Although this is correct, the relevance of this information is questionalble, especially as it is followed by the statement that “The current annual copper production of 15 megatons results in a decrease in copper grade of approximately 0.01%”, where one would expect 15 megatons ($15*10^{9}$ kg) to result in an ore grade decrease of $15*10^{9}$ kg * $3 \cdot 10^{-12}$ % = $45*10^{-3}$ % = 0.0045%...

On p. 20, the double negation “less irrelevant” is used. From the context it appears that the intended meaning is “less relevant”

On p. 25, in the last paragraph, line 2, there is a “than” which probably should be “then” and in line 3 an “explored” should probably be “exploited”.

In equation 2.1, the Cost item could better be expressed as delta-Cost.

The last paragraph on p. 32 stands rather unexplained.

In section 2.3.3 on p. 34, the numbers in the first paragraph is wrong and repeats the text below in the second paragraph (where the numbers are in the correct order).

On p. 51, the words “completely different” in the description of the social costs of carbon does not match the numbers provided.

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D1.5 Abiotic resources
Date	Oct. 19th, 2012
Reviewer	Jo Dewulf
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Contact (phone)	+32 9 264 59 49

Summary of the review and key issues

Chapter 1:

For metals, the authors bring a method based on ore grade decrease as a measure for scarcity of abiotic resources. This is an approach that makes sense. It is a rather simple approach as the reality of abiotic resources and its depletion is a field with a lot of complexities and uncertainties:
- Scarcity/availability of abiotic resources depends on more factors than just ore grade: location, rock type, chemical speciation …
- Scarcity: is spatial differentiation necessary?
- Scarcity not only depends on the physical availability (with factors like ore grade) as such, but is also determined by economic development. In this sense, in times of lower economic development, ore grades of mines extracted have gone up with time temporarily, giving some nuance to the statement at start of section 1.2.2.
- Uncertainties of reserve estimates: largely dependent on metal (cfr. Page 10: 'given no new discoveries')
- Co-mining. Numerous metals are extracted simultaneously. Allocation?

These factors are not fully captured by now: at least these items/limitations should be mentioned.

Second, the methodology is elaborated solely for copper. As such, this is acceptable as a starting point. However, the authors should at least clarify to what extent it is applicable to other metals and where some limitations might pop up, e.g.
- sufficient data available for all metals? E.g. substantial historical data set to see ore grade trends? Reserve estimates available?
- applicable for co-mined metals?

- The characterization factor developed in section 1.2. is a nice piece of scientific work; however the easiness to understand is moderate ("keep it simple"). Some clarification on "the critical state of the environment" (p 12) would be welcome.

Chapter 2

Chapter 2 makes use of a rather classical approach: surplus cost. Whereas the title of chapter 1 makes clear that it targets the characterization of scarcity of abiotic resources, this is not clear for chapter 2 from its title. Does it also characterize scarcity, or rather depletion, availability, demand, economic concerns?

The cause and effect chain is well illustrated in figure 2.1, where primary production depends on the decrease in ore grade and the mining technique. As with chapter 1, authors are fully aware of the trends/decrease of the ore grade, resulting in more ore mined per metal. However, the trends in mining techniques - for sure also a factor determining the (surplus) costs – are not explicitly covered. Should this be done? Is USD2000/10^3 kg ore (section 2.2.3) frozen forever?

In section 2.2.2, charactersation factors are explained. Also here, the easiness to grasp for a broader audience might be questioned. Well appreciated is the inclusion of metals supplied by recycling in the modeling. This is rather new. I have the impression that this may need some further elaboration and maturation:

- Differences in (un)availability for recycling. The recycling depends very much on the rate metals become available from waste. It is well known in the (precious) metal sector that there are applications with short and long life times, the latter resulting in lack of availability for recycling.
- Long term use numbers, up to 2100. To my opinion, this is far too unsure. If you look back to metal use profiles of about 20 years ago, let say 1990, metal use of 2012 could hardly be anticipated just from population and economic numbers (section 2.2.3, p28). The technical evolution hugely affects not only the quantities but especially the kind of (precious) metals as we have unforeseen high tech applications. Illustrative is the EU Critical raw materials document: after just a couple of years, it turns out a revision is already needed.

As with chapter 1, the methodology has been elaborated for copper. Implementation for other metals and concerns raised at chapter 1 need attention at chapter 2 as well.

Chapter 3

As chapter 2, chapter 3 makes use of the surplus cost approach, but now for fossil resources where it is now explicitly mentioned that depletion is characterized. This is a typical and generally widely accepted and mature methodology.
A general question in the model is about the substitution (see Figure 3.1). It is not fully clear what is covered by the authors here. On one hand, I read in section 3.1, p 39: "crude oil, natural gas and coal have very distinct characteristics and uses", indicating that substitution among fossils is not easy: is this correct and is this in conflict with the 'substitution' in figure 3? Alternatively, should I interpret substitution of fossils by other resources: nuclear, renewables, non-conventional fossils? Moreover, as fossils have both energy and material applications: does this substitution include both applications?

Completeness of scope

Incomplete				Complete
			x	

Additional comments

See above: what about completeness for other metals than the copper?

Suggestions for work package – or task leader:

Be clear about limitations for usage of methodology for other metals and about the uncertainties introduced in the (very) long term modeling.

Environmental relevance

Irrelevant				Relevant
			x	

Additional comments

Suggestions for work package – or task leader:

Please specify the differences and positioning in terms of area of protection.

Scientific robustness & Certainty

Weak				Robust
	x			

Additional comments

Given the nature of this field, authors and others have to be aware of the uncertainties. Lack of reliable data for all kind of abiotic resources is a point of attention. Future applications and hence demand of abiotic resources in 2050-2100 are hard to foresee.

Suggestions for work package – or task leader:

Be clear to the user about uncertainties or at least: specify the assumptions (e.g. in terms of recycling)

Documentation & Reproducibility

Weak				Robust
		x		
Additional comments				
Suggestions for work package – or task leader: It would be nice to show to what extent impact depends on the assumptions in the future, e.g. assumptions on reserves, demand and recycling.				

Applicability

Not applicable				Applicable
		x		
Additional comments				
Suggestions for work package – or task leader:				

Specific remarks (please indicate page/line in the document)

Chapter 1:

Figure 1.1: time line?
Table 1.1: where are reserve estimate data coming from?

Chapter 2:

Table 2.2.: did you check this secondary production fraction for 2010? (Validation?)
Figure 2.3: did you calculate the cumulative primary production and cross-check with the reserves?
Section 2.3.3, first line: differentiation between 65.07 and 3.62 is not clear at this stage.
Explanation of equation 2.4: "USD2011/kg2 ore". Ore or metal?

Chapter 3:

Table 3.2: are ranges indeed as high as mentioned, e.g. coal/individualist: 0.0017-0.0068, i.e. a factor of 4?

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D2.2 /R /PU Delivery date 31/08/2012
Date	21 October 2012
Reviewer	Antonio Di Guardo, University of Insubria, Como , Italy
Contact (e-mail)	antonio.diguardo@uninsubria.it
Contact (phone)	+39-031-2386480

Summary of the review and key issues

Note: Not being an expert in metals and in Whole Effluent Toxicity, the evaluation is therefore referred to chapter 4 (and 5 for the relevant appendices)

Completeness of scope

Incomplete				Complete
				X

Additional comments

For what I can tell, the document I reviewed seems to meet all the objectives pf WP2, as listed on the project web page. Not being an expert of LCIA I cannot be more detailed.

Suggestions for work package – or task leader:

If the deliverable itself is going to be published on its own, it could helpful for the reader if the connection of the work of WP2 to the other WPs were summarized at the beginning.

Environmental relevance

Irrelevant				Relevant
			X	

Additional comments

The different chapters seem to reflect in an adequate way the objective within LCIA, as mentioned before. Referring to more general problems of Environmental and Human Risk Assessment the tasks seem to reflect some relevant issues of current research and regulation. To me, as a referee (but it might be entirely due to my somehow different expertise) it is not clear why these subjects [(1) metals on terrestrial ecosystems, 2) whole effluents on freshwater ecosystems, and 3) toxic chemicals on higher predators, 4) direct pesticide exposure via food in human toxicity, 5) influence of spatial variability for chemicals causing ecotoxicity and human toxicity] and not others within the LCIA debate. From the web page this does not seem to be object of other WPs.

Suggestions for work package – or task leader:

My general suggestions would be to describe more specifically (or include in the Deliverable) at the very beginning the reasons why the subject listed above (1 to 5) were selected as projects of the different groups. In other terms why priority was given to those and not others. To make an example, 5 (4.2in the document) deals with spatial variability issues selecting certain models and comparing results. Why those model were chosen? The criteria of the selection to justify the comparative results are not clearly illustrated.

Scientific robustness & Certainty

Weak				Robust
			X	

Additional comments

Results presented in deliverable D2.2 are, in my opinion, generally robust and well outlined. In certain parts, however, there is a little bit of confusion on the general scope and how this is developed. The style is certainly detailed, but not always is the presentation of results complete (as one would expect in a scientific paper) and the reader may find him/herself lost in the presentation, simply because some details are not shown or referred to. I will list separately the comments for the different chapters and subchapters I reviewed.

4.1 This is a very long chapter and results are presented for a number of simulations using different models and different case studies but I had difficulties to entirely grasp the comparisons and evaluate the results, due to the complexity itself and to the lack of some details about the dataset, the equations employed and some underlying assumptions. My first comment regards the data employed in the comparison. No mention (or at least I could not find them) is made about the quality of the geographic data and their resolution. Being a spatial assessment I expected quality of spatial data to be discussed as well. This is important in evaluating the differences in results among models (eg consensus modelling approaches such as USEtox). Secondly, the modelling approach adopted is steady state (e.g. MAPPE). Steady state is a convenient way to run simulations and has advantages in terms of simplicity and rapid results, while dynamic (unsteady state) approaches are generally more demanding in terms or simulation time and complexity. The models adopted being steady state may simply overpredict in emission situations where discharge may not be continuous in time. A third point (but this maybe outside of the LCIA world) is why existing spatially resolved global model were not used, such as those developed by Frank Wania or Matt MacLeod or at ETH? As it appears in the second page of the introduction this may have been dealt with in the first deliverable. Other general points worth mentioning: model features (e.g. MAPPE) are not discussed in the results and some of them can be really important in deriving CFs and or fate in general. As an example, MAPPE (as in Pistocchi et al, 2011) reports than soil depth is 0.3 m, which is a considerably high depth for non agricultural soils and possibly very influential in determining the fate for the more hydrophobic chemicals. Last point, which is not clear to me is the influence of temperature on the spatial variability of results. From the results it does not to seem to have a role (and not only for the lack of the coefficient of the relation between dehradation rate and temperature) and this is somehow surprising.

4.2 This chapter reports unsteady results to describe the uptake of pesticides from crop in the context of human health impacts. While the approch seems robust and innovative, the description of processes is very limited: the chapter is very short and sometimes is difficult to appreciate the model employed, because the mass transport or partitioning equations are not fully described. The reader can refer to published work, but some is not (yet), such as Fantke et al, submitted.

4.3 and 4.4 These two chapters are very short but robust in my opinion. Not being an expert in ecotoxicity (at least for the effect side) I have few comments. The most important is related to 4.3 and refers to the lack of bioavailability influence in soil and its relation to Kp. It should be in my opinion discussed because of its influence on some hydrophobic chemicals.

Suggestions for work package – or task leader:

My suggestions are to address the point specified above.

Documentation & Reproducibility

Weak				Robust
			X	

Additional comments

The quality of the documentation is rather robust. However, as I mentioned before, the connections among the deliverables and the project goals are somehow lacking and their inclusion would improve the quality of the documentation. Also a full description of the methods employed would make the document clearer. Being this an intermediate deliverable the issue could also be not extremely relevant.
A few typos should be corrected, especially in 4.1 (see below)

Suggestions for work package – or task leader:

My suggestions are to address the point specified above.

Applicability

Not applicable				Applicable
				X

Additional comments

Not being an expert of LCIA I cannot really judge, apart from the general coherency.

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

p. 58 l 32 I believe that "primary" should be "particulate" instead.
P59 l29 "varaibility" should be variability
p.60 l10 . I could not find anywhere on the web "Margni et al 2002" and reference of sites were not given
P60 l11 "Margni et al 2003" is not in the reference list.
P62 equat. 4.1.1 . the definition of Kair is rather different from that in Pistocchi 2011, which is cited as source. Why?
P68 equation 4.1.5 the parameter SRi (source receptor relationship for country ith) is not described and its values are shown in figures in 5.2.3 but it is not clear how it is calculated.
P142 l3 and 14 . Some "Error! Reference source not found" are present.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D 2.2, chapter 4
Date	21 Oct 2012
Reviewer	Arno Rein
Contact (e-mail)	arnr@env.dtu.dk
Contact (phone)	+45 4525 1696

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
				x

Additional comments

The scope of the studies is clearly stated, and aims of the model approaches, together with uncertainties and variabilities, are described and discussed in detail.

Suggestions for work package – or task leader:

Environmental relevance

Irrelevant				Relevant
				x

Additional comments
The studies are highly relevant, where one of the important aspects considered is the use and applicability of more simplified model approaches, given the large amount of input parameters (and associated uncertainty) needed for highly detailed models (among others (i) with respect to the consideration of detailed spatial variability vs. archetypes, (ii) inter-correlation of toxicity data and estimations, (iii) linearization of complex dynamic plant uptake models). Based upon the studies, recommendations on the use of different models with respect to accuracy are given.
Suggestions for work package – or task leader:

Scientific robustness & Certainty

Weak				Robust
				X

Additional comments
In general, careful and detailed analyses were done, including the inter-comparison of different model approaches (different degree of complexity) and spatial resolutions, as well as applying a range of different scenarios (such as concerning emission schemes and geographic influence). Detailed sensitivity studies were carried out, on which basis the use and applicability of model approaches, together with possible improvements, is discussed.
Suggestions for work package – or task leader:

Documentation & Reproducibility

Weak				Robust
				X

Additional comments

The documentation is all in all detailed and considered sufficient, reproducibility is ensured.

Suggestions for work package – or task leader:

To this respect, probably a kind of an overview would be helpful (maybe in a table in the introduction) indicating which studies were done and compared, and where in the Supporting Information details can be found.

Applicability

Not applicable				Applicable
				X

Additional comments

The applicability of model approaches is discussed in detail, and recommendations for model application are given, based on a range of comparisons and sensitivity analyses.

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

chapter 4.1:

-) As a more general perspective, interesting could also be to consider plants in the model frame work, such as considering loss from soil by plant uptake and exchange with the atmosphere (the influence of forest cover, e.g., is discussed in some parts of Chapter 4.1)
-) As has been outlined and discussed, for future studies, interesting would also be to further investigate the role of temperature (e.g. in our plant related studies we found an important role of temperature on degradation rates)

p.63, Sentence below Eq. 4.1.1: "advection (Kadv)" is missing here (this is also included in the sum)
p.77, Eq. 4.1.10: I guess in the second term it should read dT50,S instead of dT50,W?
p.77, paragraph before Eq. 4.1.11: "..in the above integral is just unit": actually it is the exponential expression (following the integral) that gets 1 (exp(0) = 1)
p.77, last sentence: I guess it should read "..chemical emitted is" instead of "..chemical emitted in" ?
p.91, end of last paragraph: it should read "Fig. 4.23 and 4.24" (instead of 4.18 and 4.19)
p.97, and also at some other places in the text: it guess it should read "1,1,2,2 tetrachloroethane" instead of "1,1,1,2 tetrachloroethane"
p.122, last paragraph: here it reads "..show a linear dependence": if looking at Fig. 4.1.62b, it seems that there is a linear relationship only in some parts
p.124: please check Eq. 4.1.12 (I guess "1/n" should either be moved before the brackets, or each sum should be multiplied with 1/n?)

chapter 4.2

The content of chapter 4.2 is published in the articles Fantke et al. 2011a (Chemosphere 85, 1639–1647) and Fantke et al. 2011b (Environ. Sci. Technol. 2011, 45, 8842–8849). ***My remarks in the following address points that have been considered and implemented in these papers, but are not considered in the deliverable text*** (I was one of the reviewers of these papers). Moreover these two papers (Fantke et al. 2011a and b) should be cited in Chapter 4.2.

1) In the Introduction, p. 134 it reads "...traditionally considering steady-state conditions": it should be noted that some dynamic plant uptake models have already been published. Especially the very recently published models by Rein et al. (2011) and Legind et al. (2011) seem relevant in the context of the manuscript, and may also be included in the comparison of Table 4.2.1 (Rein et al. 2011 deal with chemical uptake into wheat and present a parameterized 6-compartment model; Legind et al. 2011 include a comparison of modeled data to experimental residue data for uptake into pepper fruit plants).
[done in the introduction of Fantke et al. 2011a and b, and in Tab. 1 in Fantke et al. 2011b]

2) More information on the modeling methodology would be helpful. It should be stated whether a numerical or an analytical approach was applied. How were the equations solved, and which software was used?
[done in Fantke et al. 2011b, p.8844, above Eq. 5]

3) Furthermore, more details are required on how plant growth was treated. Plant growth can have an important influence (compare e.g. Undeman et al. 2009, Rein et al. 2011). As far as I understood, growth was considered by using a time-dependent LAI, but how were other plant parts treated (e.g. fruit and corn mass)? Here it would also be interesting to find more information about how LAI curves were fitted (which function /curve characteristics was assumed? The description in the Supporting Information about this is rather short).
[done in Fantke et al. 2011b, p.8844, Section Crop Characteristics]

4) It should explicitly be stated that the approach is limited to neutral organic compounds
[done in Fantke et al. 2011b, on p. 8848, Section Potential and Limitations, bottom]

chapter 4.4:
p.168, last paragraph: I guess it should "..between 1.0E0 and 2.5E2" instead of "..between 1.0E0 and 2.5E22" ?

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	Aquatic ecotoxicity of whole effluents
Date	20121026
Reviewer	Magnus Breitholtz
Contact (e-mail)	Magnus.breitholtz@itm.su.se
Contact (phone)	+46-70-6002686

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
		x		

Additional comments

It is very difficult to make a judgment on the completeness of this chapter since a main part of the work, due to confidentiality issues, hasn't been published. In that sense, the work is incomplete.

Given the objective to provide freshwater ecotoxicity factors for TOC from emissions of industrial effluents and considering the complexity and diversity of industrial effluents, the scope of this work is truly a challenging task. Although the full analysis would have been official, my gut feeling tells me that since relevant data seem to be lacking, it may be difficult to validate the methodology to judge its potential on a broad scale.

Suggestions for work package – or task leader:

Environmental relevance

Irrelevant				Relevant
		x		

Additional comments

First of all, my expertise is not in LCIA. Hence, I have tried to make substantial efforts in understanding *when*, *why* and *where* this type of analysis is useful and relevant. As I have understood it, the *impact part* of the LCA should provide information about emissions of different parts of a product, ideally ranging from e.g. industrial process activities all the way to the waste phase. However, the methodology seems applicable for use also for more limited parts of a products life cycle, e.g. when studying emissions from cultivation of a crop. In general, the methodology may be useful either for comparing the impact between industries in a holistic way (i.e. taking into consideration chemicals, energy, transportation, etc.), or, as just mentioned, for identifying single factors (e.g. different types of chemicals) within a process. Furthermore, until know, the common approach has been to study each chemical individually, but more recently also mixture toxicity aspects have been incorporated into the methodology. When this is done, the mixture toxicity approach used seems to be according the so-called concentration addition model.

Given the above, but also based on the information provided the chapter, I have some difficulties in understanding which application the proposed new methodology should be used in? Is it to compare the toxicity of TOC between similar industrial processes or to compare to overall impact of a single process, where the chemical impact is just one of many? This lack of clarity also makes it difficult to judge the environmental relevance. So, my question is really:

"When and why is it important to have information about the contribution of the toxicity of the TOC fraction?"

Suggestions for work package – or task leader:

I have not been able to study the full documentation since this has not been sent to me. Perhaps such information is available in other, more general chapters. If not, I find it crucial to expand the background information in this chapter. A good start would be the answer the question given above.

Scientific robustness & Certainty

Weak				Robust
		x		

Additional comments Since the methodology hasn't been validated (or at least that the data is not official at this stage) one can only speculate on the scientific robustness of the methodology. In a sense, it is rather easy to find *special cases* (what if the toxicity from an industrial process is more or less related to one or a few very potent substances) when the methodology (everything from the choice of median HC50toc to chosen basis for calculating the fate factor based on the idea that TOC can be considered as a "single" chemical) may not be applicable. However, as I have understood it, in order to make it possible to perform impact assessments, one needs to be somewhat pragmatic and some of the special cases therefore need to be left out of the calculations. Still, it is rather challenging to consider TOC as a "single" toxicity parameter, both from a scientific and regulatory perspective. I also find it a little bit superficial to base the proposal on the idea that it is scientifically justifiable to "split" the effluent into an organic and an inorganic part and further claim that their joint toxicity can be explained by the concept of concentration addition (CA). A simple experiment to at least give a hint if this is justifiable would be to create a mixture of 5-10 elements and one organic substance and test if the assumption holds. A problem in this context is that elements may interact (e.g. antagonism) and thereby not follow the CA concept. I do not say that this has to be a problem, but it somehow needs to be tested and validated.
Suggestions for work package – or task leader: Although the full analysis is not available, I have some general questions related to LCIA, which may need some further handling in the text: i) is it always (for all types of effluents) relevant to use the median HC50 value given that one species may be much more sensitive than others (e.g. fish is extremely sensitive to ethinylestradiol, whereas as algae and crustaceans are not)?; ii) in traditional risk assessment the most sensitive species forms the basis for calculating the PNEC, whereas is LCIA a SSD-type approach is used: why are different approaches used and could this have negative implications for the implementation of this methodology?;iii) what is the scientific basis for assuming that the toxicity of "all" inorganics and "all" organics follow a concentration addition response pattern?; iv) which elements (metals and other inorganics) should be considered in calculating the EC50toc?

Documentation & Reproducibility

Weak				Robust
			x	

Additional comments

Overall, the manuscript is clear and well-written. The information provided makes it possible to follow how various factors have been calculated and which assumptions have been made. Since a crucial part of this work is lacking, the outline of current version of the manuscript is somewhat halting. According to the title and the introduction, the focus is on aquatic ecotoxicity of whole effluents, but the results & discussion and conclusion sections have a single-minded focus on the fate aspects of the problem. However, I sympathize with the authors that it is impossible - and not fruitful - to discuss potentially important aspects related to EFwtoc factor(s), since there is no analysis in this context to discuss.

Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable
		x		

Additional comments

For already mentioned reasons, it is a little bit frustrating that the work cannot be judged based on a complete data set. In theory, the four fate scenarios outlined in Table 3.9 may be a good start but since the EFwtoc factors and hence a final calculation of CF haven't been provided, I cannot even speculate about the usefulness of the methodology. Also, as mentioned above, I am not fully aware *when*, *where* and *why* the methodology is expected to be applied. Hopefully, the coming discussions in Brussels will help clarify this!

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

A general remark is related to the use of BLM. The authors are hopefully aware that the calibration ranges in these models are not always validated for all types of natural waters. For instance, colleagues at my department have shown that in many parts of Sweden the prevailing freshwater characteristics make it impossible to use the most commonly used BLMs (with either over-or under-estimations of effects values as a result). Although this may not necessarily be a problem in the context of the work of this chapter, I think it is important to raise this issue. I can only imagine the diversity of characteristics that different industrial effluents must have, of which some may not be within the ranges of the models.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	Chapter 4 - Toxicity of Chemical Emissions (so far with a focus on Chapter 4.1 - Spatial differentiation of eco- and human toxicity)
Date	October 22 2012
Reviewer	Matthew MacLeod, Stockholm University, Sweden
Contact (e-mail)	matthew.macleod@itm.su.se
Contact (phone)	+46 8674 7168

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments

Existing practice in LCIA assessments of the impacts of chemicals on ecological and human health often relies on generic assessments that are 'zero dimensional', i.e., they don't account for variability in time or space. One of the general goals of the LC IMPACT project is to assess the spatial variability of chemical impacts that is predicted by models with different spatial resolution, and to develop recommendations for improved practices in LCIA with respect to the treatment of spatial variability in chemical assessment.

Chapter 4.1 describes research that addresses this challenge by performing a sensitivity analysis of a highly spatially-resolved chemical fate and transport model (MAPPE) with the goal of providing information that supports the development of "archetypes" of environmental conditions and emission scenarios that can be used a less-complex model (USETox). My guess is that the motivation for developing the less-complex models is to provide appropriate tools that can be readily applied by LCIA practitioners who are not experts in chemical fate modeling, but this is not clearly articulated in Chapter 4.1 nor in any of the documentation I could find on the LC IMPACT website.

The results described in the chapter present a comprehensive analysis of spatial variability in many aspects of chemical fate in MAPPE, and a table (Table 4.1.14) of suggested archetypes for reducing uncertainty in LCIA assessments caused by spatial variability. However, only one of these archetypes (for freshwater and human exposure) is fully evaluated by comparing the less-complex models with MAPPE.

Suggestions for work package – or task leader:

The motivation for developing the archetypes for the less-complex models is not clearly articulated in the introduction to chapter 4.1.

I am not sure if the full development and evaluation of the entire set of archetypes shown in Table 4.1.14 was actually within the scope of the project, or if the goal was only to demonstrate that archetypes could be developed using the approach of conducting sensitivity analysis of a highly-resolved model. Given that the MAPPE modeling has been done, I would like to see the other archetypes also evaluated in the same way as has been done for the freshwater exposure in Figure 4.1.64.

Environmental relevance

Irrelevant				Relevant
?	?	?	?	?

Additional comments

It is very difficult to evaluate the 'environmental relevance' of the research presented in Chapter 4.1. This is not 'fundamental science', but it shares some of the same attributes. I would call this work 'fundamental method development for LCIA', and it is being carried out at a quite abstract level that attempts to evaluate a proxies for all chemicals, a wide range of emission scenarios, and several possible endpoints. Certainly the quality of LCIA assessments that include chemical impacts can be improved based on the results of this research, but in any particular LCIA assessment the contribution of chemical impacts to the outcome of the assessment could be very small. Therefore it is very difficult for me to evaluate 'environmental relevance'.

Suggestions for work package – or task leader:

Perhaps the question of the environmental relevance of this work can only be answered by accumulating case-studies that evaluate the impact of spatial versus non-spatial treatment of chemical impacts. This is beyond the scope of the current project, but could be recommended as future work.

Scientific robustness & Certainty

Weak				Robust
			X	

Additional comments

The approach taken in this work is to start from a model with a high degree of fidelity to the real environment (MAPPE), use it to identify drivers of spatial variability in model results that are relevant to ecological and human exposure to chemicals, and then to develop archetypes of model parameterizations for USETox that encompass as wide a range of that variability as possible. This is a valid approach to the problem, however, the results from the MAPPE model analysis are generally not very surprising in terms of the 'key parameters' (Table 4.1.14) that are identified as controlling spatial variability.

An alternative approach that would likely be less labor intensive would be to do a systematic analysis of the variability in outputs from USETox when it is parameterized to represent a wide range of different environmental conditions and spatial scales, and try to distill the archetypes from that analysis.

(I suppose that a drawback of such an approach would be that it would be more difficult to place boundaries on the realistic values for the archetypes that are defined.)

Suggestions for work package – or task leader:

Documentation & Reproducibility

Weak				Robust
	X			

Additional comments

I have made a list below of specific questions and corrections that should be considered in revising the document to improve readability and clarity.

Suggestions for work package – or task leader:

As mentioned also below, there are too many typos and odd mistakes in the document for me to compile a complete list or mark all of them clearly by hand. A careful technical edit and proofreading is needed to eliminate all of these problems.

Applicability

Not applicable				Applicable
			X	

Additional comments

I believe that the authors general plan to develop archetypes that encompass a large fraction of spatial variability for use with the USETox model is a viable method to address the issue in LCIA assessments.

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

I found many typos, spelling mistakes and examples of bad grammar in the document; too many to make it feasible to write a list here. I will provide my marked-up paper version of the document to the authors at the meeting in Brussels, but I grew tired of marking all the small errors in the document. A careful proofreading and correcting is needed. Below are only the most important of my 'specific remarks'…

Some data plots (for example, 4.1.33, 4.1.34, 4.1.37) do not have labels on the axis, which makes them very difficult to interpret. Some other figures (for example, 4.1.38 and many of the maps) have extremely small text, which makes them very difficult to read.

The authors should learn what compound modifiers are in English grammar and edit the entire document to employ them correctly and consistently. There are several places in the document were this would improve the readability and clarity considerably.

Page 62: I assume all of these equations are sums of rate constants. They should therefore use a small k as the symbol and not a capital K, which is commonly used for partition coefficients. And, why is k_air included as a component of k_soil in Equation 4.1.2. I can only assume that this is a typo?

Page 65: and throughout the document: The SI symbol for kilometer is km, not Km.

Page 71: It is surprising to me that MAPPE doesn't include a treatment of intermittent rain. What are the implications of this in the comparisons with USETox and the other models? This never seems to be fully discussed.

Page 72: In Figure 4.1.10 the median Ksoil (note the typo in the heading to this chart!) for almost all substances is very close to 0.1 /day. This implies a residence time of all substances in soil of only about 10 days, which seems extremely short for substances like mirex that have low volatility, low solubility, and are very persistent. Despite reading and re-reading the text accompanying this figure, I cannot figure out what process is imposing this 0.1 /day limit on Ksoil. More explanation and discussion is needed here. Is this really realistic?

Page 76, Equation 4.1.10. Check this equation for correctness. In the text the parameter dt50,s is referenced, but it doesn't appear in this equation, which makes me think there is a typo here somewhere.

Page 79: Now the authors use lower-case k to refer to both partition coefficients and rate constants!

Page 79: There is no temperature dependence of any of the chemical fate processes in MAPPE? This is surprising, and it seems that a large driver of spatial differences in partitioning (due to the strong temperature dependence of vapor pressure and Koa) and of degradation is ignored. Does this affect the extrapolation of the results to the archetypes in USETox, which I believe does have adjustments for temperature in partition coefficients and degradation rate constants?

Page 80: I don't understand the distinction between explicitly and implicitly including parameters in the sensitivity analysis. More explanation is needed here.

Page 85: The same idea is repeated directly above and below heading 4.1.3.3.1.

Page 96: Population density is population divided by area. Is this really what the authors mean in this section? I have the impression that they have actually allocated the 100 tons of emission based on the population of each country, not the population density.

Page 99: I don't understand the motivation for calculating an "intake fraction" from the spatially resolved model as the intake in a country divided by emissions only in that country. In such a formulation it is entirely possible that intake fraction could be greater than 1. It would even be infinite in any country that does not have domestic emissions of the chemical in question. The range and values of this type of intake fraction are therefore very difficult for me to interpret.

Page 100: Table 4.1.8: Are these really the sum of intake fractions (as it says in the caption)? I don't think it is valid to add the iFs themselves. In other figures (like Figure 4.1.32) the authors were much more careful to specify that they showed the spatial variability in intakes, and not intake fractions.

Page 122: Water residence time is not equal to the inverse of the advection rate.

Thank you for your input!
The LC Impact Team,
Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	Terrestrial ecotoxicity of metal emissions
Date	October 15, 2012
Reviewer	Dr. Nilima Gandhi Research Associate, University of Toronto
Contact (e-mail)	nilima.gandhi@utoronto.ca
Contact (phone)	1-416-238-4394

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments

- Overall, this report is a very good attempt to put forward the new LCIA method for terrestrial ecotoxicity of metals
- A rigorous method application is showed for Cu and Ni emissions using a wide range of soil types
- Offers new interim terrestrial CFs (CTPs) for Cu and Ni; and since the method is generic it offers a platform to produce new interim terrestrial CFs for other metals if provided with their data for speciation and ecotoxicity models
- The revised method of incorporating bioaccessibility has a high potential to improve the current state of terrestrials CFs not only for metals but also can be extended to organic chemicals
- The report also identifies areas of further improvements in method applications, data gaps and implications to other area of LCA, such as LCI
- Overall the report brings metal LCIA for terrestrial ecosystems up to the speed of current scientific developments and offers grounds to adapt the method for other types of environmental assessments such as Hazard analysis as well as generic and site-specific risk assessments

Suggestions for work package – or task leader:

- Overall, this report is a good, comprehensive document
- Some minor comments, suggestions are given in the specific comments section of this review

Environmental relevance

Irrelevant				Relevant
			X	

Additional comments

- The addition of Bioaccessibility presented here in the revised method of calculating CFs has been highly recommended and has a significant potential to improve the terrestrial assessments of chemical emissions in general; more important for metals but can also be significant for organic chemicals
- Sub-models suggested and used in the method application are scientifically sound and reasonably well-tested; thus increasing the confidence in model results and outcomes for estimating accurate terrestrial movements of metals in natural environments
- The method is generic for most metals and can be readily adapted for other types of environmental assessments such as hazard-, risk and site-specific assessments routinely completed in other jurisdictions

Suggestions for work package – or task leader:

- The method adaptation and possible challenges in doing so for other types of environmental assessments may be highlighted in a short paragraph

Scientific robustness & Certainty

Weak				Robust
			X	

Additional comments

- The method is up to the speed of scientific developments in the area of metal fate, speciation/complexations and ecotoxicity
- The revised method is a reasonable practical approach that offers a good balance of current scientific environmental knowledge for terrestrial media, data needs and their availability for real applications, and computational complexity (both for model developers and LCIA managers)
- These types of models in general are challenging for method evaluation and parameter performance; however use of field applied and well-tested sub-models raise confidence in the overall model outcomes
- A knowledge of scientific uncertainty in method application and its parameter values is necessary to avoid potential pitfalls in environmental assessments; the report can be improved a little in this aspect

Suggestions for work package – or task leader:

- A feel for qualitative understanding of uncertainties may be sufficient to relate the implications of applying the proposed method and model outcomes for natural environmental assessments.
- The report recommends using FIAM in cases when TBLM is not available for metals; however, the document does not discuss the challenges of getting the necessary data for FIAM for other metals

Documentation & Reproducibility

Weak				Robust
			X	

Additional comments

- The report is well organized, to the point with reasonable details and well formatted that takes reader in a logical flow; in short the chapter is near completion for final draft
- The documents provides good background to the field of terrestrial LCIA for metals and method adaptations
- Details of sub-models and data provided in the Supporting Information (SI) are essential for reproducing the model results
- Good discussion of model results that relate well to previous terrestrial as well as aquatic ecotoxicity assessments of metals, mainly Cu and Ni in this case
- Conclusions and future method development and data needs are essential for further research in the field of terrestrial ecotoxicity of metals; not only for LCIA but also for hazard and risk assessments

Suggestions for work package – or task leader:

- Personally I felt that more information on TBLM and FIAM results can be added in SI in order for readers to reproduce model results

Applicability

Not applicable				Applicable
			X	

Additional comments

- In terms of illustrative method application in the report, selection of a good case study of airborne metal emissions with a reasonable walk-through of method application sets a good example for model users and LCIA practitioners
- CFs (CTPs) developed for Cu and Ni are based on nearly comprehensive set of data for speciation and ecotoxicity; a wide range of soil-types selected that covers potential combinations of environmental chemistry parameters that significantly affect metal speciation and Bioaccessibility
- The method can be applied to other metals readily given the necessary data are available
- Although TBLM is not available for many metals, alternatives to TBLM (such as FIAM) may be used to develop interim CFs for those metals
- There may be challenges to find empirical equations (similar to those used in the report) in literature for other metals

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

- Page 13; last sentence of first para needs revision – "At the present state…."
- Page 13; Instead of "chemically active on its way to organisms.." I suggest to write "chemically active and potentially available for biotic uptake.."
- Page 13; "the only dissolved form are free ions." I suggest "the only dissolved form **is** free ions."
- Page 14; for the selection of Kd model I do not understand why first criteria that the empirical equations for Cu and Ni must be developed using the same set of soil types – Comments? Are you suggesting that for other metals this criterion must be considered?
- Page 14: criterion 3: Kd model must be developed for **a** large number of soils – large is a relative number and therefore should be specified in the context of the assessment
- Page 16: "(iii) models predicting chronic toxicity were preffered…" It is not cleared from the report which acute model were included and what is significance of including such model in this case both in terms of model outcome and LCIA practice
- Page 17: Soils section is repeated, redundant and must be removed
- Page 17 and throughout the report; consistently use either Comparative toxicity Potential or CTP
- Page 18: Format the ref (Huijbregts et al. 2001)
- Page 18, 19: Remove extra "Median" from the beginning of the sections 2.3.3 and 2.3.4
- Page 19: The lowest value of BF for Cu and Ni reported later in the section does not match with the range of BFs reported in the beginning of the paragraph
- Page 20: Remove the extra "The" from the beginning of section 2.3.5
- Page 21: Remove the extra "Comparative" from the beginning of section 2.3.6
- Page 21: Instead of "the CTP of Cu is determined", I suggest "the CTP of Cu is calculated.."
- Page 21: Should be (**Tables** 2.8 and 2.9)
- Page 21: "The CTP of Ni is mainly driven by EF" – Show/explain the reader how – it becomes apparent later in the report
- Page 22: Remove the extra "The" from the beginning of section 2.3.7
- Page 22: "with considerable influence of the KNiBL.." could you please specify how? Or quantitatively
- Page 23: first line there is an extra "large" in "large deposition"
- Page 23: second line I recommend "airborne emissions" instead of "a airborne emission"
- Page 23: Third line there is an extra "also" after "considered"
- Page 23: Instead of "eq 2.6" it should be "eq 2.7"
- Page 23: Instead of "across thousand of kilometers", it should be "across thousand**s** of kilometers"
- Page 23: Instead of "located withing 200 km), it should be "located **within** 200km"
- Page 23: Instead of "load in iach subarea", it should be "load in each subarea"
- Page 25: A period is missing after the reference Lavkulich 1997
- Page 25 and throughout the report, please use the consistent format for references
- Page 26: Instead of "TBLM to calculated" use "TBLM to **calculate**"
- Page 26: Instead of "Our results suggest thus" I recommend "Our results thus suggest"
- Page 27: EFs of Cu and NI using FIAM are reported in ug/L; I believe this is HC50 or if this is EF please report in the same unit as before (i.e., m3/kg)
- Page 27: Instead of "Results show that and error" use "Results show that **an** error"
- Page 27: Instead of "potentially serve as sufficient alternatives" use "potentially serve as a sufficient alternative"
- Page 27: in "Indeed, the freely dissolved concentration" I believe you mean "truly" instead of "freely"
- Page 28: last word of first para should be "pore water" not "pore waterwater"

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	Aquatic ecotoxicity of whole effluents
Date	October 15, 2012
Reviewer	Dr. Nilima Gandhi Research Associate, University of Toronto
Contact (e-mail)	nilima.gandhi@utoronto.ca
Contact (phone)	1-416-238-4394

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
	X			

Additional comments

Although there is no written scope of this report, I would assume that it would be based on the presentation of new method to estimate WET, application of the model, results discussion and method evaluation, resulting in a discussion of broad applicability of the method for similar environmental assessments. The report presented here is focused on the first aspect (i.e., presentation of the new method) only and that too is fairly concentrated on the fate modeling. With the acknowledgement that authours have not purposely omitted the subsequent sections, the reality is that the scope of the project still remain largely incomplete. I personally believe the modeling approach presented here is based on the traditional knowledge of organic chemical modeling yet lends a sophisticated aspect that is mathematically reasonable and practically promising. However; there are a number of areas that require significant attention before publishing the method as scientifically sound and acceptable for real applications. Particularly I am more interested in (1) evaluation of the new TOC method – how the model results are compared with the experimental case study analysis as well as with those derived using the single chemical approach; and (2) complete WET results including inorganic chemicals and mainly mixture of metals. There is a need for a huge number of preceding experimental studies that feed the parameters critical for model application in order to implement the method successfully. More importantly the prediction power of the model in a variety of environmental situations must be tested. The goal of this review is not to discourage developments in this direction of research but rather the intention here is to identify gaps (many of them already mentioned by the authours) in both method and its applicability.

Suggestions for work package – or task leader:

I recommend publishing the report only after completing the scope of the entire project and most importantly after the evaluation of the method itself.

Environmental relevance

Irrelevant				Relevant
			X	

Additional comments

The full scope of this work offers huge environmental benefits. Traditionally contamination problems in natural environments have been assessed on a single chemical basis and to date it has proved to be a reasonable approach to track movement and thus potential toxicity of chemicals in the environment. The model developed in this regard are applied and tested rigorously. Attempts to assess the ecotoxicity of a mixture of environmental pollutants are rare since modeling such situation has been viewed as extremely challenging. The modeling approach presented in this document addresses the pressing need of evaluating environmental impacts of chemical mixtures. This is especially important since no chemical exist as a single chemical, as has been traditionally modeled, in any environment. The scope of this work, however, is very optimistic in itself since the attempt here is to assess the ecotoxicity of whole effluents even without knowing its composition. Having said that the method presented here looks very promising and if evaluated successfully provides a huge leap in the field of assessing

Suggestions for work package – or task leader:

Scientific robustness & Certainty

Weak				Robust
		X		

Additional comments

As mentioned earlier, the method presented here stems from the traditional modeling approach of assessing single organic chemical's movement in the environment with a new angle of treating a mixture of organic chemicals as TOC. Mathematically the presented method seems correct and provides reasonable ground for exploring it further. However, one must remember the shortfalls of the approach. To name a few here: (1) the mixture of inorganic chemicals are assessed separately and I assume that would eventually require to find out the composition of the whole effluent (or I request authours to shed some lights on how they would evaluate mixture of inorganics, especially metals and other ionic compounds. (2) A large number of assumptions are involved in applying the method, which if not justified appropriately will drive the method away from reality. A rigorous check on model performance becomes essential. (3) There is no guidance, in the method or within the other parts of the document, on how to treat the compounds that are neither organic nor inorganic, e.g., organo-metallic compounds. (4) It requires a full assessment whether faithfully assess the increased toxicity of mixture when more toxic than parent compound are produced during the decomposition process (e.g., in case of polybrominated diphenyl ethers or PBDEs). (5) The method suggests incorporating Bioavailable Fraction (BF) of chemicals for the purpose of exposure and toxicity but fails to show how to calculate this fraction not just for a mixture of inorganics but also for TOC approach. (6) Extensive experimental data on fate parameters and toxicity studies are needed for each type of effluent for different industries. This is huge task in itself and makes the method less attractive for environmental applications in a short future. (7)Many parameters are highly uncertain and difficult to obtain through experiments. For example, degradation rates in soil and sediments. Although this has been a case for single chemical modeling, it may raises scientific criticisms when chemical mixtures are treated the same way as a single chemical without much knowledge. (8) I would assume whole effluents even from one industry will have day to day variations in its compositions. The report needs to shed the light on how significant these variations would be in terms of assessing WET. Overall, although I believe method seems promising it is still at an immature stage. The initial recommendations would be to address the above points and especially evaluation of the model results to check whether it produces meaningful results to put forward the method for its widespread application.

Suggestions for work package – or task leader:

Documentation & Reproducibility

Weak				Robust
		X		

Additional comments

The document here is well written and presents nicely the background of the problem, task at hand, the new method with details on how to estimate the fate parameter values for model application, and finally the highlights on uncertainty and implications of applying the method for various industries. However, I still feel the document is incomplete since it lacks the details of case studies, the results they obtained especially related to the performance of new modeling approach and some sort of a discussion on limitations on its applications. The reproducibility of this report is none since there are no model results available. Not that authours have hidden this aspect, the review process becomes challenging when only an approach is presented without the data support or results.

Suggestions for work package – or task leader:

I recommend publishing the report only after including the case study results and evaluating the model performance.

Applicability

Not applicable				Applicable
		X		

Additional comments

In terms of method application itself, it would have been more interesting to see the method applied to the presented case study with its results and discussion. For wide-spread application of the presented approach, the model equations are generic and stem from the same traditional approach for single organic chemical, thus making the approach more promising for assessing WET for other industries in general. However, parameterization of most, if not all, fate parameters demands extensive experimental work that may affect the generality dimension and delay the implementation of the method. The suggested experiments are resource (time and money) demanding since the number of different industries and their effluent types add up fairly quickly. I was also wondering whether combining inorganic chemicals in the analysis without the knowledge of their interactions among inorganics as well as with organic chemicals is advisable and reasonable for general applicability of WET studies.

Suggestions for work package – or task leader:

The authours must incorporate their views on the above aspects.

Specific remarks (please indicate page/line in the document)

Specific remarks would be helpful at the completion of this report. I found little spelling/grammer errors. I would love to review the revised document again if possible and would incorporate my detailed comments at that time.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D3.2 (T3.1: Aquatic eutrophication) With respect to Ch 2 Spatially-explicit characterization factors for freshwater eutrophication on a global scale:
Date	19 Oct 2012
Reviewer	Jaap Struijs
Contact (e-mail)	jaap.struijs@rivm.nl
Contact (phone)	+31 (0)30 2742001

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

This study successfully employs the methodology of two recent studies: 1) a method for spatially explicit fate factors for global P emissions by Helmes et al. (2012) and 2) a sensitivity distribution of macrofauna for P in Dutch freshwaters (Struijs et al., 2010a). The extension to other species and global freshwaters is an impressive step forwards in the development of endpoint characterization factors (CF) for LCA. Only a site generic endpoint CF for Europe characterization factor was developed until now although the used model (CARMEN) allowed the derivation of a country specific fate factors. This choice was made because it is recognized that country specific emission data were not always available. It is felt now that country specific CFs, both at the midpoint and endpoint level, are increasingly important to the LCA practitioner as more detailed emission data become available. This project delivers such country-specific CFs.

This chapter requires however, further elaboration of the introduction and/or discussion. This was also indicated by the reviewers of the manuscript "Species richness – phosphorus relationships for lakes and streams worldwide" by Azevedo et al., submitted to Global Ecology and Biogeography.

What I miss is the notion - or at least a discussion - that in tropical freshwaters nitrogen can be the limiting nutrient as well. There is ample literature on this. Generally, a more comprehensive description of the environmental mechanism i for this impact category in LCIA is required. Or according to the associate editor of GE&B: the conceptual framework for the study was not well developed.

Furthermore, the importance of this work requires more wording.

Suggestions for work package – or task leader:
Just accept the invitation by the associate editor to revise the manuscript. The reviewers really appreciated this work.

Environmental relevance

Irrelevant				Relevant
				X

Additional comments

There is no doubt about the high relevance of this work. Anthropogenic input of P causing nutrient enrichment in freshwater is the severest intervention with respect to freshwater quality in the industrial world. It results in ecological as well as economic damage. Moreover, in fast developing countries nutrient enrichment is now an emerging environmental problem. Until now, LCIA methodology was only applicable to temperate Europe. Nowadays ecosystems in tropical regions are under pressure due to their enormous growth of industrial and agricultural production.

Suggestions for work package – or task leader:

Publication of he study in Int. J. of LCA.

Scientific robustness & Certainty

Weak				Robust
			X	

Additional comments

The description of species richness – phosphorus relationships in global freshwaters should be improved for the purpose of publication in GE&B as suggested by the referees and the editor. I fully agree with this, especially in view of their complementary suggestion that the collected data are applicable to other studies. A thorough analysis of the relation between species richness and total P concentrations would make effect/damage factors also better acceptable in ecological risk assessment.

Suggestions for work package – or task leader:

Submit a revised manuscript according in accordance to the comments of the editor of GE&B and prepare two papers on the application of this theoretical framework: 1) for LCIA (Int. J LCA) 2) for ecological risk assessment (f.i. IEAM or ET&C).

Documentation & Reproducibility

Weak				Robust
				X

Additional comments
An impressive and useful dataset was assembled. The information in both the supporting information of the GE&B manuscript and in the LC-IMPACT report looks reliable and complete.

Suggestions for work package – or task leader:
None

Applicability

Not applicable				Applicable
				X

Additional comments
There is no doubt that the results will be adopted in LCA models.

Suggestions for work package – or task leader:
None

Specific remarks (please indicate page/line in the document)

P5, in the middle: The spatial resolution **for the global scale level,** decreases to 0.5 etc
P5 below: two times “different than..” replace with “different from…”
Other editorial items I will give on 6 Nov in Brussels

Other items:

P13, the last 3 lines of section 2.3.1 Effect factors: some words are missing (…consisted were…”)
P 13 middle: “…the temperate region (Fig 2.2)..”. This figure has only colours and lacks CF data.
P 13, 13 lines from below: the next 5 lines are difficult to understand.

P14: one but last line: 0.001 kg m-3?

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact
This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D3.2 (T3.1: Aquatic eutrophication) With respect to Ch 3. Marine eutrophication
Date	19 Oct 2012
Reviewer	Jaap Struijs
Contact (e-mail)	jaap.struijs@rivm.nl
Contact (phone)	+31 (0)30 2742001

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments
Chapter 3 describes an interesting attempt to formulate characterization factors (CF) for coastal waters at damage level. The global N fate model is based on recent developments: the Integrated Model to Assess Global Environment, IMAGE 2.4 (Bouwman et al., 2009) and achievements by Wolheim (2006, 2008). These results are applied to compute the export of N to distinguished coastal marine eco-regions from agricultural inputs, atmospheric deposition and emission from sewage water. Also new is the description of ecological effects of nitrogen in marine waters with the focus on the fate of excessive phytoplankton biomass as it is degraded in bottom waters resulting in dissolved oxygen depletion and hypoxia.
The Introduction contains a clear and well-organized survey on the environmental mechanism of this complex impact category. A complete overview of the literature is given, followed by a comprehensive description of all relevant aspects such as the question of nutrient or organic matter enrichment, primary production in marine environments, the role of organic loading in addition to nutrient enrichment.
The model includes an LME specific fate analysis for N (FF) and country-specific FFs are given in the Supplementary Information. Furthermore, an attempt is made to formulate an exposure factor (XF). The analysis of XF is based on a marginal change in the N concentration in the photic zone that induces a marginal change in the dissolved oxygen concentration in the benthic habitat.
With respect to paragraph 3.2.5 Exposure Factors (XF): Fick's 2nd law equation is used to calculate ΔDO in the benthic layer. More information a the derivation of XF is necessary. Or is Fick's law not

used? If so, remove the equation. According to P 93: “The estimated oxygen concentration in the bottom layer is obtained by integrating the DO inputs and DO losses over time. *This model is still under development*.” Nevertheless, the Supplementary Information to Ch 3 contains a table of XFs. Without a paragraph explaining how ΔDO in the benthic layer is evaluated, it is impossible to assess this part of the methodology. In the insert of p 93 (heterotrophic bacteria respiration) it is assumed that OM is fully mineralized. The value for the O2/OM ratio is calculated as 1.24, which reflects 100 % oxidation (ThOD). Bacterial grow on a substrate however, results usually in an O2 consumption of less than 100 % because part of the OM is converted into bacterial biomass. As a consequence, the OD is around 0.6 ThOD, depending on the substrate. The formed bacterial biomass will be part of the sediment. The paragraph Hypoxia (p 78, 79) although generally well written, is incomplete. Ecotoxicological parameters are introduced, f.i. abbreviated as LC50 (p 79). A phrase like “…crustaceans show the highest sensitivity to lower oxygen saturation rates (with high LC50 results…” probably means that at a relatively high O2 concentration (f.i. 4 mg/L?) already adverse effects are observed (in analogy to low EC50 values for toxic substances relatively low concentrations cause effect). But what is “LT50” and “SLC50” (not in the list of abbreviations)? This part requires more clarification. Does the paper by Vaquer-Sunyer & Duarte (2008) contain lognormal distributions of experimentally obtained LC50 values (on a negative log O2 axis)? Paragraph 3.2.3 is not clear to me, probably due to missing numbers in Figure 3.3 (Global input and fate of anthr. N). Furthermore, reference is made to Fig. 2 (8 lines from below p 85)?
Suggestions for work package – or task leader: The introduction, section 3.1.7 paragraph Hypoxia (p 78 and 79) needs an illustrative figure for clarification, f.i. SSD plot of benthic species versus negative log[O2]. Improve paragraph 3.2.3 (The fate model), especially Fig 3.3 and 3.4. Give a more understandable description of how XF is derived. Rewrite the paragraph Impacts of hypoxia on biota (p 79) and ad abbreviations to the list on page 2. On page 93/94 (EF) a clearer description of the distribution of sensitivities should be given.

Environmental relevance

Irrelevant				Relevant
				X

Additional comments

This work has a high relevance as the biogeochemical cycle of N has been tremendously changed by mankind. One of the consequences is that N causes nutrient enrichment in coastal waters which results in ecological damage to marine waters as well as economic damage in the industrial world (tourist industry). Moreover, in fast developing economic regions nutrient enrichment is now recognized as an emerging environmental problem. Until now methods only provided fate factors as midpoint CFs (and only for the European continent). Effect and damage factors were not available until now.

Suggestions for work package – or task leader:

None

Scientific robustness & Certainty

Weak				Robust
		X		

Additional comments

Because the description of the model is still incomplete, it is not possible to judge the scientific robustness and certainty.

Suggestions for work package – or task leader:

None

Documentation & Reproducibility

Weak				Robust
		X		

Additional comments
See my comments in **Scientific robustness & Certainty**

Suggestions for work package – or task leader:
None

Applicability

Not applicable				Applicable
				X

Additional comments
The results of this project will be a major contribution to current LCIA systems. The is no doubt that it will lead to characterization factors which were lacking for a long time. LCIA model developers were unable to deal with anthropogenic N emission at the ecological damage level. N is an immense intervention on a global scale and it has several negative impacts (acidification, global warming etc.) but has also beneficial aspects (food production). It is therefore hard to evaluate and it would be a great achievement if adverse effects such as ecological effects on the marine environment could be quantified and so separated from other impacts of N input. Region specific CFs (both midpoints and endpoint) for the impact category marine eutrophication would be a great step forward, not only for LCA but also for ongoing N assessment studies.

Suggestions for work package – or task leader: None

Specific remarks (please indicate page/line in the document)

Will be transferred on 6 Nov in Brussels.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D3.2 (T3.1: Aquatic eutrophication) Spatially-explicit characterization factors for freshwater eutrophication on a global scale
Date	29.10.2012
Reviewer	Prof. Jyri Seppälä
Contact (e-mail)	jyri.seppala@ymparisto.fi
Contact (phone)	+358407401708

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments

The recommended methods cover both midpoint and endpoint approaches in lakes and rivers, and they can be applied worldwide. Characterization factors (CFs) have been produced for countries and continents. These features are a clear improvement on earlier methods. However, further analysis and discussion of the reliability and applicability of the methodology is required in the next steps in order to make final conclusions.

Suggestions for work package – or task leader:

There is a need to illustrate how the results of the new methodology differ from the results calculated using previous methods

Environmental relevance

Irrelevant				Relevant
		X		

Additional comments

Environmental relevance here is assessed in terms of the scope that the methodology should describe differences in potential freshwater eutrophication impacts caused by the same emission of total phosphorus (TP) in different parts of the world. The starting point is very challenging.

The chosen endpoint seems to be relevant for streams, but in lakes oxygen depletion also plays a role in the cause-effect relationships.

There are many aspects which reduce environmental relevance. For this reason, it is difficult to give an overall evaluation of environmental relevance. For example,

- in the methods there is only one relevant stressor (phosphorous (P)) causing freshwater eutrophication. This is an appropriate simplification although there are freshwater areas in which N also plays a role in aquatic eutrophication (see Applicability)
- In principle, important processes were taken into account in the determination of fate factors. However, the morphology of lake and river basins and the recycling of P from internal stores in the sediments are missing (but the omission is understandable in this kind of application).
- CFs have been produced for countries and continents. However, due to the local and regional character of effects the resolutions based on countries and continents are very imprecise for describing the impacts of eutrophication..
- a selected endpoint indicator based on the relationships between the potentially not occurring fraction (PNOF) of autotrophs and heterotrophs organisms and total P can be considered an acceptable indicator, but it is difficult to evaluate if it is better than, for example, the influence of P on net productivity (for which worldwide data is available). See also the next comment.
- the endpoint indicators are adjusted for four different region types in the world. However, within each region different trophic types of lakes exist where the sensitivity to P varies significantly even though their TP level is currently the same. This is, for example, due to the morphology of lake and river basins and the recycling of P from internal stores in the sediments.
- freshwater eutrophication produces biomasses that will decompose in coastal areas. There is a link to marine eutrophication.

Suggestions for work package – or task leader:

- The text does not currently include an introduction in which the cause-effect chain of freshwater eutrophication has been described. It would be good if alternative endpoints could be presented at the beginning of the article as this would help to understand the relevance of the endpoint selection in this study.

-The reliability of CFs could be checked by showing the results to experts in limnology and asking their opinions about the results between different countries

Scientific robustness & Certainty

Weak				Robust
		x		

Additional comments

It is unclear how the recommended methodology can really produce damage oriented grid/country specific CFs. Damage oriented CFs means here that they can take existing eutrophication levels in different freshwater areas into account in the assessments. In reality, the concentration of TP in each grid consists of TP loads, advection (water volume, flows), P retention and other things. Maybe a more exact determination approach for grid specific CFs should be conducted by varying TP emissions in each grid. For this reason the characterization model should describe the relationship between the total emissions and concentrations of TP in grids. At present the philosophy used in the determination of CFs differs from the methodology used in acidification. On the other hand, I understand the philosophy behind the current assessment methodology and maybe the marginal change EF is the most consistent with the fate model used. However, the results of the recommended method describe potential effects without a connection to the real situation in the grids.

There are also some other weaknesses which reduce scientific robustness and certainty. For example, questions arise regarding the appropriate resolution for describing freshwater eutrophication in order to rate potential differences between geographical areas (see also Environmental relevance). For example, China is as big as tens or even hundreds of countries, but in characterization it is considered as one country.

Water quality data measurements for deriving PNOFs are missing in many areas or the samples used are limited. Limited data for TP concentrations also cause uncertainty in Europe.

The article does not attempt to describe the uncertainty of parameters in the final results. On the other hand, the model uncertainty can be estimated by using three different characterization factors in the cases of European applications. However, it is unclear what is the meaning of this uncertainty in relation to the uncertainty of the whole model.

There is question mark over the reliability of results outside Europe because their CFs are based on linear effect factors without the environmental concentration of TP (and the emissions of TP).

Suggestions for work package – or task leader:

There is a need to clarify the scientific bases of determination of CFs when the aim is to move towards more damage oriented CFs.

It is worth highlighting the meaning of the resolution by using grid based calculations in different parts of a selected country and then comparing the results to those calculated using the corresponding country specific CFs.

Documentation & Reproducibility

Weak				Robust
			X	

Additional comments

The work is well written and documented. For experts, it is easy to read and follow the idea.

Suggestions for work package – or task leader:

It is good if you take the findings outlined in this evaluation into account in the final version.

Applicability

Not applicable				Applicable
		X		

Additional comments

The paper proposes one midpoint method and three endpoint methods for two organisms (autotrophs and heterotrophs) and for two freshwater types (lakes and streams) in European countries. Although CFs based on linear effect factors can be considered as the worst approach in Europe, the situation is still quite complicated for LCA practitioners. Alternative results can be produced but can they be prioritized in order to draw final conclusions?

Assuming a situation in which there is a lake (L) with both upstream (U) and downstream (D) processes connected to L. An LCA practitioner knows that P emissions are released into U. First he indicator results can be calculated by the country specific CFs of U. Does he also calculate the lake based results be calculated using the same TP emission? Or are the calculations based on U enough for the final interpretation?

Could the indicator results of autotrophs in lakes and streams be added together in the final interpretation?

A fundamental question arises regarding how to give the right message about the limitations of the methods to LCA practitioners. Non-experts might at present believe that emissions of P released in different parts of the world can be ranked with this methodology. However, at what point can differences between results show with certainty that the X emission of P in country A is more harmful than the X emission of P in country B?

Suggestions for work package – or task leader:

In the next step, there is a need to clarify good practices: how to use the recommended CFs and how to interpret their results?

Specific remarks (please indicate page/line in the document)

The terms “autotrophs” and “heterotrophs” should be explained when they are first time mentioned in the text on page 9.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D3.2 (T3.1: Aquatic eutrophication) Spatially-explicit characterization factors for marine eutrophication on a global scale
Date	29.10.2012
Reviewer	Prof. Jyri Seppälä
Contact (e-mail)	jyri.seppala@ymparisto.fi
Contact (phone)	+358407401708

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments

The recommended method covers an endpoint approach for marine waters and it can be applied worldwide. CFs have been produced for countries and continents. These features are a clear improvement on earlier methods. However, further analysis and discussion of the reliability and applicability of the methodology is required in the next steps in order to make final conclusions.

Suggestions for work package – or task leader:

There is a need to illustrate how the results of the new methodology differ from the results calculated using previous methods

Environmental relevance

Irrelevant				Relevant
		X		

Additional comments

Environmental relevance here is assessed in terms of the scope that the methodology should describe differences in potential marine eutrophication impacts caused by the same N emission in different parts of the world.

It can be said that the chosen endpoint is relevant for marine eutrophication.

However, there are some aspects which reduce environmental relevance. For this reason, it is difficult to give an overall evaluation of environmental relevance. For example,

- in the method there is only one relevant stressor (nitrogen (N)) causing marine eutrophication. However, it is well known that estuarine systems can be limited by both nitrogen and phosphorus. For example, the Gulf of Mexico (one of the most serious dead zone in the world) and the Baltic Sea are limited by both nutrients.
- all depositions of nitrogen from air emissions to marine water are taken into account although the marine eutrophication concentrates on coastal marine systems. The solution overestimates the contribution of air emissions to marine eutrophication.
- indirect effects of biomass caused by P in freshwater systems are missing (note the link to freshwater eutrophication)

The endpoint indicator is linked to 64 large marine systems in the world. However, under each region there exist different coastal zones where eutrophication problems vary largely. For example, in Mediterranean there are tens quite restricted regions where eutrophication plays very important role, whereas the rest areas of Mediterranean do not suffer marine eutrophication. According to my understanding, the methodology cannot take this variation into account.

Suggestions for work package – or task leader:

- there is a need to rethink how P should be taken into account
- make recalculations for air based N in order to omit N deposition to open ocean areas far away from coasts
- the reliability of CFs could be checked by showing the results to experts in marine eutrophication and asking their opinions about the results between different countries

Scientific robustness & Certainty

Weak				Robust
		x		

Additional comments

It is unclear how the recommended methodology can really produce damage oriented grid/country specific CFs. Of course, a question is how we define "damage oriented CFs". My interpretation is that damage oriented CFs can take existing eutrophication levels in different marine areas into account in assessments. In reality, the concentration of N in each grid consists of N loads, advection, denitrification, sedimentation and other things. Maybe a more exact determination for grid specific CFs should be conducted by varying N emissions in each grid. For this reason, the modet used for the determination of CFs should describe the relationship between the total emissions and concentrations of N in grids but now according to my understanding, CFs were determined without the use of total emissions into each grid. At present the philosophy used in the determination of CFs differs from the methodology used in acidification. On the other hand, the philosophy behind the current assessment methodology is understandable and the chosen marginal change EF is consistent with the fate model used. However, the results of the recommended method describe potential effects without the connection to the real situation in the grids.

The method includes several assumptions. Their bases are clearly presented but their validity is difficult to proof.

In Section 3 many important limitations are presented. However, a following question needs more attention: what is an appropriate resolution for describing marine eutrophication in order to rate potential differences between geographical areas. For example, Mediterranean includes different areas suffering eutrophication in the different way, but in characterization it is considered as one area (see also Environmental relevance).

The article does not attempt to describe the uncertainty of parameters for final results and the whole model uncertainty stays unclear.

Suggestions for work package – or task leader:

There is a need to clarify the scientific bases of determination of CFs when the aim is to move towards more damage oriented CFs.

It is worth highlighting the meaning of the resolution by using grid based calculations in different parts of a selected country and then comparing the results to those calculated using the corresponding country specific CFs.

Documentation & Reproducibility

Weak				Robust
			X	

Additional comments The work is well written and documented. For experts, it is easy to read and follow the idea.
Suggestions for work package – or task leader: Please, take the findings outlined in this evaluation into account and fulfill your text as you have written in the manuscript.

Applicability

Not applicable				Applicable
			X	

Additional comments The methodology offers one indicator result that is easy to understand and country specific CFs are not difficult to apply. Thus, the use of the method is very easy for LCA practitioners. On the other hand, a fundamental question arises regarding how to give the right message about the limitations of the methods to LCA practitioners. Non-experts might at present believe that emissions of P released in different parts of the world can be ranked with this methodology. They do not understand that the method cannot take very well existing state of eutrophication in different areas into account.
Suggestions for work package – or task leader: In the next step, there is a need to clarify the interpretation of results produced by this method. In addition, missing P and its relevancy for some areas should introduce in the understandable way.

Specific remarks (please indicate page/line in the document)

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D3.3 (Terrestrial acidification) Spatially-explicit midpoint and endpoint indicators at the global scale for terrestrial acidification
Date	29.10.2012
Reviewer	Prof. Jyri Seppälä
Contact (e-mail)	jyri.seppala@ymparisto.fi
Contact (phone)	+358407401708

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
			X	

Additional comments

The recommended methods cover both midpoint and endpoint approaches and they can be applied worldwide. Characterization factors (CFs) have been produced for countries and continents These features are a clear improvement on earlier methods. However, further analysis and discussion of the reliability and applicability of the methodology is required in the next steps in order to make final conclusions.

Suggestions for work package – or task leader:

There is a need to illustrate how the results of the new methodology differ from the results calculated using previous methods, especially in Europe.

Environmental relevance

Irrelevant				Relevant
		X	X	

Additional comments

Environmental relevance here is assessed in terms of the scope that the methodology should describe differences in potential terrestrial acidification impacts caused by the same acidifying emissions indifferent parts of the world.

The emissions used in the approaches (NOx, NH3 and SO2) are the most relevant emissions in terrestrial acidification. There are some other emissions such as HCl that can play an important role in certain industrial processes, but their omission is understandable.

The atmospheric fate modeling with the help of GEOS-Chem gives an appropriate basis for the determination of global fate factors.

In the case of the midpoint approaches, the authors have not presented clearly why the recommended method (Type 1) is better than Type 2, although the use of critical loads in the approach of Type 2 has a long history in the scientific community dealing with acidification. It is difficult to say that exposure modeling with the soil fate model (PROFILE) used gives better starting points for environmental relevance than the critical load approach does.

The determination basis for the CFs of Type 2 is unclear. It seems that the calculation rule does not correspond to the accumulated exceedance (AE) method. CFs were derived without changing emissions in grids. The weakness in the characterization method of Type 2 is maybe the reason why at present Type 1 is better than Type 2. However, if the authors will make the recalculations using the new modified characterization model based on AE, the situation can be another.

The midpoint approach of Type 1 offers a basis for the endpoint approach of Type 3 but this does not mean that Type 1 is better than Type 2 in terms of environmental relevance.

The chosen effect factor in the endpoint approach seems to be relevant for terrestrial eutrophication in many areas but it is somehow unclear how the framework of EFs works on a global scale. In addition, the endpoint approach does not cover all the important effects of acidification (e.g. toxic releases of aluminum to aquatic environment and impacts on materials in built environment are missing).

Behind each approach there are many choices and inputs that can decrease environmental relevance. It is not self-evident that more data and models can increase environmental relevance. The big differences between the continent specific results produced by different methods can be confusing and also applies to country-specific results.

Suggestions for work package – or task leader:

The reliability of CFs could be checked by showing the results to experts on acidification and asking their opinions about the results between different countries and continents

See the next section.

Scientific robustness & Certainty

Weak				Robust
		x		

Additional comments

It is currently unclear in what ways the approach of Type 1 is better than the approach of Type 2. They currently produce very different results on both country and continental scales. The analyses made in the article do not reveal the most important factors causing the differences.

As mentioned in the previous section (Environmental relevance), a scientific basis for deriving CFs from the characterization model of Type 2 (equation 2) should be clarified. The model can be reconstructed using the idea of the AE method. In this way, it is possible to offer an alternative midpoint approach which may give better basis for the comparison with the approach of Type 1.

The uncertainty of results produced by the approach of Type 3 is large. Obviously, the more data and models are combined the greater the uncertainty will be. In practice, it is not possible to assess the whole uncertainty of the model in a reliable way.

The country-specific characterization factors are too rough for big countries.

Posch et al. (2008) showed that the resolution/quality of an air quality and transport model plays role in the final results in which critical loads/ ecosystem sensitivities are taken into account. In addition, it was recommended to update CFs if remarkable changes in emissions will be occurred in the future.

Suggestions for work package – or task leader:

There is a need to analysis the differences between Type 1 and Type 2 and try to find a scientific basis for the recommendation. In addition, it is recommended to reconstruct the calculation rules of Type 2. This gives better basis to compare the approach of critical load to the approach of Type 1.

It is worth highlighting the meaning of the resolution by using grid based calculations in different parts of a selected country and then comparing the results to those calculated using the corresponding country specific CFs.

Documentation & Reproducibility

Weak				Robust
			X	X

Additional comments

The work is well written and documented. For experts, it is easy to read and follow the idea. However, the difference between the approaches of Type 1 and Type 2 are described and analyzed too generally in order to make a choice between them.

Suggestions for work package – or task leader:

Please, take the findings outlined in this evaluation into account in the final article.

Applicability

Not applicable				Applicable
		x	x	

Additional comments The paper proposes both midpoint and endpoint methods for each country and continent. The use of the methods is straightforward and easy. However, there is no scientific basis presented to omit the approach of Type 2. On the other hand, it seems that the weaknesses of Type 2 related to its characterization model are obvious. On the basis of new construction it is maybe possible to produce an alternative approach compared with the approach of Type 1. Depending on the results of the scientific comparison, LCA practitioners will have one or two models recommended at the midpoint level. At present, the approaches of Type 1 and 2 offer very different results causing difficulties in the interpretation of final results. The huge uncertainty involved in the approach of Type 3 decreases its applicability. For this reason, it is very difficult to trust the ranks based on this approach. It can be said that the work represents the current best knowledge about the assessment practice on a global scale. However, in European applications there is maybe no basis to replace the earlier European country-dependent characterization factors with the factors generated in this study. They can be parallel methods in some applications.
Suggestions for work package – or task leader: In the next step, there is a need to clarify recommendations and good practices: how to use the recommended CFs and how to interpret their results?

Specific remarks (please indicate page/line in the document)

In Figure 1 the terms PNOF and AL/BC should be written as "potentially not occurring fraction (PNOF)" and "Aluminum/ Base cations (Al/BC)"

p. 11. (3 lines before equation 7): " …namely the base cations to aluminium ratio (BC/Al)" should be " … namely aluminum to the base cations ratio (Al/BC)"

p. 12. (3 and 7 lines after equation 10): should be because this notation has been used in the previous equations. The same concerns the notation used in S1

p. 16 (equation 16): should be , i.e. it is the total emission P of country i. Please, make this correction to the text after the equation.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D3.4 and D3.5
Date	28-10-2012
Reviewer	M. Schaap
Contact (e-mail)	Martijn.Schaap@tno.nl
Contact (phone)	+31 6 11783060

Summary of the review and key issues

The study presented here uses two state of the art chemistry transport models to assess spatially differentiated intake factors for ozone and particulate matter. Therefore, an important step forward is made compared to the current situation.

Spatially differentiated intake factors have not received a lot of attention so far. This is evidenced by the few references available and use of e.g. European value of one study for ozone applied to the whole world. The robustness of model responses to emission changes has received little attention and in Europe only has been assessed within the EURODELTA and HTAP studies. The validation status of these reduction simulations is limited. The range in results, especially for PM, indicates the uncertainty associated with these simulations. Hence, the uncertainty is still large and many challenges need to be resolved to further improve the impact factors step by step.

Completeness of scope

Incomplete				**Complete**
			X	

Additional comments

- In both deliverables it not completely clear whether or not the simulations of EMEP and TM5 were performed within this project or not. I concluded that the EMEP data were taken from a previous study, but that the TM5 data were generated within the project. Is that so?
- For PM it is unclear to me which set of intake factors are to be used.
- Both reports lack a motivation for the use of TM5 and EMEP models.
- The reports do not provide a discussion on the quality of the model performance in comparison to observations, which could be important for the interpretation of the CFs.
-

Suggestions for work package – or task leader:

Environmental relevance

Irrelevant				Relevant
				X

Additional comments Air pollution levels are controlled by emission (mixture) density and meteorology/climate. Moreover, the impact of air pollutants on population and natural areas is dependent on the proximity of receptors to sources. Hence, emissions in different regions of Europe and the world are anticipated to have different impacts. Incorporating these impacts in LCA is a meaningful, but challenging endeavor.
Suggestions for work package – or task leader:

Scientific robustness & Certainty

Weak				Robust
		X		

Additional comments The study presented here uses two state of the art chemistry transport models to assess spatially differentiated intake factors for ozone and particulate matter. Although state of the art models are used, major uncertainties are still associated with the chemistry transport modeling that affect the robustness of the results. Spatially differentiated intake factors have not received a lot of attention so far. This is evidenced by the few references available and use of e.g. European value of one study for ozone applied to the

whole world. The robustness of model responses to emission changes has received little attention and in Europe only has been assessed within the EURODELTA and HTAP studies. The validation status is limited. The range in results, especially for PM, indicates the uncertainty associated with these simulations. Hence, the uncertainty is still large.

One issue that stands out is the different approach taken for ozone and PM with respect to the use of archetypes. For ozone this is not used, though urban areas have generally lower ozone due to ozone titration. In case of PM the factors by Humbert et al (2011) are used to downscale IF fractions to a subgrid level. Somehow, I have the feeling the Humbert et al factors and the explicit regional modeling cover partly the same ground.

Note that besides model uncertainties major uncertainties are associated with the emission data used in the study. Major uncertainties include:
1. Many activity data and emission factors for the developing world are very uncertain.
2. NMVOC speciation is based on limited studies for western conditions and basically not updated since the early nineties.
3. Spatial allocation may be poor on a global scale. Missing information may mean allocation following population density causing unwanted correlation with the impact results presented here.
4. Time profiles for emissions are important. Diurnal cycles especially for primary species, and meteorological dependency for all sources impact atmospheric formation, lifetime and mixing. Current simulations lack them (TM5) or are very basic (EMEP).
5. Downscaled emission data tend to overestimate emissions in urban areas as energy use in cities is more efficient than in rural areas.
6. Biogenic emissions of VOCs are very uncertain and impact the NOx and NMVOC response.

The SOMO35 based impact assessment is very sensitive due to the threshold in the indicator. EMEP overestimates background ozone and therefore has larger SOMO35 levels than other models. This may result in different responses per unit emission reduction. The robustness of the assessment based on different ozone indicators should be investigated in the future.

Suggestions for work package – or task leader:

The uncertainty discussion could be a bit expanded. Especially for PM.

Documentation & Reproducibility

Weak				Robust
			X	

Additional comments

The study performed here is reproducible, but requires access to the SRM data or the facilities to perform the simulations yourself.

Suggestions for work package – or task leader:

Applicability

Not applicable				Applicable
			X	

Additional comments

The global Ifs based on TM5 are based on large source regions. As for Europe, the variability within these source regions may be large. Therefore, also the use of these factors should be taken with care.

Due to the setup I have the feeling that the ozone response from NMVOC is very uncertain for biomass combustion and not applicable for biomass plantations (biogenic emissions).

Suggestions for work package – or task leader:

Specific remarks (please indicate page/line in the document)

D3.4

Title and authors on title page are different from page 8.
Page 17. Under concentration there are a few P's too much for PM2.5 and PM10.
Page 17. EMEP –SRMs should have all Y
Figure 7. Yellow range missing… or strange pattern, please explain.
Page 22. Third line from below: no effect should be more limited effect
Page 24. EURODELTA also performed these simulations with height differentiation.
Page 26. The recalculation of TM5 Impact factors to different zones using Humbert et al (2011) may seem to be partly a double counting (as meteorology is in both) and US conditions are applied everywhere. What is the impact of this assumption?
How good are the models to represent measured concentrations? Will there be systematic under or overestimations?
Stack height does impact iF for secondary material (see Eurodelta)
Page 39. I would not present iF for POM as the TM5 model does not treat it as a semi-volatile component, which it is currently associated to be. Rather use PPM2.5. The global POM and BC emissions are very uncertain. This should be noted.
Page 56. How do you apply the urban increment data? Urban scale emission data are uncertain to say the least.
Page 61. Which set of factors are recommended to be used?
GEOS-CHEM is mentioned in the introduction, but not used.

D3.5

For ozone two sets of SRMs are calculated discriminating all sectors and SNAP 1. Why wasn't the solvent use sector chosen? The difference in NMVOC speciation per sector may be much more important than stack height.

Page 7, 2nd line from below: The LOTOS-EUROS model was used to…
Page 12. Why is there no map for total NMVOC emission? Can easily be provided by the TM5 modellers.
Page 21. Figure 1.10: should the sign be the other way around? Why is former Yugoslavia excluded?
Page 24. Line 1. PM should be ozone.
Page 28, 2nd line from below. PM2.5 should be …
Page 32. Please provide a first guess for the effect of USA and Canada. Strange…
Page 33, What is the main mechanism behind the impact outside Europe? LRT of ozone? Or reservoir species? Grid resolution?
Page 42. How do the results derived here compare to van Zelm et al. 1999.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact

This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	D3.4: Recommended assessment framework, method and characterisation factors for human health impacts of fine particulate matter formation: phase 2 (report, model and factors)
Date	19.10.2012
Reviewer	Marko Tainio, Systems Research Institute, Poland; National Institute for Health and Welfare, Finland
Contact (e-mail)	marko.tainio@ibspan.waw.pl
Contact (phone)	+48-22-38-10-231

Summary of the review and key issues

Completeness of scope

Incomplete				Complete
				X

Additional comments

The purpose of the task is to create characterization factors for particulate matter (PM) air pollution emissions so that the health effects of PM can be estimated in life-cycle impact (LCI) studies by using these factors. The scope of the task is challenging but the approach used answers well for this challenge.

Suggestions for work package – or task leader:

The scope of the task is complete.

Environmental relevance

Irrelevant				Relevant
				x

Additional comments

This work is highly relevant for the life-cycle impact (LCI) assessment studies, and to assessing of health effects of different products & services.

Suggestions for work package – or task leader:

-

Scientific robustness & Certainty

Weak				Robust
			x	

Additional comments

Scientific robustness is high. However, there are few methods that would need more clarification and/or discussion. Especially health effect estimation part of the methods should be described better. See Specific Remarks (below) for details.

Suggestions for work package – or task leader:

See Specific Remarks.

Documentation & Reproducibility

Weak				Robust
		x		

Additional comments

Terminology and the description of methods should be improved. Especially acronyms should be checked. For example, both PPMCO and PM010-2.5 acronyms are used to describe coarse fraction of PM. In page 6, acronym used for the Respiratory Hospital Admission is RAD, which usually means Restricted Activity Days (RHA is acronym for Respiratory Hospital Admission's). YLD is described as "Year equivalent Lost Due to morbidity" while WHO use definition "Years Lived with Disability". Instead of YOLL, WHO also use acronym YLL in the DALY context.

Completely own discussion is the use of term intake factor (iF). Most of the previous studies use term intake **fraction** (iF) to describe the fraction of emissions that is e.g. inhaled by the population. Also the original iF paper by Bennett et al., from year 2002, use term intake fraction. I don't completely understand why the existing term is replaced with new one (this issue is briefly mentioned in page 13 but the chapter would need to be rewritten since these two terms are mixed – issue that also make my point of using two different terms from same concept)?

Methodological issues are described in the Specific Remarks (below).

Suggestions for work package – or task leader:

Terminology should be harmonized and the report should be rewritten thoroughly.

Applicability

Not applicable				Applicable
				x

Additional comments

The description of the results with intake factor, effect factor and characterization factor concepts allows the usability of these results beyond the context of this project.

Suggestions for work package – or task leader:

-

Specific remarks (please indicate page/line in the document)

Below is the list of main issues that would need more description and/or rethinking:

- The description of the health effect calculation (effect factor) should be more extensive, including references to original studies (e.g. in table 6, page 29). Now some of the data and methods are not presented clearly.
- The same dose-response and severity scales were used all over the World while the intake factor differences between regions were taken into account. I would propose that the team will consider also region specific effect factors because background health effects differ largely between different regions. Also, the issue on non-linear dose-response function (page 59) could be taken into account if the effect factors would differ in different parts of the World. Non-linear dose-response functions have been developed e.g. WHO 2004 (http://www.who.int/quantifying_ehimpacts/publications/ebd5.pdf).
- Urban increment approach and its usage would need to be described more details. For example, is urban increment taken into account in the final iF results for urban areas? The approach resembles the so called city-delta approach used in the RAINS/GAINS models, developed by IIASA, but the way how it has been used in this study is unclear. Also, authors might be interested to compare the results of urban increment for different cities with the study of Apte et al. (http://pubs.acs.org/doi/abs/10.1021/es204021h). In that study, iF's for urban air pollution emissions in 3646 cities were estimated and these iF's could be compared with the iF's presented in Table 20 (page 54).
- Duration of endpoint for 'mortality' and 'YOLL chronic' is one year in Table 6 (page 29) indicating that one mortality case means loss of life of one year (YLL = 1). This seems underestimation of the effect since earlier has been assumed that one mortality case due to PM2.5 would mean loss of life equal to 10 years (YLL = 10). Also, reporting of both mortality and YOLL mortality gives an impression that both short term and long term mortality estimates were used and combined?
- Emission weighted iF, population weighted iF and parameterization of the iF should be described more detailed in page 26, or nearby (now emission weighted and population weighted iF's are described only in the supplementary material (page 72)). Differences between these three iF's is important for understanding of these results.
- The intake factor equation 2 in page 14 is repeated in page 25 (equation 8) using different symbols. Symbols should be more consistent throughout the report.

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

Template for reviewers of LC Impact
This template is designed for the reviewers of the LC Impact work. Please use this template to give your input and feedback.

Review of deliverable/task	3.6
Date	25/10/2012
Reviewer	Dr. Olivier Baume and Dr. Enrico Benetto
Contact (e-mail)	olivier.baume@tudor.lu enrico.benetto@tudor.lu
Contact (phone)	

Summary of the review and key issues

The operationalization of the noise assessment method described in 3.1 is certainly very pertinent to the stated goal and complete enough. The idea of providing a tool for the calculation of user defined CFs is brilliant. The framework could be easily extended if additional or complementary data and information are available.
The main improvement point concerns the documentation, i.e. the form of the report and the presentation of the operationalization. Both may still be much improved in a revised version of the deliverable. We recommend the authors to consider our comments in the related sections as possible inputs to the revision process. A few comments in the section "scientific robustness" may also help to better clarify a few methodological issues.

Completeness of scope

Incomplete				Complete
			X	

Additional comments

Considering the current state of the art of LCA, the scope of the assessment method is very complete. The authors have successfully managed to build an assessment scheme which is much closer to specific (spatially defined) situations (i.e. emission/targets conditions) than any other proposed method for noise assessment in LCA.

From our experience, current noise assessment methods in LCA have the tendency to overestimate the noise impacts due to the too coarse level of detail and scope of the situations described. It is

unclear how far this also applies to the method presented here and to the estimation of the related parameters and variables. Whether an enlargement of the scope of the assessment to include much more detailed situations, e.g. by considering a number of archetypical situations, corresponding to actual conditions, within each cell could be beneficial or not should be discussed. Because of the importance of (existing) background noise to the assessment, the actual reliability of the proposed assessment method is unclear as well.
Suggestions for work package – or task leader:

Environmental relevance

Irrelevant				Relevant
				X

Additional comments

Noise assessment is very relevant in LCA (despite most often ignored) and therefore the relevance of the proposed approach is very high.

Suggestions for work package – or task leader:

We would suggest better introducing the relevance of noise as impact category in this report but maybe this has already been done in other WPs.

Scientific robustness & Certainty

Weak				Robust
			X	

Additional comments

The soundness and limitations of the SC approach should be discussed further. The spatial approach certainly allows a better representation of local specificities but how far we are from the real situation. In other words, how far is this approach from the case of the modeling of detailed spatial contexts using dedicated noise models?

Main concern is about the consideration of background sound environment. As the impact is related to the increase of sound pressure as compared to the background conditions, this element is primordial. At page 12, the value of background sound is considered equal across all centre-frequency bands. This assumption is not discussed. From our perspective, it would be worth analyzing the consequences of this assumption on the CFs, e.g. considering other scenarios and performing a sensitivity analysis. This should be included at least in the final discussion.

It could be informative to evaluate the sensibility of target persons for an increase of noise pressure correspondent to the functional unit.

§1.4.6: the notion of “surface” is somehow unclear. In particular Sn is not explicitly written as a function of n. An example representing a clear scheme of situation case with the virtual source “theoretical” on the BANOERAC grid and a typical target would help the understanding.

Page 14: “by a factor of **2**”: could this be better justified? What is the assumption behind and the impact on calculation? Following the same line of reasoning, page 26: “at an average distance of 1 m from the receivers”. Why has the distance of 1m been chosen? Page 35: what is the rationale of adopting a pessimistic view by choosing the “maximum background sound power level”? This would require more discuss as the impact of a source in a silent environment can be huge, while sources can mask each-others and hence background noise could have a masking effect. This is especially relevant for the determination of FF. As a generic remark, from our perspective synergistic and masking affects are not treated and discussed enough in the report and would deserve better consideration as they have potentially a huge impact on the final results.
Suggestions for work package – or task leader: We would suggest the authors considering whether the above mentioned points could provide value to the method and its presentation.

Documentation & Reproducibility

Weak				Robust
		X (documentation)	X (reproducibility)	

Additional comments

From our perspective, the presentation of the report could be further improved.
In general the nomenclature is not precise enough for a sound understanding and linear reading of the formulas. Examples:

- "D" is mentioned twice (once for the Distance and once for the Directivity); the same for H.
- the list of indexes is missing ("i" for the period – day, evening, night – c for the compartment, f ….)
- ρ represents the population density (page11) and at the same time the volumetric mass of air (page 12) and is not listed in the nomenclature
- Page 12, the case of S/S0 is not consistent in the two equations
- Page 14: ψ is not in the nomenclature, is not explained page 14 (where it is introduced) and does not seem to be used in formulaes?
- Table 1.1: is the directivity factor "Q" or "$D_{\theta,\psi}$?
- Page 15, after table 1.1: "to a D value..". Should not it be DI instead of D?
- Page 17: again a problem of consistency with the case of P (p)
-

Equations could be numbered for easier reference.

Page 12: Lpavg is not well defined: how is it averaged?

Page 13: please explain how the second equation is obtained from the first one (from 10log(4πd) to 10log(100))

Page 14: "the mean intensity **Iavg"**

Page 15 last paragraph: practically, which other types of attenuations could be considered?

Page 19, table 1.2: we would suggest to add the bold text: "definition of the G coefficient **for acoustical ground properties"**; instead of "attenuation of sound propagation" would put "effective flow resistivity" which is a more usual term in acoustics; instead of "g value" would put "G **coefficient**"; first item in the table content: "very soft (sno**w** or…**")**

Page 22, equation: what is the meaning of H?

Page 23 top: $A_{\text{ground, favourable, min}}$: what is the meaning of favorable?

Page 23: speed of sound is first 343.2 m/s and later 340 m/s…

Page 24: the chapter is on the **effect** factor and then in the second line of text you wrote "the unit of **fate** factor.."; in the question (and related text), are the indexes I and f interchanged (α should

depend on f and β on i)? Page 26 top: why is "D" not listed? Page 27 top: α does not appear in the equation. Furthermore, it is cited once with the suffix m an once with n. What is the meaning of the "prime" in R_{ref}' ? Table 1.4: should not be "SC" instead of "AC"? Table 1.5: should not be "AC" instead of "SC"? Is there an issue with the cross-references to chapter sections in the text? For example page14, at the top, the cross-reference to section 3.1.2.2? Where is this section? There are plenty of such references which cannot be found. Maybe we missed something. The way how map algebra actually works could be better explained. An additional introduction on the objectives of the deliverable would certainly be beneficial for the readers, as a reminder of D3.1 Please provide more details on the reference to CNOSSOS. §1.3: Additional qualitative and quantitative information on the concepts behind SC and AC would be certainly beneficial for the readers. LCA readers could gain a better appraisal of the acoustic specifications. Readers coming from the acoustic field would be more comfortable if a clearer description of the acoustic scenarios would be provided. We suggest to clearly present AC and SC, precise choices, data and approaches used (for an acoustician it is very difficult to understand as he/she is waiting for a very detailed physical context). Also, the simplification from an ideal noise model that would be practically intractable in an LCA framework should be more clearly stated. Data flowcharts in Figure 1.1 and 1.2 (in their current form) are not useful and hardly understandable. We suggest to add more detailed about the calculation steps, and the relations between the elements. Also, they are maybe introduced too eraly without proper connection to the equations considered in a later stage. From our perspective, the Executive Summary (in the current form) could be extended to be more informative for decision makers and possible users of the method. For example, insightful description of the SC and AC context could be provided. Finally, a scheme to illustrate the calculation steps (defined in 3.1) would also facilitate the reading, to show at each moment where are we in the implementation of D3.1
Suggestions for work package – or task leader: We would suggest the authors considering whether the above mentioned points could provide value to the method and its presentation.

Applicability

Not applicable				Applicable
			X	

Additional comments

The overall applicability of the noise assessment framework illustrated is “very good”.
Basic inventory data (sound powers) are still missing, which maybe is the main limiting point but still independent from the work described here.
The tool for the calculation of user defined CFs will certainly be very useful for the practitioners.

From our perspective, it is unclear how the assessment framework will be applicable to mobile sources, e.g. transports. The applicability is not discussed enough in the current report. Is the SC approach to be preferred? Is the grid dimension sufficient to catch the transport specificities? Or is the AC approach to be preferred, but still is it flexible enough?

Suggestions for work package – or task leader:

The applicability of the framework proposed to different (static and mobile) source types could have been discussed more in detail, in a dedicated section, e.g. before the discussion section.

Specific remarks (please indicate page/line in the document)

Pages 6 and 7: "Hz" is missing after "2000"
Page 17: section 3.1.2 does not exist; please check the numbering of paragraphs
Page 3: D is repeated twice
Page 4: indexes are used with different meanings: I,c f… not consistent
Page 5 end: spelling: "two direction***s*****", "…**form **of** raster**..**"**
Page 8: what is the precise meaning of "vertical average"?
Page 10: instead of "meteorological variation" would be better "ground and meteorological conditions"
Page 11: a**t** a location
Page 14: "second term **to** the right" ?
Page 28: "case" instead of "cased"; last paragraph: "**we** will focus" instead of "will focus.."
Page 32: please add the bold: "..while the most affected **ones…"**

Thank you for your input!

The LC Impact Team,

Prof. dr. M. Huijbregts

7.4. List of participant at the workshop in Brussels

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7.5. Minutes of LC-impact meeting with domain expert

7.5.1. WP1: Resource use impacts (Stefanie Hellweg, ETH)

In the below text, the bullets list the comments by the reviewers, while some specific answers of the team are provided after the ⍰ sign.

OVERALL COMMENTS (refer to all tasks)

- The deliverable contains methods of different development stages (some very novel and not so applicable, others more applicable but less new)
- Goals of the assessment should be stated more clearly (what do we want to assess?)
- Indicators are rather diverse ⍰ scientific project with new developments; harmonization not from the start (includes also exploratory research)
- Check double counting? e.g. land use and pesticide application ⍰ done on the meeting for inter-WP overlaps (within the task on land use, some further action concerning erosion needed)
- Give overview: mention also gaps (e.g. missing impact assessment of flooding); framework
- Land use overlap with other impact categories (human health etc.) ⍰ land use as LCI flow which induces various impacts (e.g. carbon storage⍰ impact on climate change and human health); put it into one framework (what is LCI, what is LCIA)
- Involve specialists from relevant disciplines
- REFERENCE state (in general PNV, but sometimes different, give guideline when which is appropriate)
- Need separate document for stakeholders (only globally applicable methods, SOME harmonization) – more reader friendly and application oriented
- Time frame? E.g. less than 100 yr for metals? ⍰ changed to 20 yr for metals; reasoning will be provided.
- Average or worst case, e.g. for land use?

Task 1.1: Land use
Ottar Michelsen:

- land use at least 2 decades, still not there yet
- reference? Potential vegetation or present harvest level?
- consider also transformation
- relative versus absolute changes
- double counting: physical and chemical impact ⍰ issue was checked and no double counting is performed by current method proposals
- Completeness: how many impact pathways should be assessed in order to be completed
- Chapter 1:
  - data deficit, rough subdivision
  - Number of species: Invasive species? Fragmentation? --- discuss if this is really a useful indicator?

    - Not clear how close reference points
    - improvement of already existing methods
- Chapter 2
    - what functions are assessed
    - how possible that functional CF bigger than SR CF?
- Chapter 3:
    - Data limitations: average versus worst case; average masks results (shown in Chap 3 and 4); worst case situation could be much worse than average
- General questions. How many impacts should be assessed (different types of biodiversity, ecosystem?

Jo Dewulf

Chapter 1:

- most mature approach
- Different land uses (broadest method in terms of applicability)
- Room for refinement: zoom into agriculture (distinction of different annual crops)
- Unep/Setac – clarify relation ⍰ done on meeting (work was input to UNEP/SETAC working group)

Chapter 2:

- More novel, potential for applicability far more narrow; only land occupation; only for America so far; land use types are limited; not so obvious application of CF; clearer overview of CFs could be provided

Chapter 3 (forest and GWP):

- Interesting, but where is overall target (one particular land use type)?
- Environmental impact is Global warming (confusing)? Where is cause-effect-chain? FU 1 m3 of wood or method for LCIA? ⍰ not linear the crop, also did assessment on LCI; mention clearly range of application in space and time (what production),
- Check with forestry people?
- N2O considered too?

Chapter 4 (erosion):

- Strange again functional unit,
- link to biodiversity, ecosystem services or human health not so clear
- Is it only yield or also green manure etc…?
- Wheat over the whole world should be corrected for areas where it is really grown
- Crop rotation? ⍰ tradeoff simplifications and global level, discuss these issues (in connection also to Chapter 5 which considered crop rotations), assumptions, boundary conditions

Chapter 5 (erosion II)

- Emergy; scientific concept most interesting
- NPP – why a good indicator?
- Boundary of land use impacts
- Soil formation should not be addressed? Why?

Chapter 6 (seafloor)

- Interesting; but no integration to other chapters
- What aspect of biodiversity, ecosystem services or human health is considered?

Water use
Markus Berger
- Wetlands, salinity is first step (needs to be expanded),
- Relevance does not seem to be high species density assumption: correction for actual value of wetland in salinization study would show very relevant results; selection of case studies not done according to relevance
- Uncertainty: well addressed
- Additional explanations to increase understandability
- Represent case study in abstract as starting point
- Define land use type for green water chapter

Chapter 1
- New water consumption definition needed
- Wetland as a cone more explanations
- Involve experts

Chapter 2
- Difficult to understand
- Area change 1 m3 from large area smaller than for 1 m3 from small area
- Rarity score different from conventional, add explanations value choices discuss
- Different endpoints discuss with tox WP (done; approach is in line with methods from other WP)
- Involve experts

Chapter 5
- Impact assessment needs to be reconsidered (direct translation to blue water) simplification
- Better focus on inventory only.
- Weighting by natural availability, ratio of ET and P instead?
- How to deal with "credits" if forest is cut down and new land use demands less water?
- Coordination with land use is land use LCI and water use then impact; mention shortcomings in LCIA (e.g. missing impact assessment of flooding)

Marine resource use
Ole Ritzau Eigaard
- Midpoint indicator
- Great achievement (8 new impact categories), but data intensive and large uncertainties
- Red list good…
- Better define by-catch
- Trophic level… easy to communicate, but broad assumptions
- MTL & PPR: broad assumptions, difficult to separate impacts of other drivers (e.g. technological development and management regulations)
- Is LCA suitable of covering such complex topic?
- WPY, F-/B-Overfishing: clearer state limitations of methodIan Vazquez-Rowe
- Single-stock based, LCA should go beyond

- Seafloor: difference between gear missing, but important for determining impacts
- Fish-oriented (other organisms?)
- Mass as unit (trophic level, primary production rate?) agree, but still an informative indicator (mass and primary prod important – define scope)
- By-catch: concept needs clarification, how does this relate to sustainable yields?
- Aquaculture: is approach valid there too? Partly applicable, but also new impact categories required.
- Time horizon for future assessments? Inclusion of temporal variability? Continuous updates needed and possible (e.g. every 5 years based on annual stock assessments). Fisheries management is more on yearly basis, future projections is less relevant here
- approach is dream scenario (apply on case study); feedback is helpful and good; define function

Mineral and fossil resources
Jo Dewulf
General
• Are we moving beyond environmental LCA when talking about availability? More an economic issue?

Chapter 1
- What is target, scarcity or availability? What do we want to assess? future cost to society
- Ore grade to measure scarcity – easy to understand and communicate, but simplification of reality (other issues: easiness to access etc., but good as first step) average assumption, geologists: have an idea about decrease, but everyone is lying…
- Spatial differentiation? Global market?
- Future demand, running into economics, going beyond physical characteristic
- Problems: ore grade does not always decline
- Reserve estimates: uncertainty,
- Co-mining? Allocation? Ore grade then? inherently addressed, allocation in terms of ore grade

Chapter 2 (surplus cost)
- Energy efficiency gains with time? Process development in mining techniques?
- Storage in anthroposphere different (Application and metal dependent)
- Forecast until 2100 demand uncertainties large; inconsistency with stakeholder responses; maybe really go down to 20 years for the metals
- Cost? raise cost to society because of resource extraction
- Minerals: fixed costs
- Phosphourous: large uncertainties of reserves

Chapter 3
- Rather straightforward, not as novel
- Substitutionability versus other fossils or other energy sources? based on IPCC scenario, more for inventory
- Assumptions: reserves, demand and recycling – does it all matter?

7.5.2. WP2: toxicity related impact categories and spatial differentiation (Ralph Rosenbaum, DTU)

Task 2.1 Metals

N. Gandhi:

- The new method for calculating terrestrial ecotoxicity CF of metals should be considered for other metals as well. The limitation is the availability of sub-models for calculating the underlying factors, particularly terrestrial biotic ligand models (TBLM). A solution would be to use free ion activity model (FIAM) – but even there this might be limited because in many studies where effect data is reported, the water chemistry is not reported which challenges derivation of the FIAM.
- An aspect to be considered in the current method is whether the TBLM can be applied to the soils used in the current dataset (particularly, the TBLM might not be suited to the calcareous soils).
- The method might be adaptable to other environmental assessments, which would be a challenge but would have benefits.
- A qualitative discussion on the uncertainty in method application and parameters would be helpful to avoid pitfalls.
- In order to increase reproducibility of results, more information on TBLMs and FIAMs should be provided in supporting information.
- Unexpected result: FIAM should overestimate rather than TBLM due to also considering competition with other cations. Could assessment factors be the reason?
- BF and AF should be explained more explicitly including units and interpretation/definition (call AF "accessible fraction" instead of "bioaccessible fraction"?)
- An important question is how to (at least "crudely") treat the "untreatable" metals framework will change the more metals are covered as other processes become dominant.
- A consistent definition of a limited set of archetypes for all metals and ultimately all emissions (hardness, pH, DOC, ...?) should be considered at some point.

Task 2.2 Whole effluents

Methodology and results for calculating the fate factor, and the methodology for calculating the effect factor for aquatic ecotoxicity of whole effluents is presented by C. Raptis. The approach for whole effluent toxicity (WET) is: WET = toxicity from organics + toxicity from metals

The main comments from the reviewer (N. Gandhi):

- The proposed additivity concept for metals is not always working and should be reconsidered. Synergistic effects are expected to occur in metal mixtures and child compounds may be more toxic. A test with lab mixture(s) would provide insights.
- The main focus was put so far on the FF, but the results show that the range in FF is small 7.7-23.2 day, indicating that the focus should be put on the EF
- What is the applicability of the new method? How to apply it to other industries? What is the prediction power of the model in various environmental situations? How to evaluate mixtures of inorganics? What about mixed-type compounds like organo-metals, being neither purely organic nor inorganic? More background information is needed.

- Is degradation rate for mixtures faster or slower compared to that for isolated compounds? This should be addressed in the FF calculations
- The new approach is worth exploring, but model validation/evaluation is difficult and the question remains whether the proof of concept is ok.
- A discussion of median toxicity versus most sensitive species toxicity would be useful.
- Which elements (metals/inorganics) should be considered for EC50 calculation?
- The calibration range of BLMs does not cover all types of natural waters and limit their applicability.
- A rigorous check on model performance and assumptions is vital.
- How can the bioavailable fraction (BF) be calculated for the TOC approach?
- What is the influence of variations of effluent composition among all industries including daily variability?
- Concentrations need to labelled very explicitly what they express (free ion, bulk, etc.).
- Why was TOC chosen and have alternatives been considered/tested?

Organic chemicals

R. Rosenbaum: in spatially differentiated method, should we go for archetypes of spatial scaling?

M. McLeod: For practical reasons, we should go for archetypes.

N. Gandhi: We can learn from the work on characterization of metals in freshwater, where all freshwater archetypes are found in all continents.

Task 2.3 Terrestrial ecotoxicity

- Lack of bioavailability influence in soil and its relation to Kp, especially for hydrophobic chemicals.

Task 2.4 Pesticides

Discussion:

- The description of processes is very limited (e.g. mass transport and partitioning).
- Future work: study the influence of chemical interactions, metabolites (molecule degradation products)
- General discussion on steady state vs. dynamic models: using steady state models with properly defined assumptions and properties might be sufficient to generate relevant results
- Dynamic processes for ionisable chemicals.
- Derive alternatives for agricultural practice.
- Discuss the impact of metabolites.

Actions:

- Update references

Task 2.5 Spatial variability of organic chemicals

Some main points:

- The report should briefly discuss the reasoning for selection of the focus topics.

- The completeness of results presented should be improved and/or discussed more clearly (e.g. lacking details on datasets, equations, and assumptions for comparison exercise; quality of geographic data and their resolution).

Completeness of scope

- Objectives met
- Need to readjust structure according to the task goals

Scientific robustness and certainty

- Results are difficult to understand
- Structure not always clear
- Change "toxicity" to "exposure" in the title
- Justify the choice of models to compare for spatial variability (check deliverable 1)
- Compare the features of different multimedia fate and exposure models
- Quality of data: for geographical data, justify the origin of the data (model data); for chemical data, provide more info on the OMNITOX project and selected chemicals
- Discuss steady state vs unsteady state modeling (steady state may over-predict impacts of non-continuous emissions), mention/discuss more transparently the model properties (soil depth, influence of temperature on spatial variability seems surprisingly low)
- The definition of Kair is different from original reference.
- Archetypes not all developed and tested, but reviewers recommend to evaluate all of them.
- Archetypes could be distilled by systematically analyzing variability of outputs from USEtox when parameterized for wide ranges of spatial scales and environmental conditions.
- The motivation for developing less complex archetypes is not discussed.
- Scientific robustness: is this in depth analysis the most efficient path to analyze spatial differentiation? E.g., we could instead take USEtox and analyze the variability of results according to chemical parameters in the default generic landscape
- Consider the role of plants in the model framework
- Integrate archetypes into USEtox
- What is the right resolution? Ecoregions? Choose and provide recommendations for the most relevant resolution

Discussion:

- Integrate an archetype model within each continent landscape in USEtox
- It would be interesting to develop a "smart" model that would systematically choose the right resolution when the user enters emission data

Actions:

- Clarify the structure of the deliverable
- Merge parts from M18 deliverable in the final deliverable

7.5.3. WP3: Non-toxic pollutant impacts (Phillip Preiss, USTUTT)

1. Attendees:

Reviewers:
Jyri Seppäla (Aci + Eutro)
Jaap Struijs (Aci + Eutro)
Marko Tainio (Fine particulate matter + ozone)
Martijn Schaap(Fine Particulate matter+ ozone)
Enrico Benetto (Noise).

LC-IMPACT consortium:
Anna Kounina, Assumpcio Anton, Ligia Azevedo, Marta Torrellas, Michael Hauschild, Nuno Cosme, Philipp Preiss, Reinout Heijungs, Rosalie van Zelm, Stefano Cucurachi, Thomas van Goethem.

2. Introduction

The participants introduced them self shortly.

The Task Leaders and corresponding Experts where asked to present their points (ca. 10 Minutes each) followed by discussion of the topic. The key outcomes and decisions where collected based on the written and presented feedback. Minutes were written by Philipp Preiss and the task leaders
T1: Nuno Cosme & Ligia Azevedo
T2: Anna Kounina & Ligia Azevedo
T3: Philipp Preiss & Thomas van Goethem
T4: Stefano Cucurachi & Reinout Heijungs.

In the following the notes for the separate Tasks are listed.
3. Task 1 Aquatic eutrophication (Nuno Cosme)
Partners involved: Technical University of Denmark (DTU) and Radboud University (RU)
Impact categories: Marine eutrophication (developed by DTU) and freshwater eutrophication (developed by RU)
Invited expert reviewers: Jaap Struijs and Jyri Seppälä

Nuno Cosme (DTU) started with a short presentation on marine eutrophication followed by Lígia Azevedo on freshwater eutrophication.
Jaap Struijs and Jyri Seppälä shared their comments on the M33 deliverable report on both categories. The experts' review focused on "Completeness of scope", "Environmental relevance", Scientific robustness & Certainty", "Documentation & Reproducibility", and "Applicability".

1) Notes on Freshwater Eutrophication
    - Other effect factors than linear (marginal effect change and overall effect from background situation) tested in freshwater;
    - Only P is modelled;
    - Effects on autotrophs and heterotrophs in lakes and rivers.
    - Review comments by Jaap Struijs:
    - Not evident that P is the sole limiting nutrient in tropical waters – both N and P can be limiting and hence N should also be addressed;
    - Can the developed model be applied to derive water quality criteria (e.g. protecting 95% of the species)?
    - Review comments by Jyri Seppälä:
    - Questions ability to predict damage as a consequence of additional loading of nutrients;
    - Recommends more inspiration from acidification characterisation than from ecotoxicity acidification.

2) Notes on Marine Eutrophication
    - Fate model based on Bouwman (export to river), Roy (deposition) and Van Drecht (SW emissions);
    - Denitrification in river systems before export to marine waters;
    - Marine-N loss model (delivering the FF), plus XF (with PP rates) and EF (SSD with average gradient);
    - CFs for countries, continents and global default.
    - Review comments by Jaap Struijs:
    - Why not use CARMEN model for fate? More differentiated than what Cosme applies (e.g. multiple separate parts of the Mediterranean Sea);
    - Likes the approach, in particular the exposure and effect factors, but perhaps replace the fate model with CARMEN for Europe.
    - Review comments by Jyri Seppälä:
    - P contributes to marine eutrophication in many coastal areas (e.g. Gulf of Mexico, Baltic Sea) and contributes to organic loading from freshwater eutrophication;
    - Not all airborne N should be taken in – only deposition on coastal areas (not open sea);
    - Suggests consideration of background situation and to give higher weight to already stressed ecosystems;
    - Use marginal rather than 0.5/HC50 when calculating EFs;
    - Biggest uncertainties -> improvement needs: spatial resolution (e.g. Mediterranean not as a single spatial unit);
    - Possibility to take P into account;
    - Suggests testing ME damage assessment factors on the case study and compare to damage results for other ICs and assess if the results of ME are meaningful.

3) General review comments across both ICs:

- Trade-off between precision and applicability;
- Completeness of scope;

- Testing the validity;
- Uncertainty analysis;
- Recommended level of spatial details.

4. Task 2 Acidification

Main feedbacks:
Environmental relevance weakness of Type 2 midpoint (critical load exceedance method)
A scientific basis for recommending characterization model of Type 1 (soil modelling) over Type 2 (critical load exceedance) should be clarified

Action points (short term):

- Qualitative description of existing critical load methods
- Verification of quantitative results of the current CL approach (Bouwman 2002)
- Action points (long term):
- Generate midpoint characterization factors based on Posch et al. (2008) and perform a quantitative comparison with midpoint approach of Roy et al. (2012)

5. Task 3 Human Health impacts due to PM and Ozone and EcoSystem impacts due to Ozone

Main feedback was on the fate modelling and the issues of non-linearity and non-marginal parameterisation of the regional and global CTM.

Summary of comments by Marco Tainio:
The scope is challenging but the study answers well for this challenge.
Deliverable use state-of-the art methods & tools.
Presenting of the results with intake factor, effect factor and characterization factor concepts allows the usability of these results beyond the context of this project.

Suggestions for improvement:
In the text improve balance between description of dispersion/exposure and effect factor (CRF).
Better explain concept of "urban increment" and make clear whether it is applied or not.
Recommendation to change term "Intake factor" into "intake fraction" because "intake fraction" is the convention according to: Bennett, D. H., T. E. McKone, J. S. Evans, W. W. Nazaroff, M. D. Margni, O. Jolliet & K. R. Smith (2002) "Defining Intake Fraction". Environmental Science and Technology, 36, 206A-211A.
This paper proposed, for the sake of consistency, the term intake fraction (iF) as the primary label for quantifying the emissions-to-intake relationship.
We shall consider the paper "Global Intraurban Intake Fractions for Primary Air Pollutants from Vehicles and Other Distributed Sources" Joshua S. Apte et al, 2012 because it provides iF's for urban air pollution emissions in 3646 cities around the world.

Summary of comments by Martijn Schaap:

Methodology used by LC-Impact: Two state of the art chemistry transport models were used to assess spatially differentiated intake factors for ozone and particulate matter.
Source receptor calculations are very large computational efforts that only few groups are able to do. More complete and detailed data do not exist at the moment. Reports provide a significant step forward.
The Set-up to derive source-receptor-matrices (SRM) follows political boundaries, which may not be optimal for LCA. Also for ozone creation due to NMVOC emissions SRM differentiating the solvent use sector and the differences in NMVOC speciation per sector may well be important.
For ozone no urban scale issue is addressed.
Although state of the art models are used, major uncertainties are still associated with the chemistry transport modelling that affect the robustness of the results.

Action points (short term):

- Take into account the written feedback.
- Discuss the use of the CFs for PM and the city delta approach in the report.
- Extend the chapter on uncertainties with CTM model uncertainties and potential impacts on results. More extensive discussion on uncertainties. More extensive discussion on human health impact based on concentration-response functions. For ozone urban scale issue will be addressed, i.e. it will be discussed that there is actually a need for a kind of “urban-decrement”. However, it is not expected that this issue can be solved in that way that a correction of CFs will be possible.

Action points (long term):
Suggestion for improvements – New project
Design a LCA relevant SRM approach:

- Redefine the regions of interest and devise a smart sampling strategy to assess sensitivities to emission changes.
- Or assess sub-region sensitivities on the different continents

Incorporate more detail on the emissions used:

- VOC speciation
- Spatial allocations
- Temporal variation

Use several models to test robustness of the central estimate or use several studies to compile impact factors

6. Task 4 Noise

Main feedbacks and derived Short term actions

- Sensitivity analysis. Included in final report: results of global sensitivity analysis already available.
- Documentation: form of report improved in new draft version.

- Applicability of framework to mobile sources: it is already applicable, but it will be better specified (e.g. train journey: 20% urban, 80% rural)

Short term actions - Points of further discussion
Background sound level:

- Equal value across all frequency-bands is due to limited availability of data.
- BANOERAC is not only the best available report on background sound emissions data but also the only one.
- Assumptions will be made more explicit.
- Research priority.
- Long term actions – Right level of spatial definition

Soundness and limitation of the spatial approach to the definition of CFs:

- Enlargement of the scope of the assessment including more archetypal situations of emission? To be analysed if more data is available
- Finding the right balance between spatially explicit CFs and archetypal CFs. User-defined approach already developed in the report may be further explored
- Synergistic and masking effects: included at the level of background sound emissions but:
- literature will be further investigated to verify if suitable for inclusion in the model.
- Long term actions – From midpoint to endpoint

It adds extra uncertainty but allows for reducing indicators:

- DALY for annoyance?
- DALY for sleep disturbance?
- Available but criticised

7.6. Combined presentations expert review

The combined presentations of the expert review are reported in the following pages.

Review LC Impact

D 1.3 Recommended assessment framework, method and characterisation factors for water resource use impacts: phase 2

M.Sc. Markus Berger

Brussels, 05 November 2012

Technische Universität Berlin
Department of Environmental Technology
Chair of Sustainable Engineering

Outline

- General comments
  - Completness of scope
  - Relevance
  - Scientific robustness & Certainty
  - Applicability
- Suggestions for improvement
  - Chapter 1
  - Chapter 2
  - Chapter 3
  - Chapter 4
  - Chapter 5

General comments

- Completeness of scope
  - Two important gaps in life cycle impact assessment of water use are identified and tackled - especially impacts on wetland ecosystems are addressed comprehensively.
  - The study quantifying biodiversity impacts from salinity in a coastal wetland, provides an interesting first step. Global characterization factors are so far missing and results cannot be transferred easily.
  - In a similar way the method estimating green water consumption of potential natural vegetation provides the (very important) basis for impact assessment of green water consumption by enabling the determination of net green water consumption. However, a proper impact assessment scheme is still lacking and it is not clear how to deal with “negative” net green water consumption

General comments

- Relevance
  - As shown in the results of the two case studies, impacts from area loss in the Peru wetland and from salinity increase in the coastal Spanish wetland seem to be of lower relevance compared to biodiversity damages from other interferences.
  - If it is a finding from this research that land loss and salination impacts resulting from water consumption in wetlands are of minor importance compared to other environmental interferences causing biodiversity damage, this should be clearly mentioned.
  - If this conclusion is not justified, it should be explained why ecosystem damages of water consumption in wetlands are so small compared to other impacts.

General comments

- Scientific robustness & Certainty
  - It is an inherent problem that those methods developing endpoint damage factors (chapter 1-4) suffer from uncertainties as many assumptions have to be made and statistical regressions are not always significant.
  - The two case studies (chapters 3 & 4) are more robust as they assess damages for a concrete wetland and a particular situation. In contrast, chapters 1-2 take a global perspective where even more assumptions and generalization is required.

    → Uncertainties cannot be avoided but are addressed in a clear and transparent manner. If possible, uncertainties are quantified and sensitivity analyses were conducted – this is very well done!
  - Green water consumption of PNV is determined in a robust manner and also here uncertainty estimates are provided enabling reliable results. In contrast to the high scientific robustness on the inventory level, the framework for impact assessment (which is not the focus) is not as robust

General comments

- Documentation & Reproducibility
  - In general the entire deliverable is documented well allowing for a good understanding of the basic ideas of the methods as well as the results and discussions.
  - However, some methodological steps are explained very briefly making it difficult to really understand methodological details for scientists not involved in the development.
  - Moreover, the position of the case study in chapter 3 is misleading as in the abstract it is claimed to be the test of the method developed in chapter 1 and 2. However, this is not true as the methodology is not the same and this study was performed before. So it is rather the foundation on which the work of chapters 1 and 2 is based.

General comments

- Applicability
  - Fate factors developed in chapter 1 are available for wetland on a global level. As soon as effects factors are available on a global level too (in progress), this method is perfectly applicable in LCIA.
  - The case study concerning salination in coastal wetlands (chapter 4) is an interesting first step but so far not applicable as results cannot be transferred directly to other wetlands.
  - Chapter 5 allows for a global determination of net green water consumption by providing evapotranspiration figures for PNV.
  - May be guidance on how evapotranspiration of the present land use type can be determined would be helpful for practitioners, which would allow for determining net green water consumption.
  - I recommend publishing characterization factors and PNV green water consumption figures by means of GIS shape files or in Google Earth Layers.

Suggestions for improvement

- Chapter 1 (new version)
  - It is stated that only consumption but not withdrawal of SW and/or GW leads to impacts in SW/GW-fed wetlands. This is based on the assumption that non-consumptive withdrawal is discharged back to the wetland.
  - However, according to the general definition of non-consumptive water use, the discharge needs to occur within the same watershed – not-necessarily within the same wetland.
  - Hence, for the first 4 chapters a new and more precise definition of water consumption is needed, which defines water consumption as evapo(transpi)ration, product integration, or discharge into seawater or **into areas outside the originating watershed.**

Suggestions for improvement

- Chapter 1
  - The wetland cone is modeled in two ways – using the wetland area and the water surface as base of the cone.
  - Using the surface water area and an average depth implies a virtual cone filled with water. This assumption is justified as a reduced volume leads to a decreased base area indicating the loss of wetland area.
  - However, using total wetland area as base of the cone implies a virtual cone consisting of water and soil. So a reduced volume of water can probably not be directly translated into a reduced base area indicating the loss of wetland. I assume that effects of soil and porosity need to be taken into account. This needs to be addressed or at least discussed.

Suggestions for improvement

- Chapter 2
  - Equation 2.1 is quite complex and it should be explained how you developed this relationship. By trial and error? Is it a general equation in biodiversity assessment?
  - The fate factors, provides a unit of m² of wetland lost. So according to traditional endpoint modeling, I would expect that the effect model provide something like PDF/m^3_{lost}. This is not the case as A_{new} and $A_{original}$ are parts of the equation.
    → a change from 10 to 9 m² would lead to different results than a change of 1000 to 999 m². Here some extra explanation is needed.
  - There is a weighting of the potentially affected fraction of species based on the rarity score. This sound reasonable but (I think) not really consistent with other methods assessing biodiversity damage from other environmental interferences (e.g. EI 99). This should be checked and if true mentioned, that results may not be directly comparable to other biodiversity damages.

Suggestions for improvement

- Chapter 5
  - In the introduction on page 68 you say that “The difference in these green water flows represents the lack of recharge of groundwater and surface water run-off (so called blue water) and can therefore be assessed by characterization facors (CF) for water consumption”.
  - This is a fundamental assumption and no evidence is given that this is true. I have doubts that the additional evapotranspiration of 1 m³ of soil moisture leads to an equal lack of 1 m³ of blue water.
  - I suggest focusing on providing robust green water consumption figures of PNV rather than touching a totally different topic of green water impact assessment.

Suggestions for improvement

- Chapter 5
  - In the life cycle impact assessment section (5.2.4.2), three options are suggested:
    - Assessment without characterization (CF=1): This is a pure inventory quantity and doesn’t make sense in the impact assessment stage
    - Using CFs for blue water consumption: I think this is not valid, as there is no evidence, that 1 m³ of green water consumption leads to 1 m³ of blue water consumption
    - Weighting water consumption by a function of natural availability: This sound reasonable, but why do you take ET of PNV as an indicator? Wouldn’t the ratio of ET and P be more reliable and consistent with the scarcity indicators used for blue water consumption assessment?

Suggestions for improvement

- Chapter 5
  - How to deal with negative net green water consumption, which is likely to occur, when e.g. forests are transferred into agricultural land in e.g. Central Europe.
  - On the one hand green water credits are justified as green water increases. On the other hand a green water increase due to lower evapotranspiration leads to decreased precipitation elsewhere. So the role of natural vegetation on the global water cycle cannot be ignored.
  - The next question, is how the altered land use and green water consumption influence blue water availability

Thanks for your attention!

markus.berger@tu-berlin.de

Technische Universität Berlin
Department of Environmental Technology
Chair of Sustainable Engineering

Reviewer of LC-IMPACT

Review on D1.2 & D1.4

Bruxelles, 6 November 2012

Ian Vázquez-Rowe

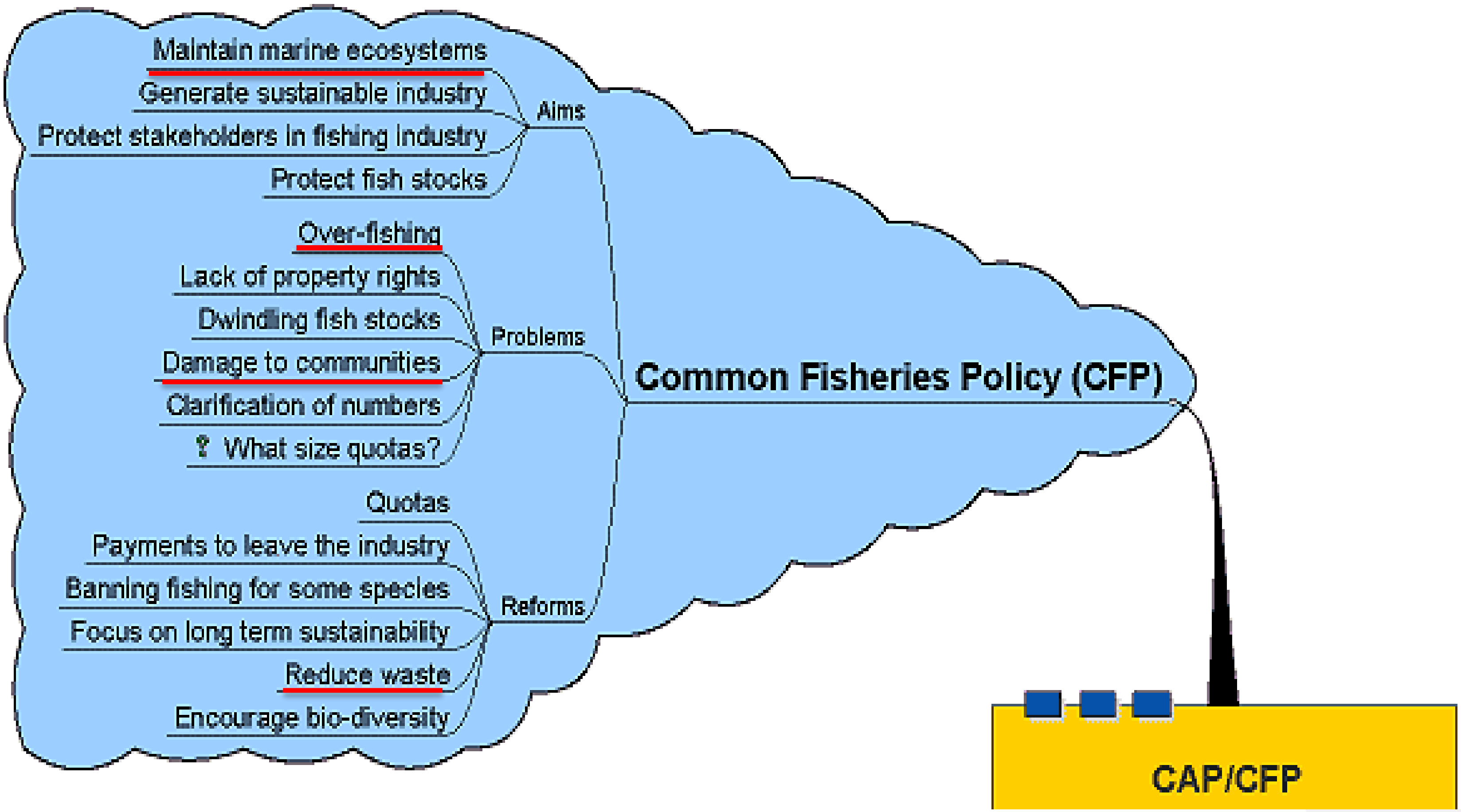
Public Research Centre Henri Tudor (CRPHT) - Resource Centre for Environmental Technologies (CRTE)

Esch-sur-Alzette - Luxembourg

Index

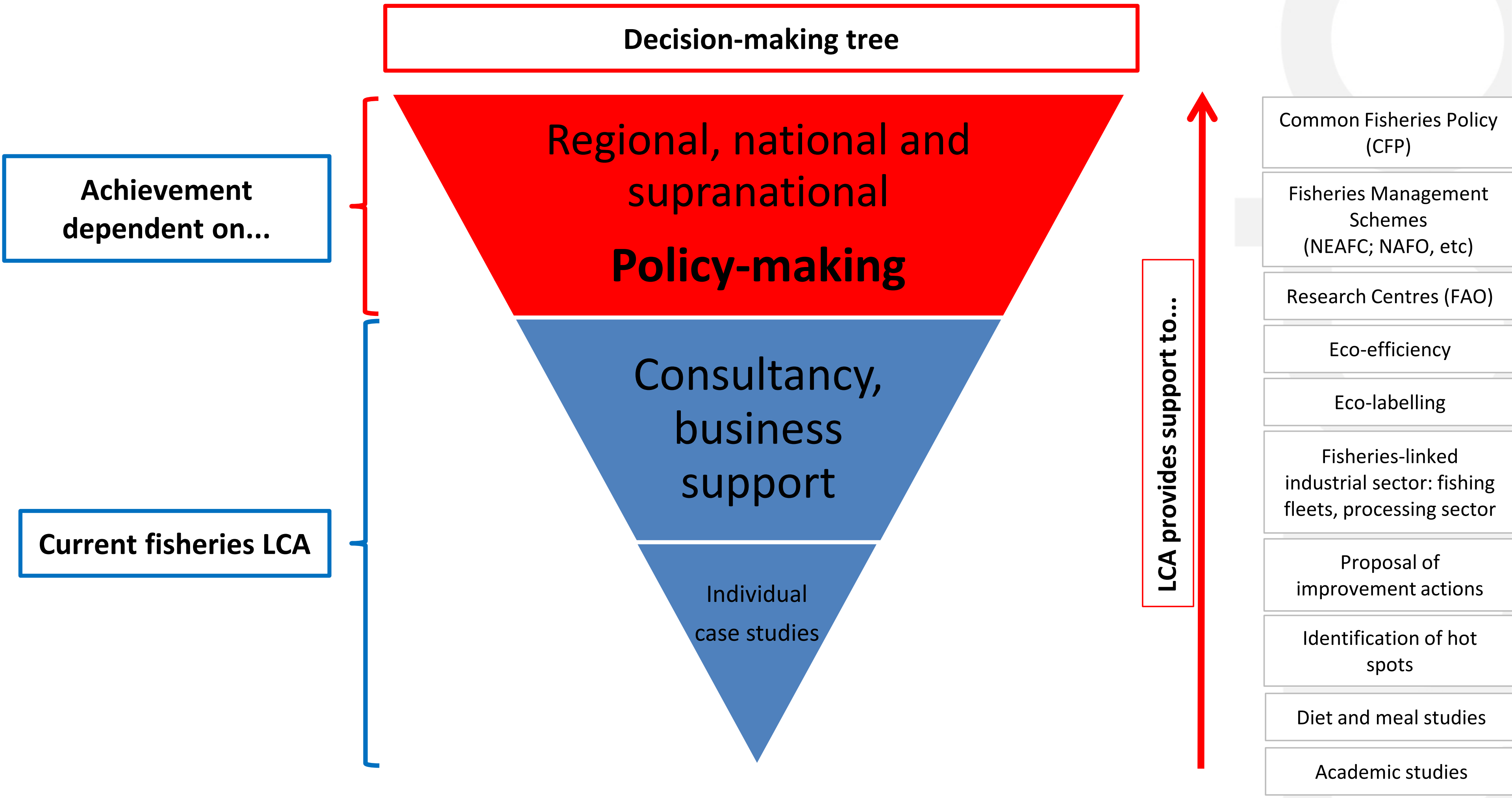
- CFP and LCA in context
- D1.2. Seafloor impacts on fishing.
- D1.4. Marine Resource Use. Overfishing.
- D1.4. Marine Resource Use. Primary production.
- D1.4. Marine Resource Use. By-catch impacts.

The Common Fisheries Policy. Utility of LCA



Source: http://www.bized.co.uk/educators/16-19/economics/markets/presentation/pricecontrol2_map.htm

Utility of LCA



LC-Impact: Deliverable 1.2. Seafloor impacts

- **Previous state-of-the-art:**
- A few publications had dealt with seafloor disturbance in fisheries LCA:
  - Methodology: Ziegler et al. (2003), Ziegler & Valentinsson (2007).
  - Case studies: Ziegler et al. (2011), Vázquez-Rowe et al. (2012a,b).
- Limitations:
  - Weak quantification of impacts (swept area per FU).
  - Weak link with current midpoint/endpoint perspectives.
- Current publication:

«Link spatial fishing effort data with vulnerability characterization layers»

LC-Impact: Deliverable 1.2. Seafloor impacts

- **Strengths:**
- ✓ Appropriate link of fishing effort to marine habitats.
- ✓ Detailed number of different coastal and marine habitats (>130).
- ✓ Allows inclusion of other impacts besides fishing that may occur on the seafloor (shellfish collection; constructions, etc).
- **Limitations:**
- ✓ Does not account for other impacts on seafloor (e.g shading).
- ✓ Actual quantification of different impacts of gears (e.g creels or trawls) remains unclear.
- **Future perspectives:**
- ✓ Langlois et al. (2011) → Sinergies? Coastal/intertidal habitats
- ✓ Endpoint categories.
- ✓ Quantification of impact in terms of potential destruction on seafloor.

LC-Impact: Deliverable 1.4. Overfishing

- **Aim of the study:**
- Include overfishing as a quantitative impact category "*to match fishery management needs*".

- **Strengths:**
- 1st step for LCA to improve its utility for policy-makers (CFP).
- Expands the scope of LCA assessment in fisheries.
- Provides 1st environmental management tool in which stock assessment is combined with other environmental dimensions.

- **Limitations:**
- Single stock based → Final perspectives in LCA should go beyond this perspective.
- Fish-oriented → Doesn't account for stock patterns of e.g. sessile organisms with different extraction patterns.
- Reproducibility → Annex with MSY data would be useful.
- Mass as unit of assessment.

LC-Impact: Deliverable 1.4. Overfishing

➢ **Mass as unit of assessment:**

1 t of European hake
- Trophic level ≈ 4.5
- High primary production rate

1 t of European pilchard
- Trophic level ≈ 3.0
- Medium primary production rate

The use of NPP values highlights the utility of mass within the current CFP framework, but fails to face the effort the ecosystem has performed in producing the assessed amount of biomass.

LC-Impact: Deliverable 1.4. Primary production

- **Aim of the study:**
- *Fishing down the food web* for a Swedish case study.
- Analysis on how MTL and PPR should be correctly interpreted.

- **Strengths:**
- Chronosequential analysis of MTL evolution in the Kattegat.
- Provides interesting elements to improve fisheries management.

- **Future perspectives:**
- Fishing down, fishing through, and increase to overfishing. These scenarios depend on specific chronosequences of fishery evolution over time (Foley et al., 2013).
- Inclusion of IUU and influence fishery management should be discussed.

LC-Impact: Deliverable 1.4. By-catch impacts

- **Aim of the study:**
- Evaluate utility of Swedish Red List linked to fisheries management advice.
- Apply the selected approach to attribute impacts from incidental catches to a specific seafood product.

- **Strengths:**
- Highlights importance of marine biodiversity vs. single stock assess.
- Provides method to assess endangered, depleted or rare species.

- **Limitations:**
- Data quality/availability may impair the usefulness of the method.
- Lack of clearness explaining the different concepts.
- Its utility for rare or strongly depleted species is interesting, but where do we fix the threshold to use this perspective.

LC-Impact: Deliverable 1.4. By-catch impacts

Discards. Discards, or discarded catch is that portion of the total organic material of animal origin in the catch, which is thrown away, or dumped at sea for whatever reason. It does not include plant materials and post harvest waste such as offal. The discards may be dead, or alive.

Bycatch. Bycatch is the total catch of non-target animals. Discards are not a subset of bycatch as the target species is often discarded.

Incidental catch. The term ‘incidental catch’ is used in the context of rare incidents or events such as catches of marine mammals, turtles, or seabirds. Incidental catch is generally expressed in numerical terms rather than in terms of weight.

All definitions taken from FAO technical reports.

tudor
PUBLIC RESEARCH CENTRE HENRI TUDOR

Thank you for your attention !

Dr. Ian Vázquez-Rowe
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Resource Centre for Environmental Technologies
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LC-Impact review

Deliverable 1.4 (marine resources)

Deliverable 1.2, Task 6 (seafloor impact)

The Reviewer

- Ole Ritzau Eigaard, Researcher at the Danish National Institute of Aquatic Resources
- Phd in fisheries technology and management from Wageningen University
- Master of Science in biology from Copenhagen University

Deliverable 1.4 objectives

“to take product-related quantification of the biological impact of fishing a step further than state of the art at the beginning of the project”

Eight new impact categories defined and/or investigated within four groups

Group 1 (target species/resource categories):
Wasted potential yield (WPY), F-overfishing and B-overfishedness

Group 2 (bycatch categories):
Red list Index (RLI) and Vulnerable, Endangered and Critically endangered (VEC) discard to catch proportion,

Group 3 (seafloor integrity categories):
Seafloor impact category (defined by affected area*restoration time)

Group 4 (food web categories):
Mean Trophic Level (MTL) and Primary Production Required (PPR).

Wasted Potentail yield, F-overfishing, B-overfishing

- Strengths
  - Builds a strong bridge to Fisheries management
  - Politically relevant and very integrative

- Weaknesses
  - Vulnerable to high uncertainty, variability and availability of input data
  - WPY Methodology is complex and it’s scientific robustness is difficult to assess

Suggested improvements for WPY

1. Be clear about differences between F defintion in surplus production models and cohort based stock assessments such as VPAs
2. Revise/elaborate defintion of fishing mortality in deliverable (footnote page 10)
3. Revise equation 1 in deliverable according to point 2 above
4. Be very careful in giving definitions and limitations of WPY expression, e.g. that WPY is only an index/proxy of potentially wasted yield and how it would deal with: i) different F-types according to point 1 above, ii) large uncertainties in reference point estimations, iii) species specific differences in stock charachteristics such as stock-recruiment relationships and growth paramters.

Red List index (RLI) and Vulnerable & endangered species categories (VEC)

- Strengths
  - Simple approach
  - Scientifically sound
  - Easy to communicate

- Weaknesses
  - Vulnerable to the generally low coverage and reliability of discard monitoring and estimates (perhaps Fully documented fisheries may improve this)
  - Definitions of bycatch and discards need to be improved

Mean Trophic level (MTL) and Primary production required (PPR)

- Strengths
  - Easy to communicate
  - Integrative

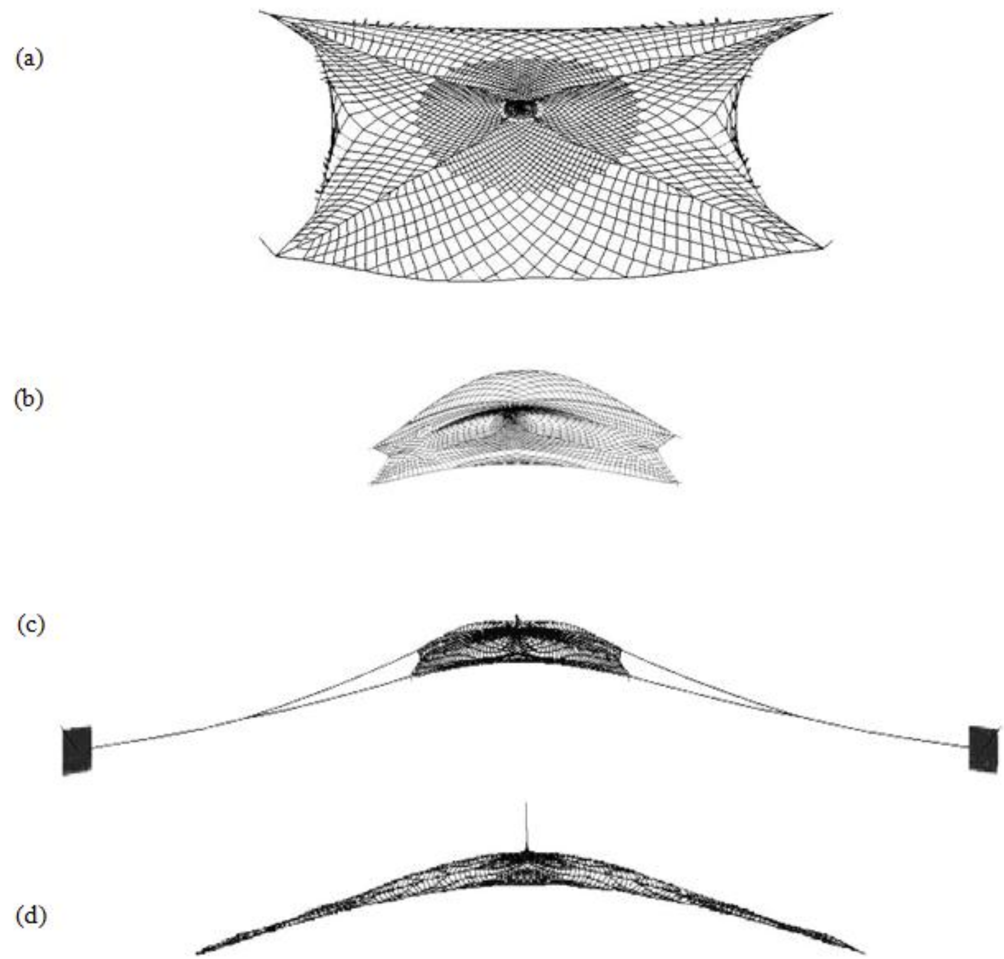
- Weaknesses
  - Requires very broad assumptions to be fulfilled
  - Very large risk of confounding with other drivers of MTL such as technological development and management regulations

Sea floor impact category

- Strengths
  - Easy to communicate
  - High relevance to MSFD

- Weaknesses
  - In its present form relatively crude assumptions are required on the gear sea bed interactions (area and severity of gear impact)

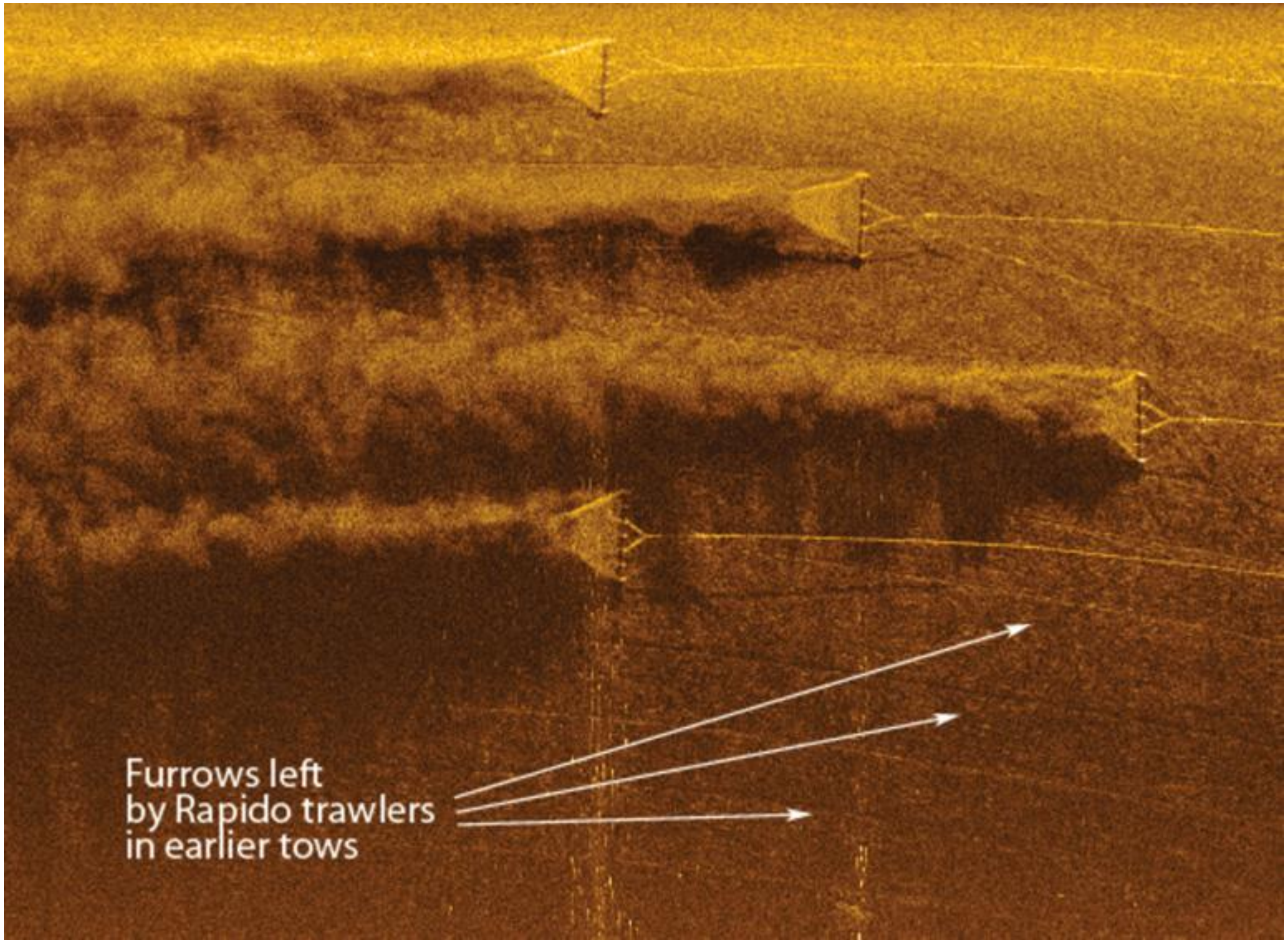
An OTB is not just an OTB, and seafloor area affected varies greatly with species targeted



The severity of Sea Floor impact is not straight forward to assess across different trawl fisheries

Pelagic doors in demersal trawling

A lot of ongoing research to better estimate seafloor impact



Suggestions to expand the scope of Seafood LCA methodology

- Investigate the possibilities of including impact categories of **social** and **economic sustainability** into Seafood LCAs vis a vis current efforts (e.g. in the CFP reform) of developing an ecosystem based approach to fisheries management.
- Include **fuel comsumption** and **emissions of greenhouse gasses** in fisheries

General thoughts in relation to the future of Seafood LCAs

- One major challenge is the poor reliability and/or coverage of many input data (e.g. discards, fishing effort descriptors, biological reference points, etc.)

- A second point of concern is whether standard LCA framework, being such a well established methodology within non-renewable ressource economy, is actually well suited for reducing the enormous variability of renewable marine resources to deliver simple and meaningful impact assessments and trade-offs suitable for decision making

- But, there are methodological benefits of the LCA framework, and it is not impossible that seafood LCAs can potentially become and integrated part of fisheries management

- Keep improving impact categories and put faith in number of ongoing efforts to improve monitoring, data collection and estimation of the environmental impact of fisheries and (e.g. FP7-BENTHIS, MYFISH, SENSORS, etc)

Review of

LC-IMPACT

D1.2 Land use – 6 November 2012

Dr. Ottar Michelsen
The Industrial Ecology Programme
NTNU – Norwegian University of Science and Technology

Brief summary

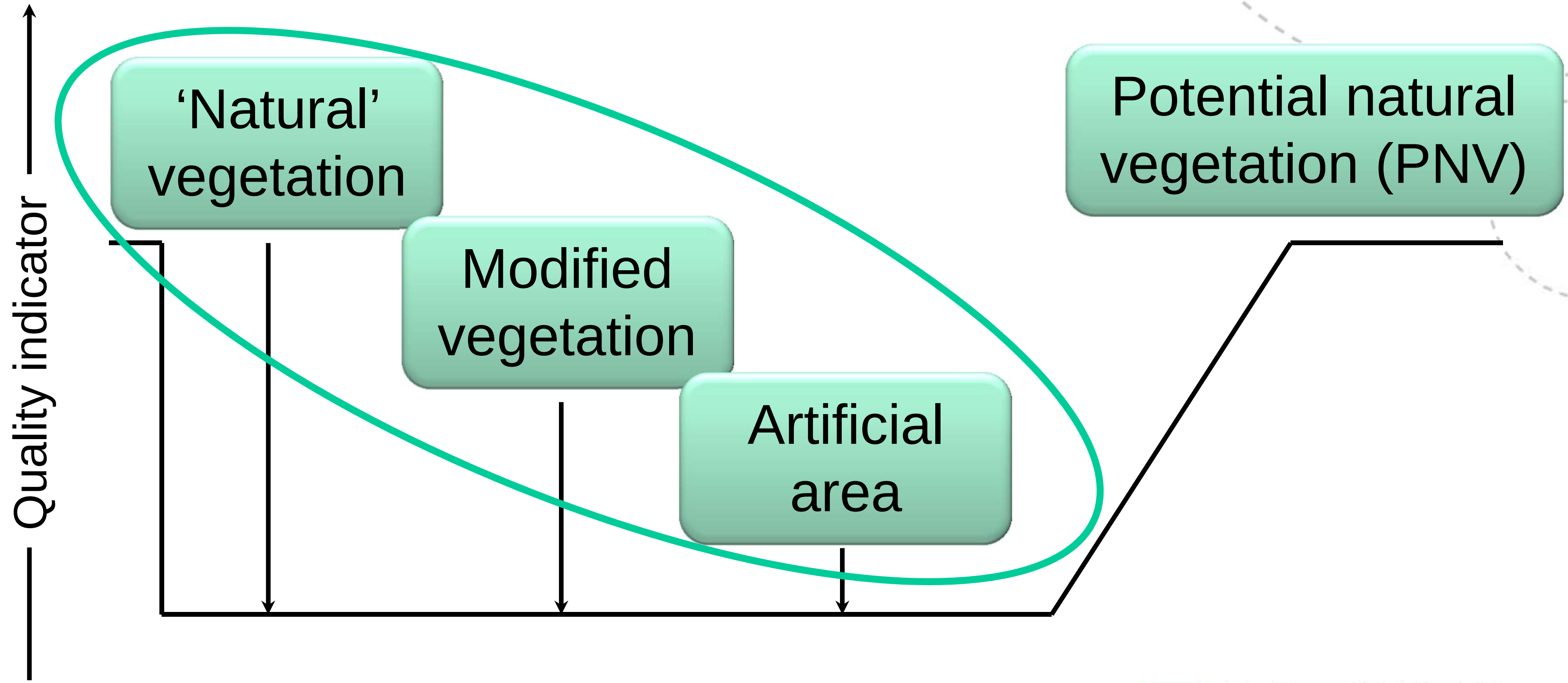
- ‘The goal is to develop operational and scientifically sound methods for the assessment of land use on ecosystem services, biodiversity and human health’
- (Species) diversity
  - 14 (9) biomes, 8 land use types
- Functional diversity
- Climate impact from changes in logging rates
- Soil erosion
- (Seafloor impacts from fishing)

Completeness

- How many impacts from LULUC should be considered before an assessment is ‘complete’?
- What is the reference situation?
  - Potential vegetation for biodiversity vs present harvest level
  - Relative (biodiversity) vs absolute (soil depth) changes
- Double counting?
  - As long as changes in species composition is assessed following a change in land use, how to separate between impact from (physical) land use changes and chemical impact normally considered as stressors on their own

NTNU
Innovation and Creativity

Reference situation



Environmental relevance (I)

- (Vascular plant) species diversity as indicator for biodiversity
  - Chap. 1 shows clearly challenges – different taxa responds differently, what do we actually measure
  - 8 land use types/14 biomes – still data deficit and large within biome variation
  - Number of species – what about invasive species, edge effects in fragmented areas (and artificial areas as shown)
  - So – is species diversity a useful indicator for biodiversity?
  - Not clear how closely sampling points and (natural/semi natural) reference points are related (as an example, in table 2.7 'forested riverbank' is used as reference, not all included datasets are potentially a forested riverbank?)

Environmental relevance (II)

- Ecosystem functions
  - But what functions are assessed
  - A focus on functions should (in theory) identify some redundancy in species, still some CFs are higher for FD than SR
- Climate impacts from changes in logging
  - Only changes in carbon stocks are addressed, also impacts from changes in evapotranspiration and albedo should be considered
  - I miss the actual data on changes in greenhouse gasses

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Average vs worst case

- Average data masks results, particularly shown in the chapters 3 and 4
- Should average data be given as default values when actual data might indicate higher impact?
- Worst case as default, practitioners must provide real data if lower values are used

Applicability

- Chapter 1 – improvement of already existing methods on biodiversity – CFs might be used as are (with some cautions…)
- For the other methods – applicability must be shown in case studies

Suggestions

- Some general questions should be addressed
  - Reference situation
  - Number of impacts from LULUC in LCA?
- More focus on actual implications
  - What is actually assessed; what is ‘functional diversity’
  - How should the scores in chapter 3 and 5 be used – do the results in chapter 3 indicate the need of a relocation of logging?
- Case studies to show applicability

WP3 – Expert Review

November 6th 2012 - Brussles

Authors:
Anna Kounina, Henrik Fred Larsen, Joachim Roos, Ligia B. Azevedo, Mark. A. J. Huijbregts, Manuele Margni, Michael Zwicky Hauschild, Nuno Miguel Dias Cosme, Olivier Jolliet, Philipp Preiss, Rainer Friedrich, Reinout Heijungs, Rosalie van Zelm, Sebastien Humbert, Sandra Torras, Stefano Cucurachi, Thomas M. W. J. van Goethem

<u>Many thanks to the reviers</u>
Jyri Sepalla (Aci + Eutro)
Jaap Struijs (Aci + Eutro)
Marko Tainio (Fine particulate matter + ozone)
Martijn Schaap(Fine Particulate matter+ ozone)
Enrico Benetto (Noise)

10.00 – 10.30	**Welcome , Highlights and cross cutting issues**		Mark Huijbregts
10.30	**WP1 Resources**	**WP2 Toxicity**	**WP3 Other impacts**
10.30 – 11.30	Land	Metals	Eutrophication
11.30 – 12.30	Water	Whole Effluents	Acidification
12.30 – 13.30	**Lunch**		
13.30 – 14.30	Marine resources	Organic chemicals	Fine particulate matter + ozone
14.30 – 15.30	Metals + Fossils	Organic chemicals	Noise
15.30 – 16.00	Summarize key issues	Summarize key issues	Summarize key issues
16.00 – 16.30	**Coffee break**		
16.30 – 16.40	Main outcomes WP 1		Stefanie Hellweg
16.40 – 16.50	Main outcomes WP 2		Ralph Rosenbaum
16.50 – 17.00	Main outcomes WP 3		Philipp Preiss
17.00	Closure		Mark Huijbregts

Plan for WP3 Session

- Presentation of Task Leader and Experts (20 Min.)
- Read trough the comments of the experts
- Discussion on basis of main points
- Summarize the key outcomes/ decisions together with the group
- Make sure minutes are made – **Task leader self and one backup T1: Nuno / Ligia; T2: Anna / Ligia T3: Philipp / Anna & Thomas; T4 Stefano / Reinout**
- Philipp: Present the key outcomes/ decisions in the main session in the afternoon (10 minutes!)
- Manage the time !

Deliverables WP3 for M33

- D3.2: Recommended assessment framework, method and characterisation factors for **ecosystem impacts of eutrophying** emissions: phase 2 (report, model and factors); (Task 3.1: DTU, RU)
- D3.3: Recommended assessment framework, method and characterisation factors for **ecosystem impacts of acidifying** emissions: phase 2 (report, model and factors); (Task 3.2: Quantis, USTUTT, RU)

- D3.4: Recommended assessment framework, method and characterisation factors for **human health impacts of fine particulate matter formation**: phase 2 (report, model and factors)
- D3.5: Recommended assessment framework, method and characterisation factors **for human health and ecosystem impacts of photochemical ozone formation**: phase 2 (report, model and factors)
- D3.6: Recommended assessment framework, method and characterisation factors for **noise impacts**: phase 2 (report, model and factors); (Task 3.4: CML, USTUTT)

Summary Key Issues

Incompleteness = 1 ; Complete = 5								
	D3.2_A freshwater	*D3.2_A marine*	*D3.2_B freshwater*	*D3.2_B marine*	*D3.3*	*D3.4&5 Schaap*	*D3.4 Marko Tainio*	*D3.6*
Completeness of scope	4	4	4	4	4	4	5	4
Environmental relevance	5	5	**3**	**3**	**3.5**	5	5	5
Scientific robustness & Certainty	4	**3**	**3**	**3**	**3**	**3**	4	4
Documentation & Reproducibility	5	**3**	4	4	4.5	4	**3**	**3.5**
Applicability	5	5	**3**	4	**3.5**	4	5	4
Suggestions for improvement								

Summary General Issues

- Illustrate how the results of the new methodologies differ from the results calculated using previous methods
- The validation status of the reduction simulations for secondary air pollutants SIA and ozone is limited.
- If two or more new recommendations: which one?
- Robustness of primary data (emission inventory; CTM (chemistry, dispersion etc.);
- Terminology has to be defined unambigious

Thank you for Your Attention!

Project Task 3.1: Aquatic eutrophication

Experts meeting

November 6th 2012 - Brussels

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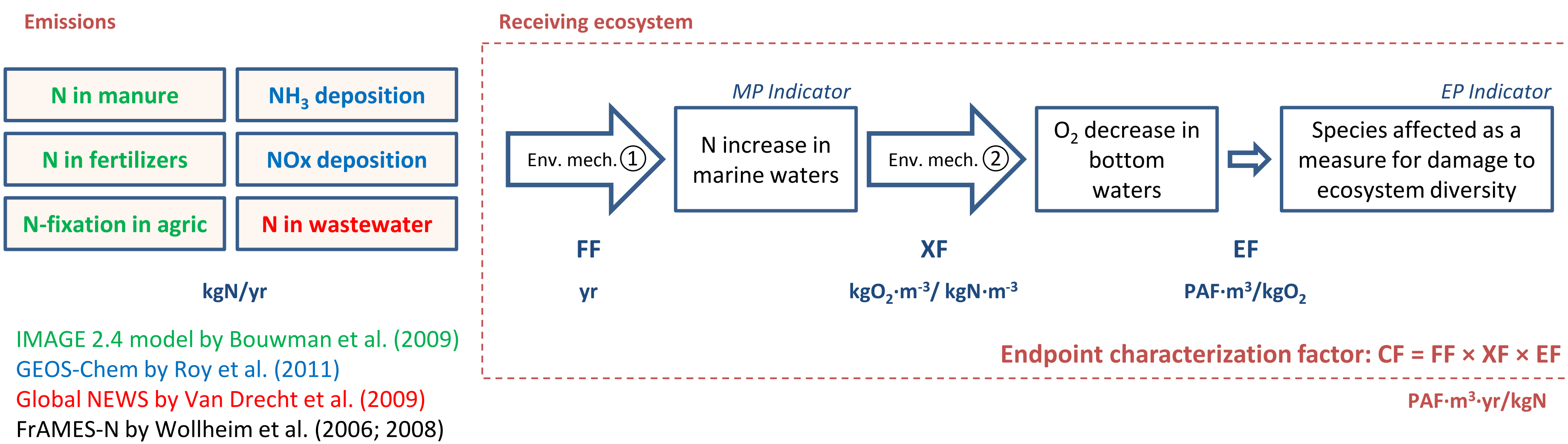
Marine eutrophication - Model framework



- **Fate** modelling:
  - River-N fate models (from anthropogenic emission sources to export to marine waters)
  - Marine-N fate modelling (fate of nitrogen in the marine compartment)
- **Exposure** modelling (intermediate link from fate to effects, relating photic zone processes with bottom layer processes)
- **Effect** modelling (processes leading to impacts on biota)

Marine eutrophication

The framework is used to define the **Characterisation Factor** (CF, unit: PAF ·m³·yr/kgN): $CF_{ij} = FF_{ij} \times XF_j \times EF_j$

- FF_{ij} is the **Fate Factor** (unit: yr) for emission route *i* to receiving ecosystem *j*

$$FF_{i,j} = \frac{N_{input\ \ i,j}}{N_{emitted\ \ i}} / \lambda_j \qquad \lambda_{LME} = \lambda_{denitr} + \lambda_{sed} + \lambda_{adv\ LME}$$

- XF_j is the **Exposure Factor** (unit: $kgO_2 \cdot m^{-3}$/ $kgN \cdot m^{-3}$) in receiving ecosystem *j*

$$XF_j = OM{:}N\ ratio \times O_2{:}OM\ ratio \times NIE_j \times Vol.Correction\ Coef.$$

$$NIE = \frac{EmpN_{input}}{TheorN_{input}} \qquad EmpN_{input} = \frac{DIN}{DIN\ content\ in\ N_{tot}} \times M_N \times A_{LME} \qquad DIN = 10^{(logPP - 2.332)/0.442}$$

$$TheorN_{input} = PP \times \frac{M_C}{M_N} \times A_{LME}$$

- EF_j is the **Effect Factor** (unit: $PAF \cdot m^3/kgO_2$) in receiving ecosystem *j*

$$EF = \frac{\Delta PAF}{\Delta[O_2]} = \frac{0.5}{HC_{50}} \qquad HC_{50} = 10^{avg(log EC_{50})}$$

Marine eutrophication

Sensitivity

Contribution of input parameters to the CF

$$SR_X = \frac{(CF_{end} - CF_{start})/CF_{start}}{(X_{end} - X_{start})/X_{start}}$$

SR ($N_{input}/N_{emitted}$)	
Min	Max
1.000	1.000
same result for all Country-to-LME	

SR (sed)	
Min	Max
-0.002	-0.124
sensitivity correlated with RT	

SR (denitr)	
Min	Max
-0.013	-0.946
sensitivity correlated with RT	

SR (RT)	
Min	Max
0.024	0.987
sensitivity correlated with RT	

High sensitivity

SR (PP)	
Min	Max
1.279	1.279
same result for all Country-to-LME	

SR (VCC)	
Min	Max
1.000	1.000
same result for all Country-to-LME	

SR (HC_{50})	
Min	Max
-0.909	-0.909
same result for all Country-to-LME	

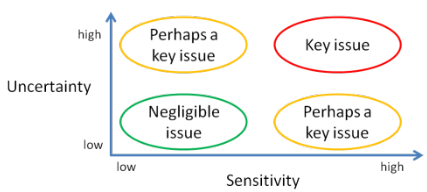
Uncertainty

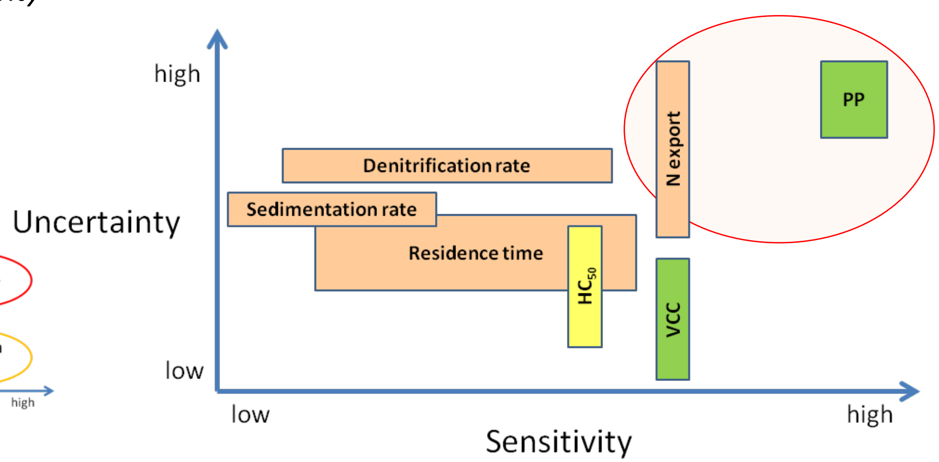
Extreme values for
N-export splitting rule
Range: 0-1 ord.magn.

Extreme values for
marine-N loss rate
(best-worst case scenarios)
Range: 0-1 ord.magn.

Key issues

Combining sensitivity and uncertainty





Marine eutrophication

Endpoint CFs available for:

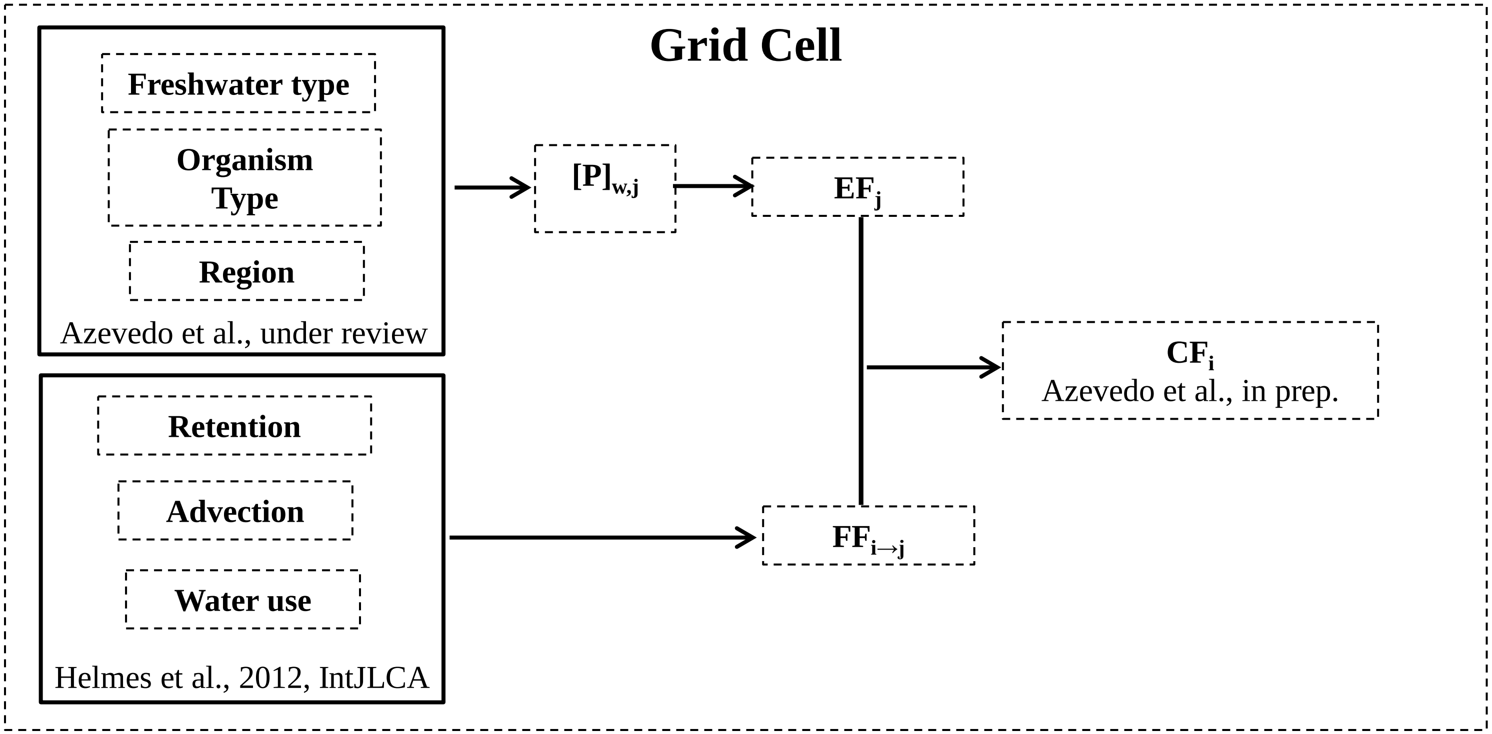
- 214 country-to-LME combinations * 6 N emission routes
- 143 countries * 6 N-emission routes
- Site-generic at region/continent and global default scale

- Spatial differentiation with 2 ord.magn.
- NFs for the same 143 countries * 6 emission routes (from 2005 data)
- NFs aggregated in regions/continents and a global default

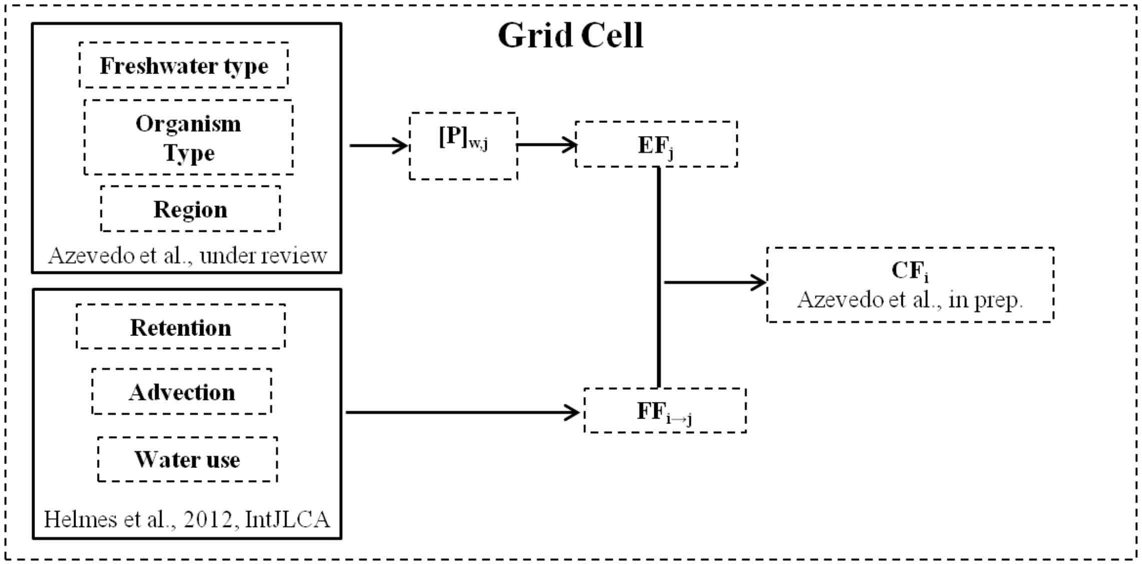
Weaknesses:

- Dependency on third-party models (emissions, deposition)
- Unknown uncertainty associated with these ‘input’ models
- No spatial differentiation for marine sedimentation and denitrification rates

Freshwater eutrophication



Freshwater eutrophication



Method description:

- 0.5°x 0.5° resolution (both midpoint and endpoint)
- Spatial detail of CF: grid, country, and continent
- Stressor: phosphorus (as total P)
- EF: Potentially not occurring fraction of species
- EF types: Linear, marginal, and average change

Pros:

- Model spatial resolution
- Additional P transport pathways
- Spatial resolution of the reported CFs
- Comparison of multiple EF types
- Effect based on more ecological compartments

Cons:

- Not all ecological compartments are available worldwide
- Field monitoring of P concentrations (used in marginal and average EF) are scarce outside EU
- Limited emission data from point and non-point sources

Thank you for your attention!

References

Bouwman et al. 2009. *Global Biogeochem. Cycles* **23**, GB0A04, doi:10.1029/2009GB003576.
Helmes et al. 2012. *Int J Life Cycle Assess* **17**: 646-654
JRC. 2011. *EUR 24571 EN*. Publications Office of the European Union, Luxemburg.
Roy et al. 2011. *Submitted to Atmospheric Environment*, September 22.
Struijs et al. 2009. in: Goedkoop et al. (Eds.) *Aquatic Eutrophication*.
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Struijs et al. 2011b. *Integr Environ Ass Manage* **7**: 280–286.
Van Drecht et al. 2009. *Global Biogeochem. Cycles* **23**, GB0A03, doi:10.1029/2009GB003458.
Wollheim et al. 2006. *Geophysical Research Letters* **33**, L06410, doi:10.1029/2006GL025845.
Wollheim et al. 2008. *Global Biogeochem. Cycles* **22**, GB2026, doi:10.1029/2007GB002963.

Spatially-explicit midpoint and endpoint indicators at the global scale for terrestrial acidification

M 33 deliverable summary

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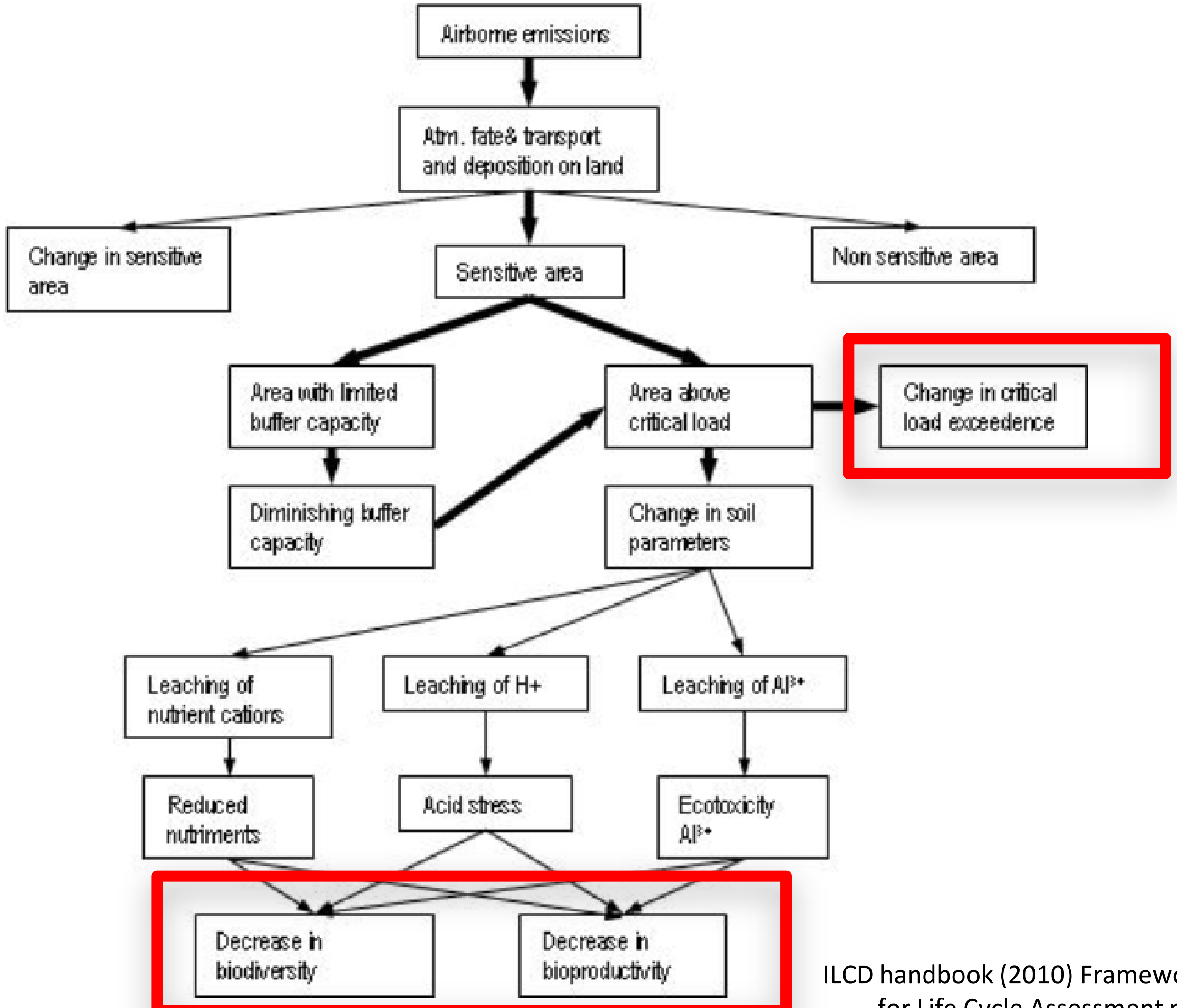
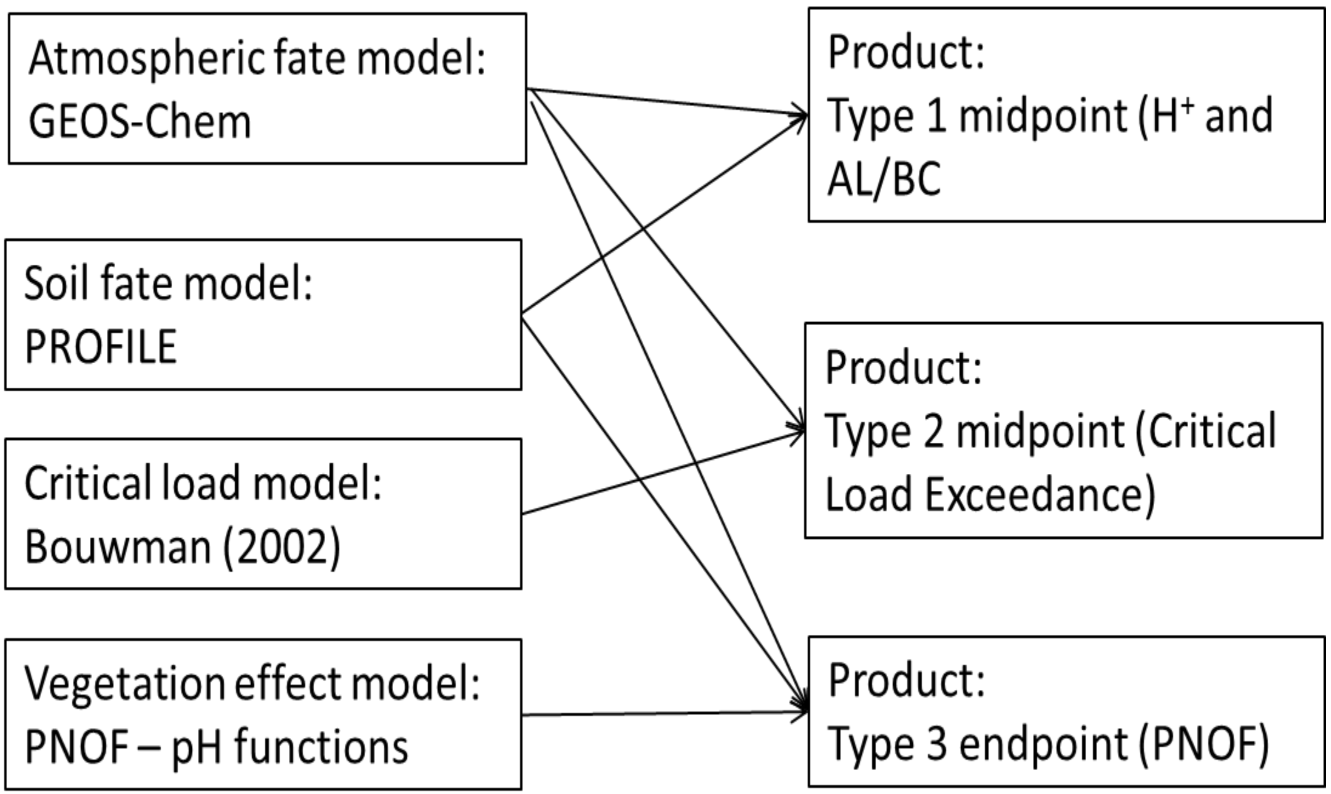
Quantis

Environmental mechanism



ILCD handbook (2010) Framework and requirements for Life Cycle Assessment models and indicators

GEOS-Chem, PROFILE, critical load and PNOF – pH models are used to derive Type 1, 2 and 3 results



Characterization models

Type 1

$$CF_{i,p} = \sum_{j} (FF_{i,j,p} \times SF_{j,p})$$

Type 2

$$CF_{i,p} = \sum\nolimits_{j} (FF_{i,j,p} \times t_{meq\_p} \times ER_j)$$

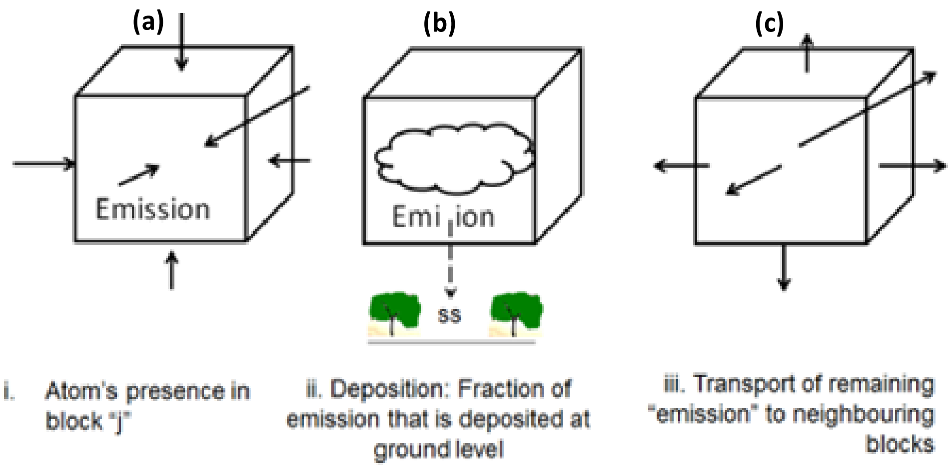
Type 3

$$CF_{i,p} = \sum_{j} (FF_{i,j,p} \times SF_{j,p} \times EF_j)$$

- **FF**: fate factors
- **SF**: soil sensitivity factor
- **ER**: criticality exceedance ratio
- $\mathbf{t_{meq\_p}}$: transfer term to convert units
- **EF**: vegetation effect factor
- ***p***: the pollutant (NO_x, NH_x, or SO_2) emitted in location *i* and deposited in location *j*

Roy et al. (Submitted) Global spatially-explicit characterisation factors for terrestrial acidification: the influence of atmospheric fate, soil sensitivity and plant species response.
Roy et al. (2012) Life Cycle Impact Assessment of Terrestrial Acidification: Modeling Spatially Explicit Soil Sensitivity at the Global Scale. Environmental Science and Technology 46 (15):8270–8278

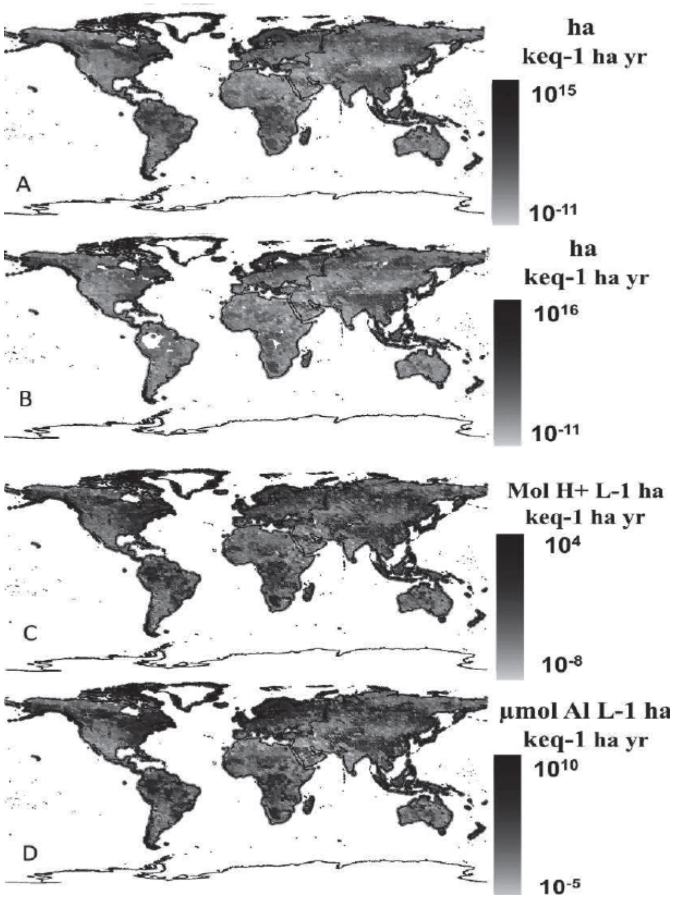
Atmospheric fate factors



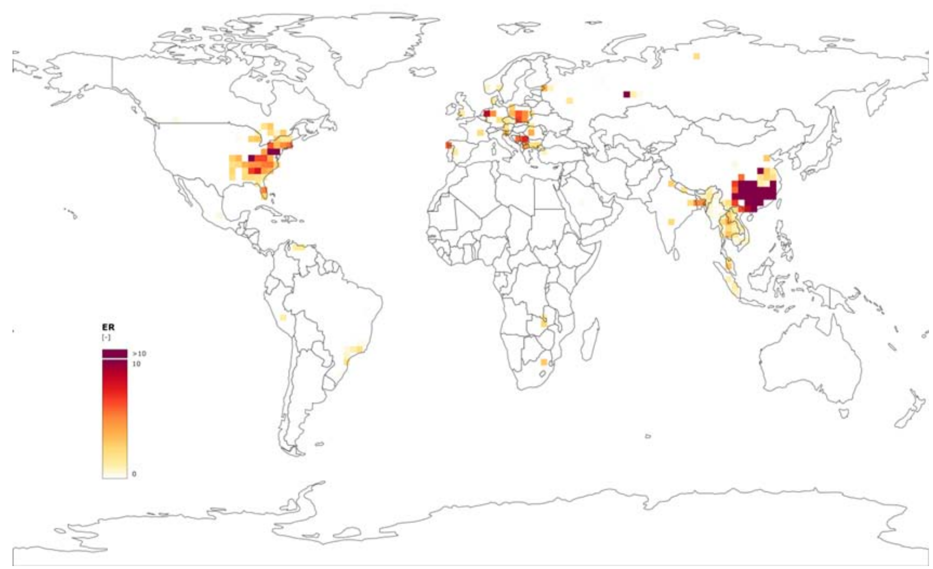
$$(In_i + Em_i + Em'_i) - \left(Out_i + Dep_i + Acc_i\right) = 0$$

Roy et al. (2012) Spatially-differentiated atmospheric source-receptor relationships for nitrogen oxides, sulfur oxides and ammonia emissions at the global scale for life cycle impact assessment. Atmospheric Environment In press

Soil sensitivity factors and critical load exceedance



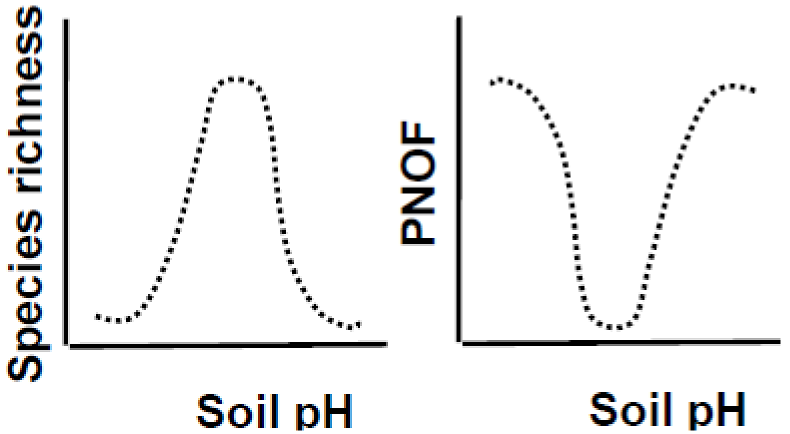
Worldwide SFs due to S from SO2-SO4 emissions-related deposition based on the (A) BC/Al, (B) Al/Ca, (C) pH, and (D) Al soil chemical indicator (Roy et al. 2012)



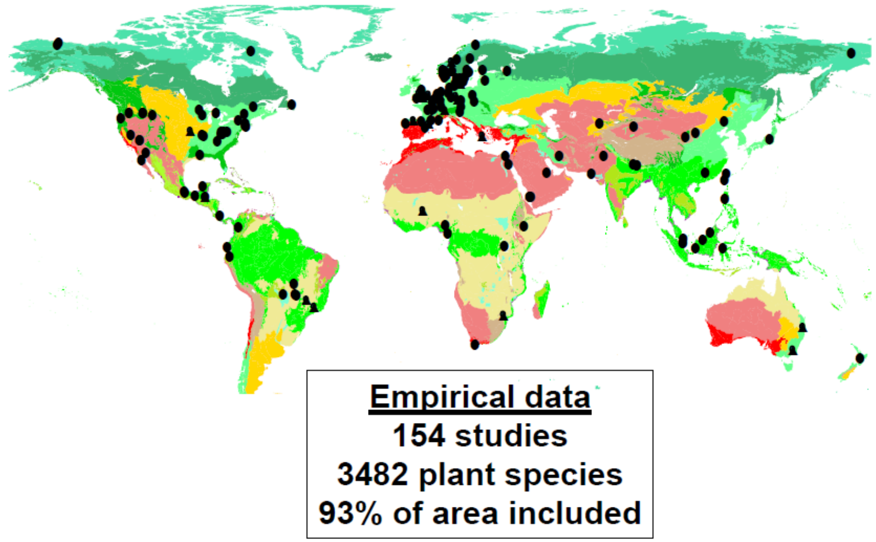
Exceedance ratio (ER_j) on a 2°×2.5° resolution

Ecological effect factors

$$ePNOF_{i,b} = 1 - \frac{S_{i,b}}{S_{opt,b}}$$

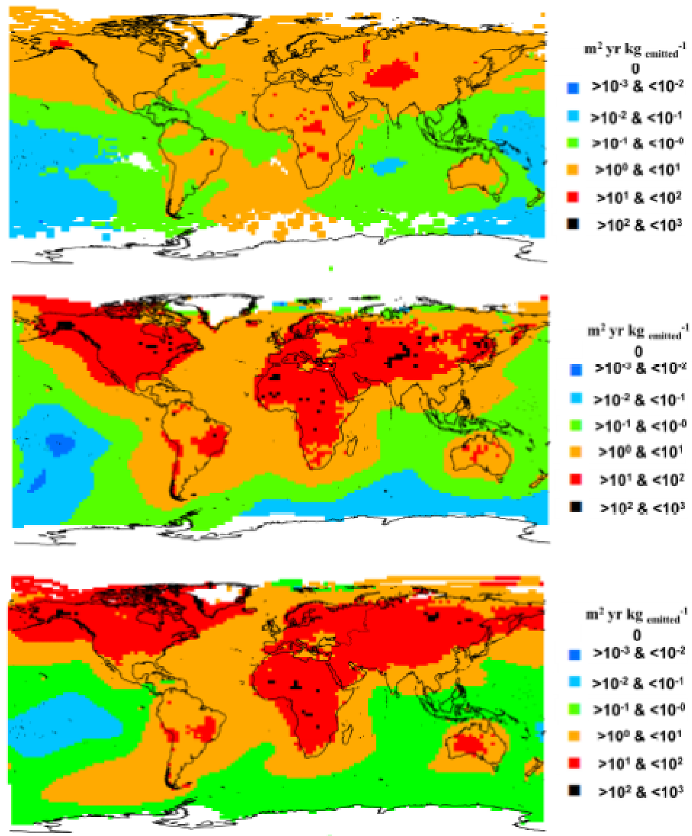


- **ePNOF:** empirical Potential Not Occurring Fraction
- **$S_{i,b}$:** species richness at soil pH i in biome b
- **$S_{opt,b}$:** species richness at optimum soil pH of biome b



Azavedo et al. (submitted) Biome-specific response relationships of terrestrial plant species richness and pH at the global scale. Global Ecology and Biogeography

Results: endpoint characterization factors



2x2.5 resolution endpoint [$m^2 \times yr \times kg\ emitted^{-1}$] characterization factors for an emission of NO_x, NH_3 and SO_2 emissions, respectively

Roy et al. 2012

Interpretation

- **High impacts to soil:** high latitude areas of Canada, Scandinavia, and Eastern Russia:
  - important SO2, NH3 and NOX emissions
  - low acid neutralizing capacity (ANC) of these soils

- **High impact on terrestrial species:** North America, Africa, western and northern Europe and central Asia
  - sensitive ecosystems such as the boreal forests of Canada and Scandinavia

- **Difference between critical load and soil sensitivity modelling approach**:
  - sensitivity classes:
    - CEC and base saturation for critical load approach
    - pH, Al+ and BC, silicate weathering as well as biological processes for soil sensitivity modelling approach
  - Inventory modelling approach:
    - background emissions for critical load approach
    - marginal change in emissions (following a 10% increase in the emissions) in soil sensitivity modelling approach (Roy et al. 2012)
  - A large SF may be caused by a significant change in indicators values, a large area and/or a very small deposition value for sensitivity modelling (Roy et al. 2012)

THANK YOU FOR YOUR ATTENTION

Key issues and findings
- freshwater eutrophication

Jyri Seppälä, SYKE

LC-impact meeting 6.11.2012

SYKE

General aspects

- A clear improvement on an earlier situation: both midpoint and endpoint approaches in lakes and rivers, CFs for countries and continents
- The work is well written and documented. For experts, it is easy to read and follow the idea

- Further analysis and discussion of the reliability and applicability of the methodology is required in the next steps in order to make final conclusions

Environmental relevance and Scientific robustness & Certainty

- The meaning of resolution - the local and regional character of effects

- Scientific bases on the determination of CFs – potential impacts and damage orientated approaches

How to derive CFs?

-
- Azevedo et. al. 2012:
  - $CF_{i,s,w} = \sum_j (FF_{i \to j} \cdot EF_{s,w,j})$
  - $FF_{i \to j}$ is the partial fate factor for emission grid i to downstream grid j
  - effect factors ($EF_{s,w,j}$) are determined on the basis of PNOF of species s depending on $C_{w,j}$ in the freshwater w of grid j. Thus, $EF_{s,w,j}=f_{s,w,j}(C_{w,j})$
  - three types of effect factors (LEF, MEF, AEF)
- A more appropriate approach ?
  - Damage on species s, $D_{s,w} = f_{s,w,1}(L_{w,1}, \ldots) + \ldots + f_{s,w,n}(L_{w,n}, \ldots)$
    where $L_{w,i}$ = external loading of freshwater w in grid i
  - $CF_{i,s,w} = \frac{\Delta D_{s,w}}{\Delta L_{w,i}}$

SYKE

Applicability

- The proposition: one midpoint method and three endpoint methods for two organisms (autotrophs and heterotrophs) and for two freshwater types (lakes and streams)
  - all for European countries
- Alternative results can be produced but can they be prioritized in order to draw final conclusions?
- How to use the recommended CFs and how to interpret their results?

SYKE

Key issues and findings - marine eutrophication

Prof. Jyri Seppälä, SYKE

LC-impact meeting 6.11.2012

SYKE

General aspects

- A clear improvement on an earlier situation: CFs for countries and continents
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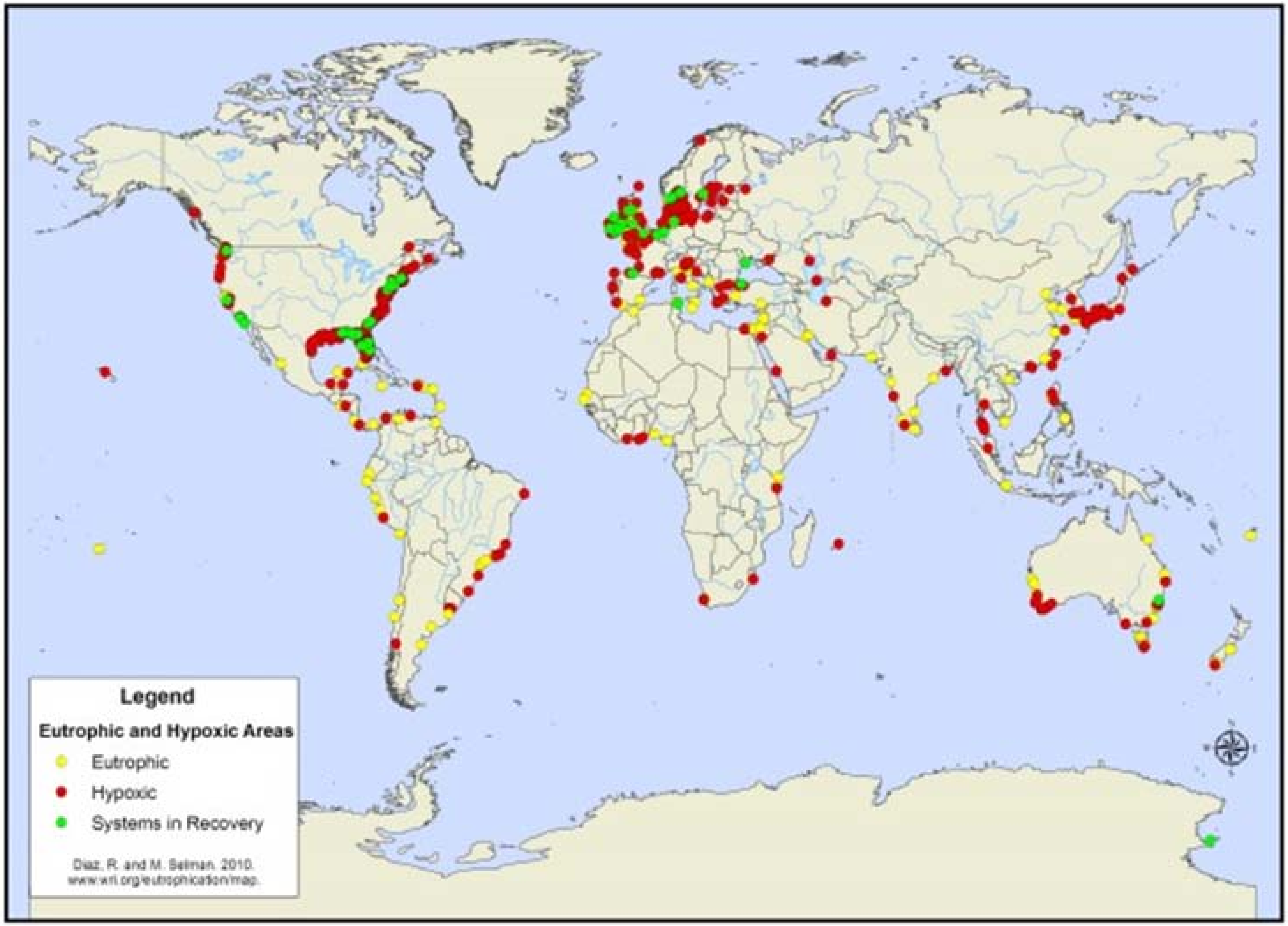
SYKE

Environmental relevance and Scientific robustness & Certainty

- P has also
  - direct contribution to marine eutrophication in many costal areas (e.g. Gulf of Mexico, Baltic Sea)
  - indirect contribution to organic loading from freshwater eutrophication
- All airborne N deposition has been taken into account in marine areas – overestimation of airborne N
- The meaning of resolution - hot spots in different marine areas
- Scientific bases on the determination of CFs – potential impacts and damage orientated approaches

World Hypoxic and Eutrophic Coastal Areas



SYKE

- A more appropriate approach ?
  - Damage, D = f_1 (L_1, …) +….+ f_n (L_n, …)
    where L_i = external loading of grid i
  - $CF_i = \frac{\Delta D}{\Delta L_i}$

SYKE

Key issues and findings
- marine eutrophication

Jyri Seppälä, SYKE

LC -impact meeting 6.11.2012

SYKE

- A clear improvement on an earlier situation: both midpoint and endpoint approaches, CFs for countries and continents
- The work is well written and documented. For experts, it is easy to read and follow the idea
- Further analysis and discussion of the reliability and applicability of the methodology is required in the next steps in order to make final conclusions

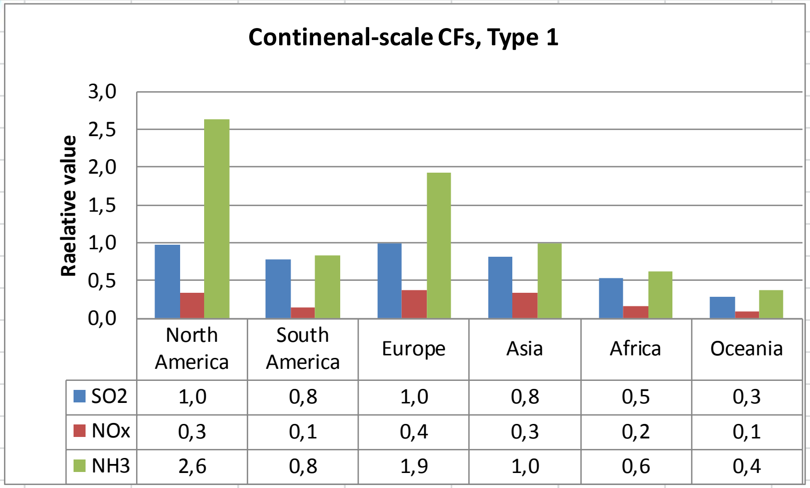
Environmental relevance and Scientific robustness & Certainty

- The meaning of resolution - grid-specific or country-specific CFs?

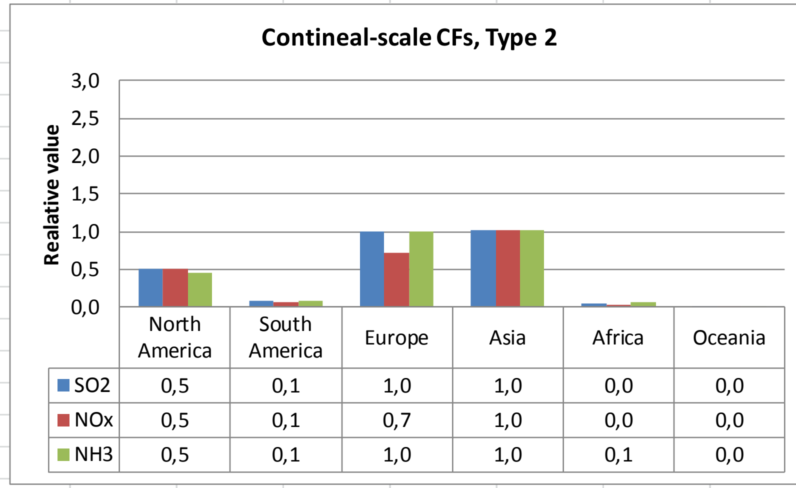
- Scientific bases on the determination of CFs
  – midpoint approaches: Type 1 VS. Type 2
  - Type 2 ≠ Accumulated exeedance (AE) method

SYKE

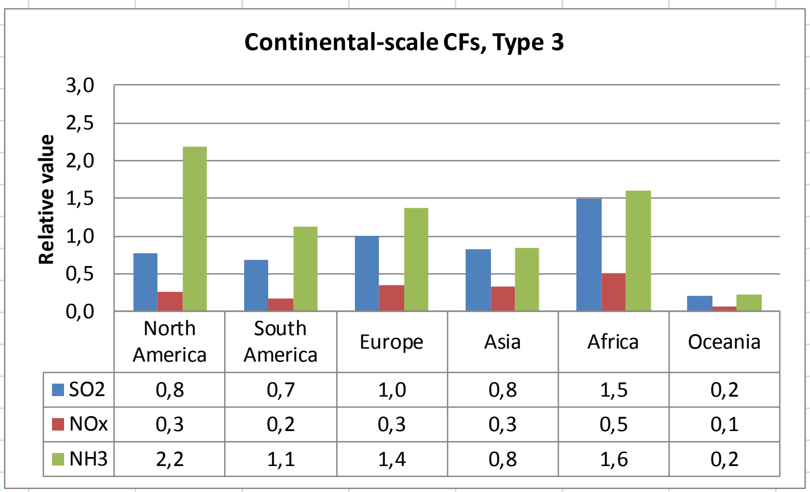
CF for European SO2 is 1 in the figures



	North America	South America	Europe	Asia	Africa	Oceania
SO2	1,0	0,8	1,0	0,8	0,5	0,3
NOx	0,3	0,1	0,4	0,3	0,2	0,1
NH3	2,6	0,8	1,9	1,0	0,6	0,4



	North America	South America	Europe	Asia	Africa	Oceania
SO2	0,5	0,1	1,0	1,0	0,0	0,0
NOx	0,5	0,1	0,7	1,0	0,0	0,0
NH3	0,5	0,1	1,0	1,0	0,1	0,0



	North America	South America	Europe	Asia	Africa	Oceania
SO2	0,8	0,7	1,0	0,8	1,5	0,2
NOx	0,3	0,2	0,3	0,3	0,5	0,1
NH3	2,2	1,1	1,4	0,8	1,6	0,2

SYKE

Recommendations

- Midpoint approaches:
  - Type 1 is better than Type 2 ? (I can agree)
  - Type 1 is better than corrected Type 2b ? (based on the idea of AE) (I do not know)
- Midpoint VS endpoint (Type 3)
  - Huge uncertainty in Type 3
  - Could we accept the acidifying impacts in Africa compared to the impacts in Europe and Asia ?
- Could we recommend that the new developed global method (with greater uncertainty in terms of air-transport modeling and critical loads) should replace the earlier recommended model in European applications?

Freshwater eutrophication

- Spatially-explicit, global scale, damage level
- Questions about P as limiting nutrient
- Nevertheless: empirical PNOFs vs log con P
- Difference “absence” and “non-presence”?
- Applicability in water quality management?

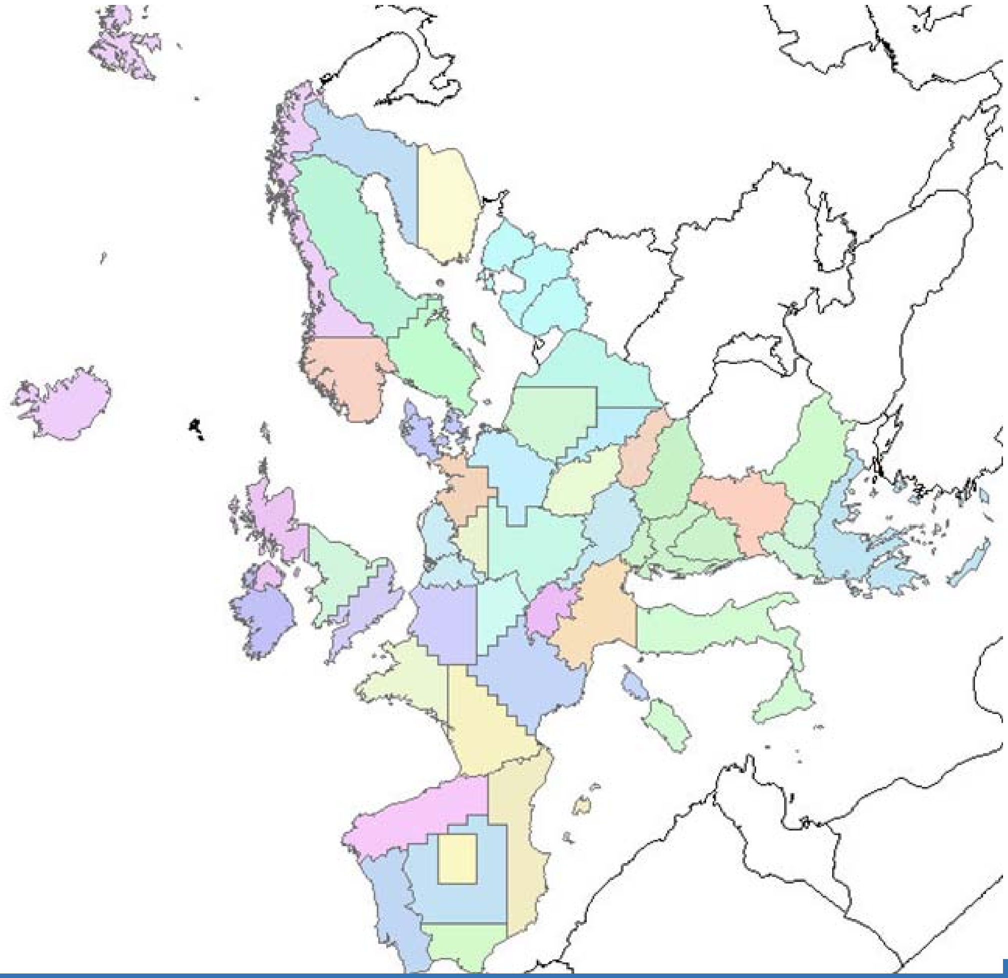
Marine eutrophication

- Characterisation factors at the damage level
- Based on a global N fate model
- Coastal waters: large marine ecosystems
- Fate factor: lower resolution than CARMEN?
- New: exposure (XF) and effect factor (EF)!
- What if... XF, EF are applied in ReCiPe?
  - Differentation in Mediterranean?
  - Answers to questions why some coastal waters are more vulnerable than others

WP3.3: **P**rimary **PM**, secondary **PM** (SIA) due to SO2, NOx and NH3; Ozone due to NOx and NNMVOC - Impacts on human health and ecosystems

- Emission scenarios (pre-defined)
- **Fate:**
  - Regional / global: Source Receptor Relationships **previously derived** with IMPACT World, EMEP and TM5 simulations
  - Archetypes: Meta-Model: Humbert et al 2011
- **Concentration Response → Impacts:**
  - Human Health: literature and project review
  - Ozone on Ecosystem: new method develeoped

Source regions (countries and sub-regions) for which EMEP_EU_SRM source-receptor relations have been used



Europe

66 sub-regions

PPM2.5, PPMcoarse,
NMVOC, SO2, NOx, NH3;

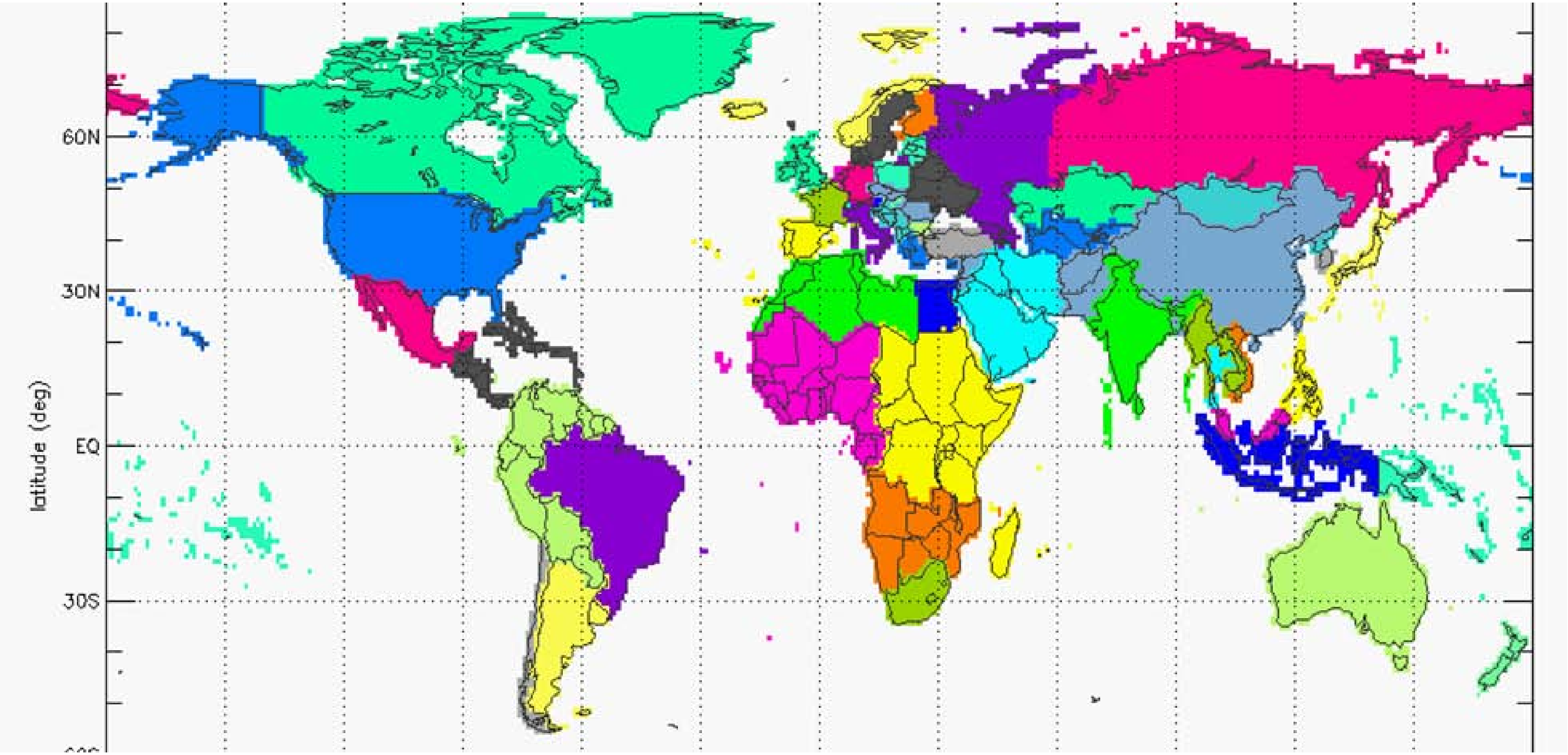
2 different heights,

5 different meteorologie

2 different background emission scenarios

ozone on ecosystem;

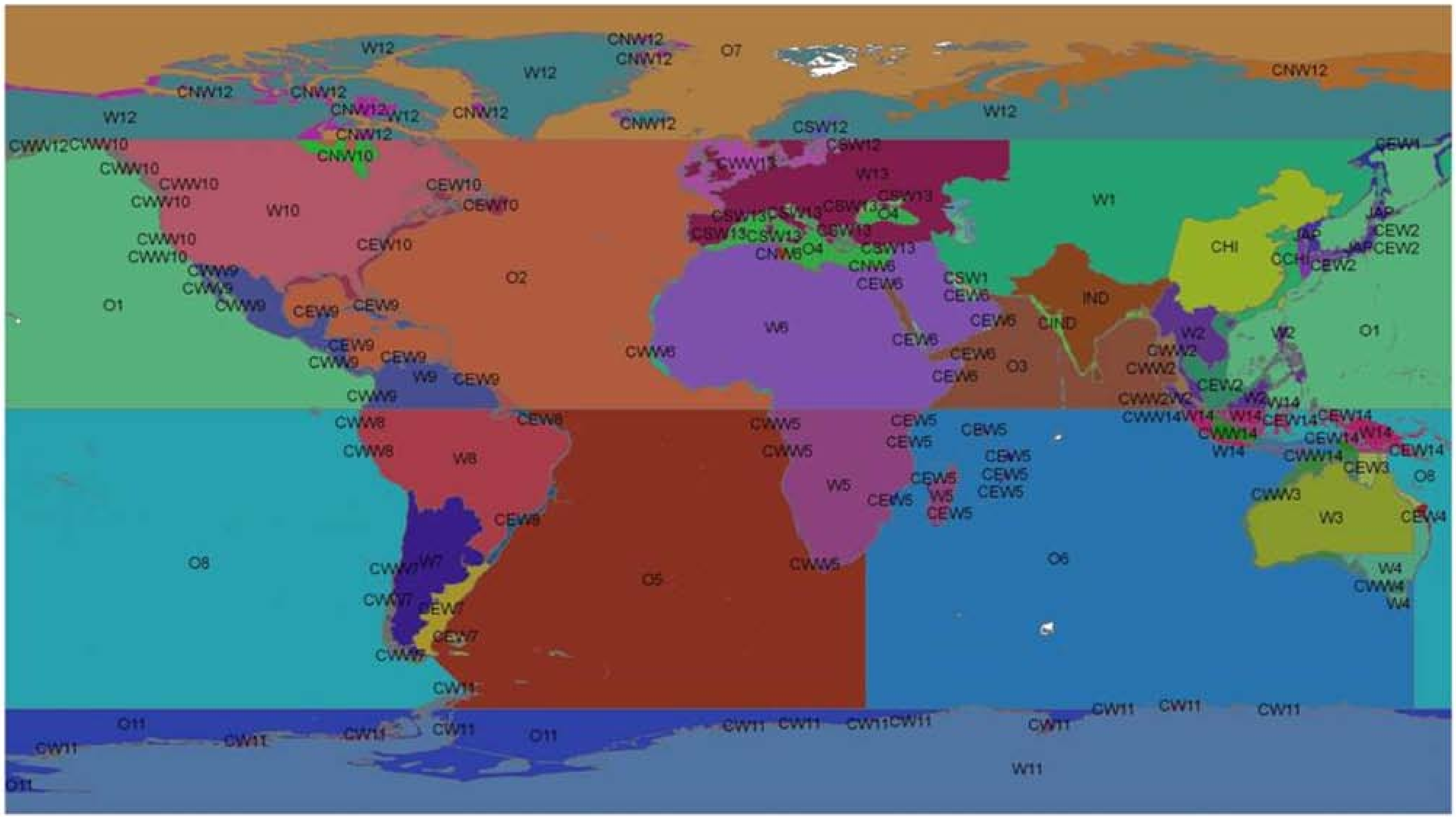
Global "Source-Receptor-Matrices" derived with TM5-FASST model



World, 56 regions,

POM, BC, NMVOC, SO2, NOx, NH3; no different height, no ozone on ecosystem;

IMPACT World Location of the 17 regions W1-W14 and IND, CHI and JAP



World, 17 regions,

PPM & SIA; no different height, no ozone

Approach Ozone EcoSystem

Ozone damage to natural vegetation by emissions of NOx and NMVOC

$$CF_{x,i,e} = \sum_j \sum_e (FF_{x,i \to j} \cdot EF_{j,e})$$

Characterization factor (in PAF·m²·yr/kg) :

- for substance x → NOx, NMVOC
- for vegetation type e → grassland, forest
- in region i → 65 european regions

Fate Factor

$$FF_{x,i \rightarrow j} = \frac{\Delta AOT40_j}{\Delta M_i}$$

Partial Fate Factor (in ppm.h·yr/kg):

- represents the change in ozone threshold concentration in a receiving compartment cell j (in ppm.h) due to a change of emission of precursor x in region i ([kg/yr])
- derived with the EMEP atmospheric fate model
- emissions were decreased by 15% compared to the baseline emission inventory

Effect Factor (1/3)

$$EF_{j,e} = \frac{\partial PAF_{j,e}}{\partial AOT40_j} \cdot A_{j,e}$$

Effect Factor (in PAF·m²/ppm.h):

- marginal change in the Potentially Affected Fraction (PAF) due to the marginal change in ground level ozone exposure
- EF based on a lognormal relationship between the PAF and ground level AOT40
- $A_{j,e}$ is the are occupied by vegetation type e in grid j

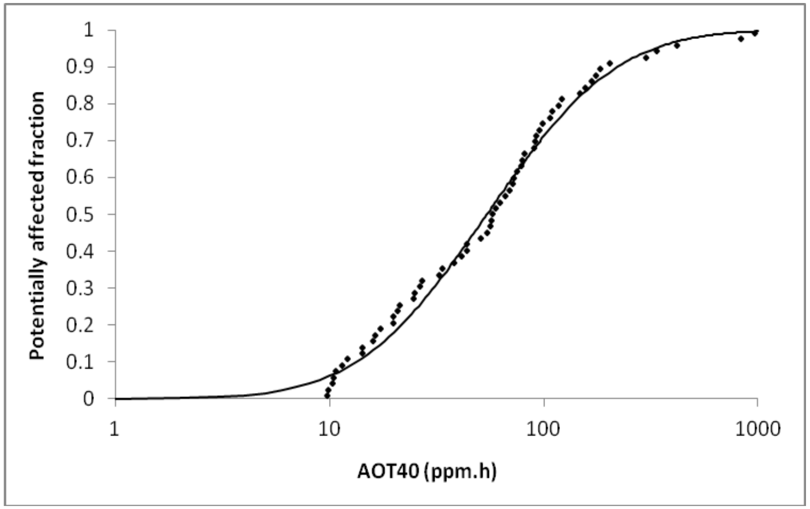
Effect Factor (2/3)

- species-specific ozone dose-response functions were used to calculate

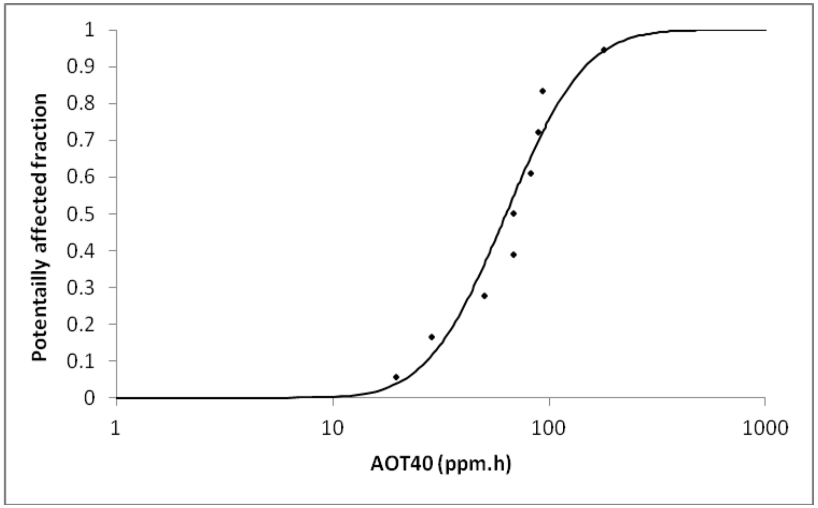
Species Sensitivity Distributions (SSDs)

- by calculating an EC50 value for each species

$$\frac{\partial PAF_{j,e}}{\partial AOT40_j} = \frac{1}{\sigma_e \cdot \sqrt{2 \cdot \pi} \cdot AOT40_{j,e} \cdot \ln 10} \cdot \exp\left(-\frac{1}{2} \cdot \left(\frac{\log(AOT40_{j,e}) - \mu_e}{\sigma_e} \right)^2 \right) dAOT40$$



grassland



forests

Effect Factor (3/3)

$A_{j,e}$ (in km^2):

• the Global Land Cover 2000 (GLC2000) database was used to calculate the grid-specific area occupied by each vegetation types

	Land cover classes	EF grassland	EF trees	Not considered
1	Tree Cover, broadleaved, evergreen		1	
2	Tree Cover, broadleaved, deciduous, closed		1	
3	Tree Cover, broadleaved, deciduous, open		1	
4	Tree Cover, needle-leaved, evergreen		1	
5	Tree Cover, needle-leaved, deciduous		1	
6	Tree Cover, mixed leaf type		1	
7	Tree Cover, regularly flooded, fresh water (& brackish)		1	
8	Tree Cover, regularly flooded, saline water,		1	
9	Mosaic: Tree cover / Other natural vegetation		1	
10	Tree Cover, burnt		1	
11	Shrub Cover, closed-open, evergreen	1		
12	Shrub Cover, closed-open, deciduous	1		
21	Snow and Ice (natural & artificial)			1
22	Artificial surfaces and associated areas			1
23	No Data			1

Results

- CFs for NMVOC emissions are in the range of 0.2 – 2.2 PAF.m^2.yr/kg,
- with lowest CFs for Finland and highest for Italy.

- CFs for NOx emissions are in the range of -0.06 – 9.58 PAF.m^2.yr/kg
- with lowest CFs for the Netherlands and highest for Switzerland

- negative characterization factors for NOx indicate reduced ozone formation.

NMVOC

NOx

Thank you for Your Attention!

Review of D3.4: Recommended assessment framework, method and characterisation factors for human health impacts of **fine particulate matter** formation: phase 2 (report, model and factors)

Marko Tainio

1. Systems Research Institute, Polish Academy of Sciences, Poland
2. National Institute for Health and Welfare (THL), Finland

1. Completeness of scope

- Aim is to create characterization factors for particulate matter (PM) air pollution emissions
- The scope is challenging but the study answers well for this challenge

2. Scientific robustness & Certainty

- Scientific robustness is high:
  - Deliverable use state-of-the art methods & tools

3. Applicability

- Presenting of the results with **intake factor, effect factor** and **characterization factor** concepts allows the usability of these results beyond the context of this project

4. Suggestions for improvement

1. Intake factor – effect factor balance
2. Health effects
    1. Duration of effect
    2. Short term + long term mortality
3. Urban increment
4. Intake factor or intake fraction?

iF – EF balance

Intake factor

- ~39 pages + supplement
- iF's based on two models (TM5-FASST and IMPACT World)
- Region and country variation taken into account

Effect factor

- ~7 pages
- One dose-response function for all sources and all regions

Study is dispersion/exposure focused!

Possible improvements for effect factor?

- More description of the health calculation methods & data
- Dose-response function differences in different part of the world (e.g. China)?
- Background health effect differences in different regions?

It is expected that till the end of 2012 there will be a new summary of recommendations regarding CRFs from WHO become available. This recommendation will be reviewed for the final deliverable for LC-IMPACT. This may include also an accounting for non-linearity of CRFs at high background concentrations like in Asia and in urban environments. It will also include an analysis of influence of age distributions, risk group fractions and incidence rates, as far as such data is available.

Page 34

	CRF PM2.5	CRF PM10	Disability weight (DW) per endpoint	Duration of endpoint	DALY per case
Bronchodilator Usage Children	-	1460	0.22	0.00274	0.0006028
Cardiac Hospital Admissions	-	0.43	0.71	0.038	0.02698
Chronic Bronchitis	-	8.6	0.099	10	0.99
Infant Mortality	-	0.58	1	80	80
LRS Adults	-	13100	0.099	0.00274	0.00027126
LRS Children	-	18600	0.099	0.00274	0.00027126
WLD	2070	-	0.099	0.00274	0.00027126
YOLL chronic	101	-	1	1	1
Bronchodilator Usage Adults	-	9130	0.22	0.00274	0.0006028
Cough Days	-	-	0.07	0.00274	0.0001918
LRS Children Excl Cough	-	-	0.099	0.00274	0.00027126
Mortality	-	-	1	1	1
MRAD	5770	-	0.07	0.00274	0.0001918
Respiratory Hospital Admissions	-	0.56	0.64	0.038	0.02432

Table 6, page 29

DALY comments/questions

- Were short term and long term mortality result combined?
  - Possibility of „*double counting*"?
- Duration of mortality 1 year
  - For „chronic mortality" effect likely longer than one year (~10 years)

Urban increment

- Urban increment was estimated based on methods similar to City-Delta methods
- Method is based on increment of PM **concentration** in urban area
- How concentration increment was turned to iF's (page 57)?
  - And were these results used in the calculations?

Apte et al. 2012 study

- iF's for urban air pollution emissions in 3646 cities around the world
  - Possibility to use this data in LC-IMPACT?

Environmental Science & Technology

Article

pubs.acs.org/est

Global Intraurban Intake Fractions for Primary Air Pollutants from Vehicles and Other Distributed Sources

Joshua S. Apte,[†] Emilie Bombrun,[‡] Julian D. Marshall,[*,‡] and William W. Nazaroff[§]

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Intake fraction (iF) or intake factor (iF)?

- Intake **factor** term was used in the report
- Bennett et al. 2002 defined and used term intake fraction
- Why two different terms for same method?

Spatial dependent Ozone and PM10 Characterization Factors for use in LCA

A review of D3.4 and D3.5

M. Schaap


Relevance

Air pollution levels are controlled by:

- emission mixture and density
- meteorology and climate conditions.

The impact of air pollutants on population and natural areas is dependent on the proximity of receptors to sources.

Hence, emissions in different regions of Europe and the world are anticipated to have different impacts.

Incorporating these impacts in LCA is a very meaningful, but challenging endeavour.

Data needed for assessment

Sensitivity of modelled concentration to an incremental change in emissions

These exist for many regions and components (focus on concentrations)

However, different models are used and strategies differ

→ world wide systematic analyses are lacking

In Europe EMEP performs these analyses in an operational sense for developing mitigation strategies

A group of models is used to benchmark the EMEP model for this purpose (EURODELTA), HTAP goes in this direction


Methodology used by LC-Impact

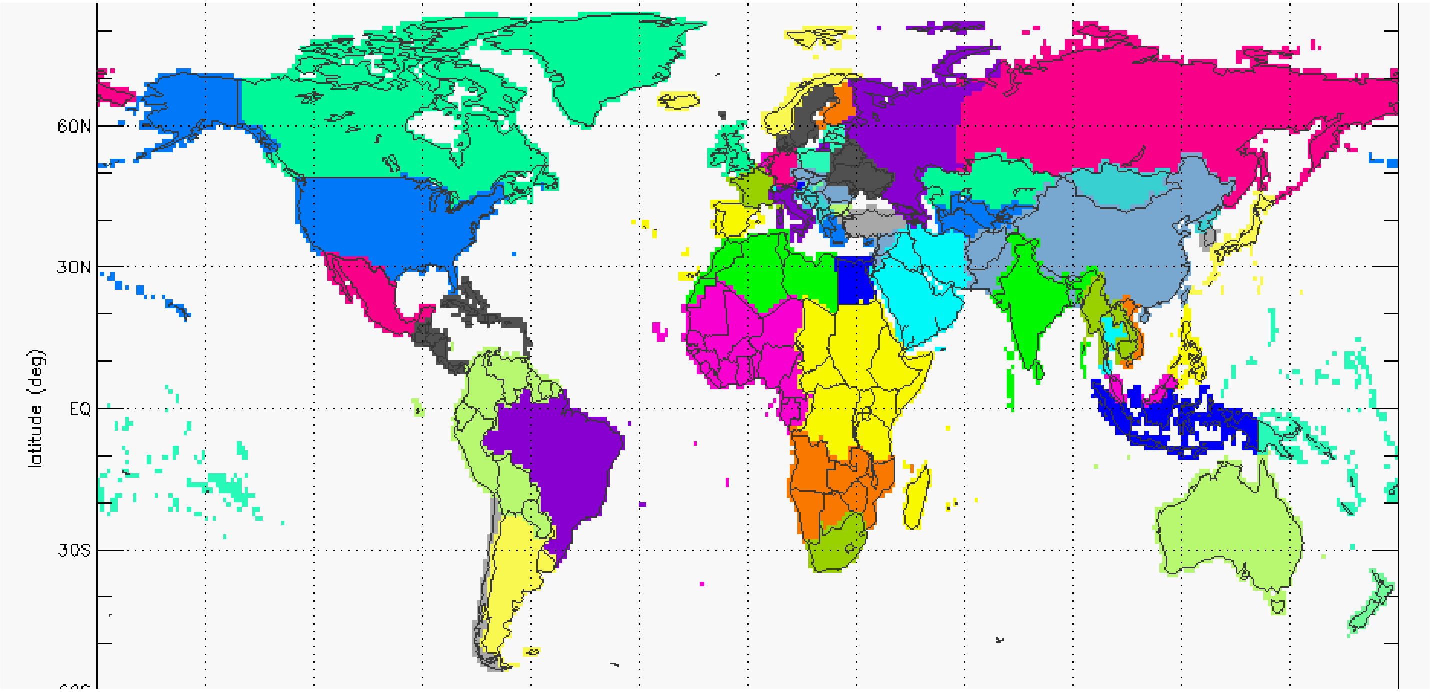
- Two state of the art chemistry transport models were used to assess spatially differentiated intake factors for ozone and particulate matter.
- Source receptor calculations are very large computational efforts that only few groups are able to do.
- More complete and detailed data do not exist at the moment.
- Reports provide a significant step forward.

Discussion on SRM setup

Set-up follows political boundaries, not optimized for LCA

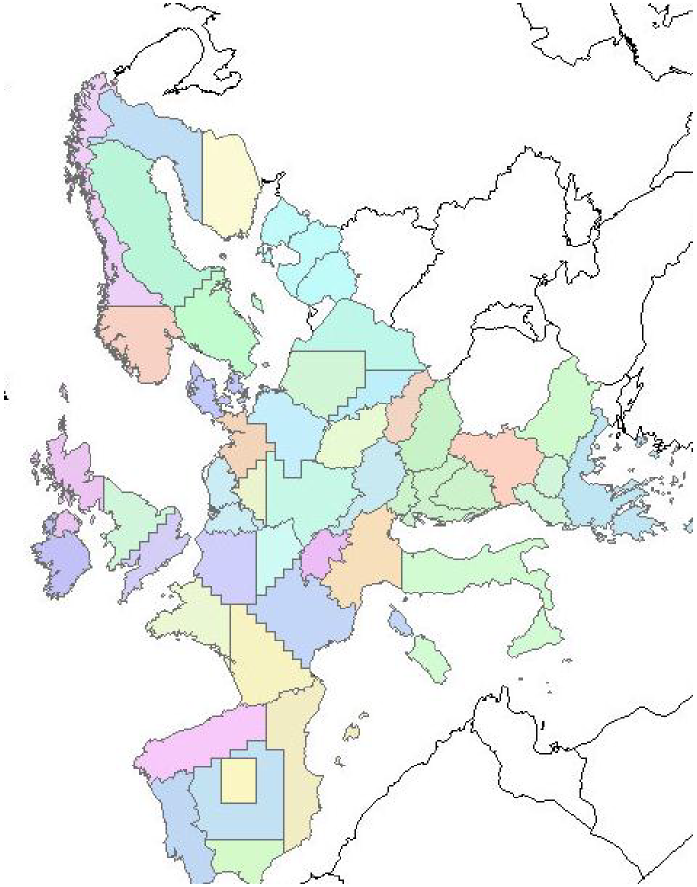
- New York and Alaska have the same impact

Intra-region variations will be large as well, even with use of archetypes

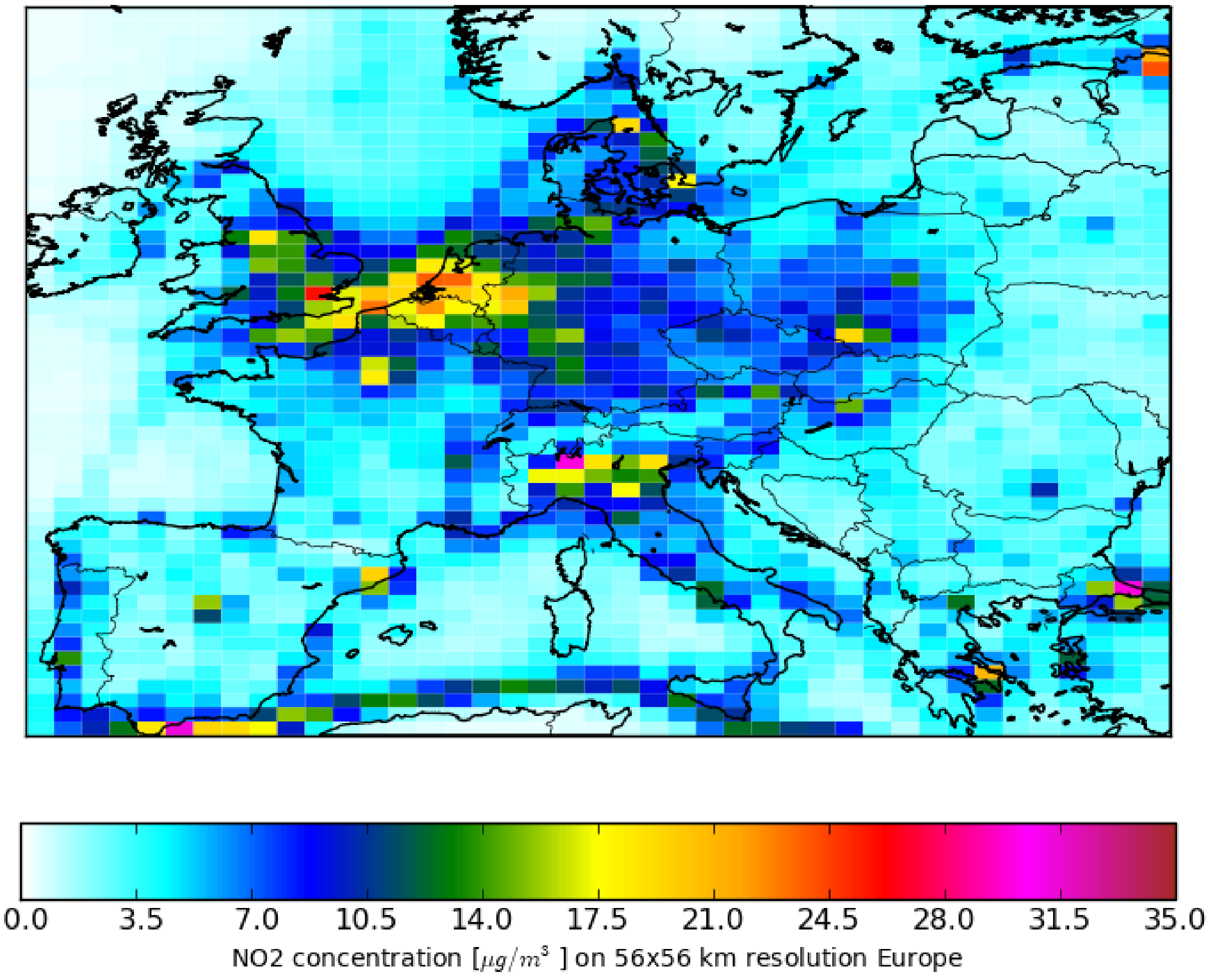


Discussion on resolution impact

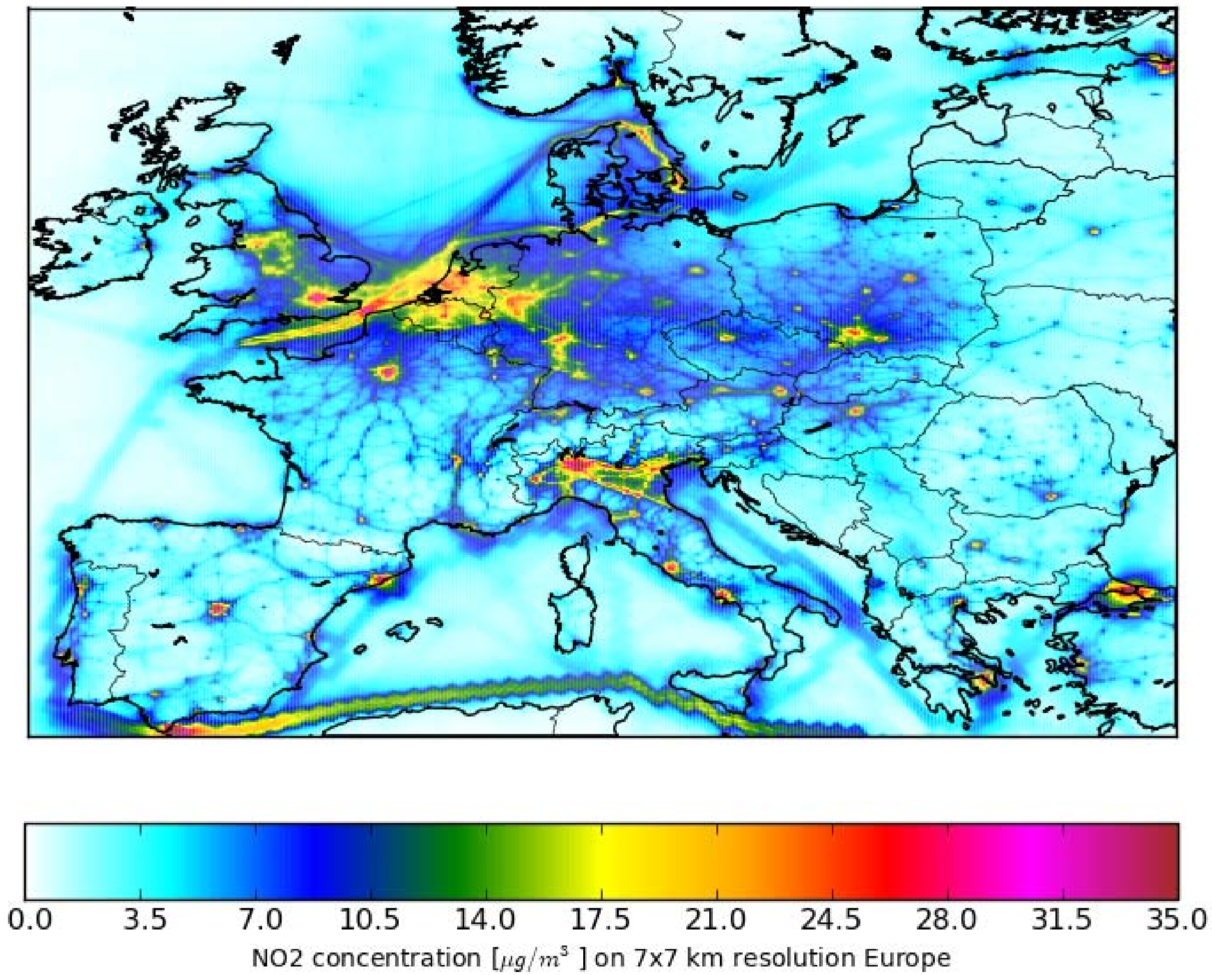
- Regions used by the EMEP simulations used here have been adapted
- 0.5 x 0.5 degrees represents 25x50 Km
- All emissions vs stack emissions
- For ozone solvent use sector and the difference in NMVOC speciation per sector may be much more important than stack height



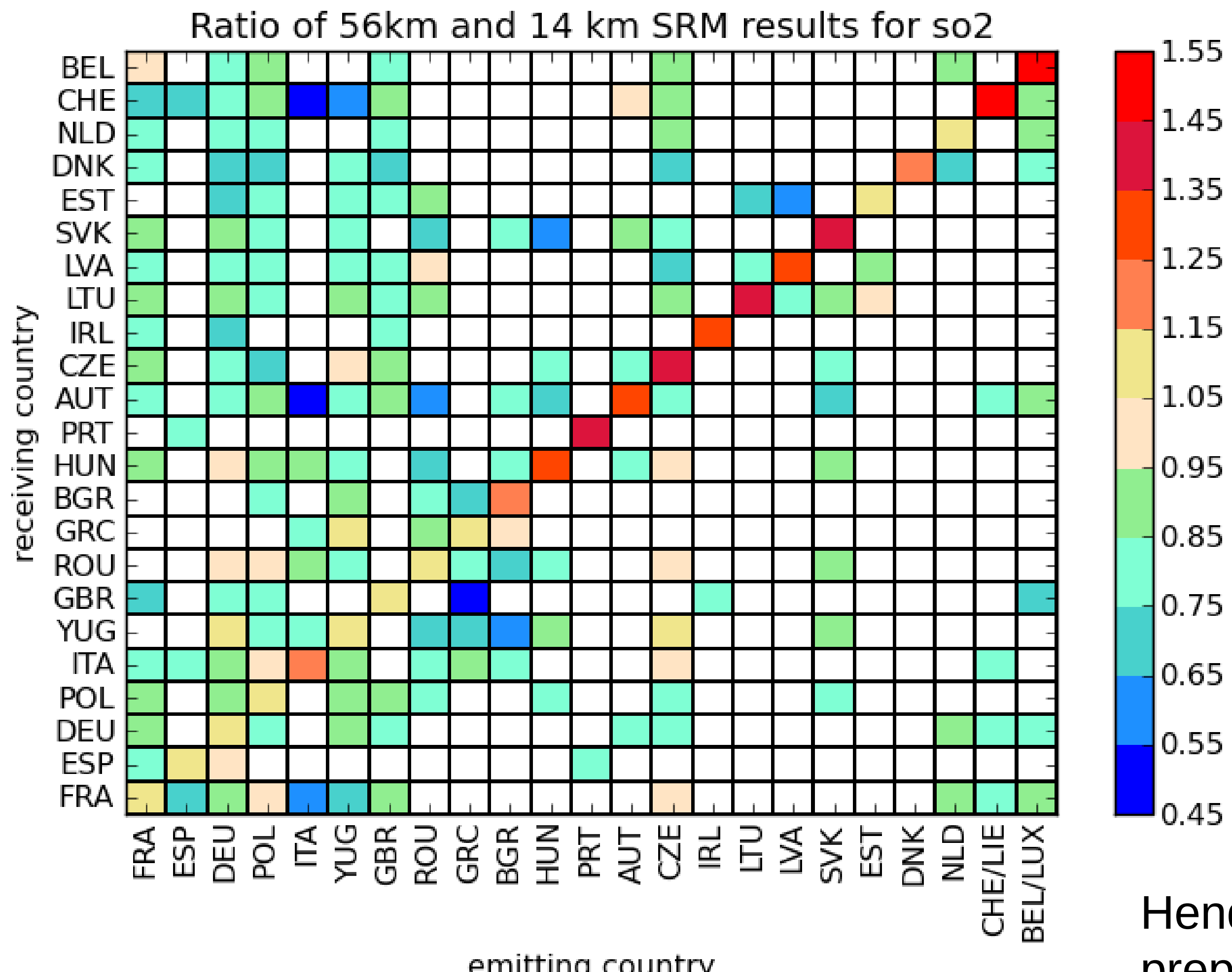
First results: NO2 at 56 and 7 Km resolution



First results: NO2 at 56 and 7 Km resolution



SRMs for SO2: Ratio of 14 to 56 Km resolution



Hendriks et al., in prep

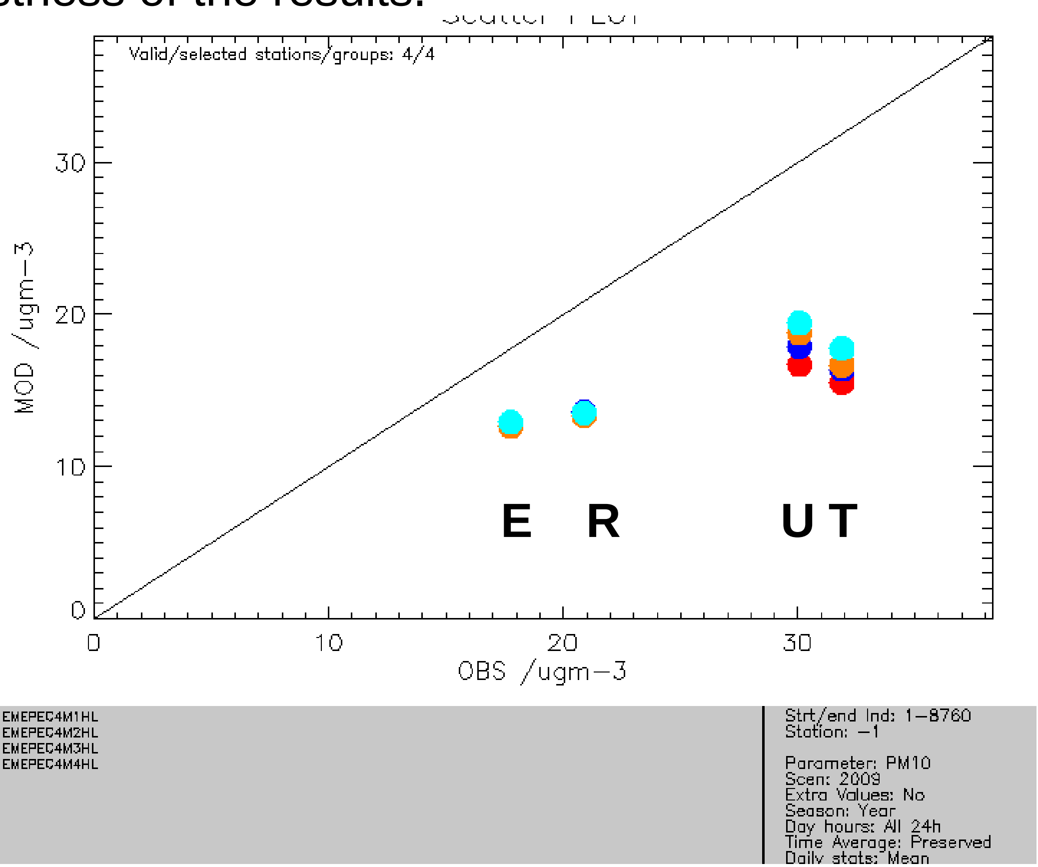
For ozone no urban scale issue is addressed

- Different approach taken for ozone and PM with respect to the use of archetypes.
- For ozone this is not used, though urban areas have generally lower ozone due to ozone titration.
- In case of PM the factors by Humbert et al (2011) are used to downscale IF fractions to a subgrid level for the global models
- Why not using the same for Europe (EMEP)?
- For Europe the city delta approach is proposed: but how to apply it?

- Note that the urban increment or city delta approach is still not accepted for operational use in AQ community as underpinning emission information is not available.

Methodology

- Although state of the art models are used, major uncertainties are still associated with the chemistry transport modelling that affect the robustness of the results.



PM model uncertainties

- Organic carbon missing in most models (and these SRMs)
- Little known about formation routes and source contributions for OC (or OM)
- OC considered to be volatile now. (causes an absence of urban rural gradient)
- In Europe nitrate remains a challenge
- Dry deposition process (regional)
- Wet deposition process (global)

Robustness

The reports do not provide a discussion on the quality of the model performance in comparison to observations, which could be important for the interpretation of the CFs.

The SOMO35 based impact assessment is very sensitive due to the threshold in the indicator. EMEP overestimates background ozone and therefore has larger SOMO35 levels than other models. This may result in different responses per unit emission reduction. The robustness of the assessment based on different ozone indicators should be investigated in the future.

Major uncertainties are associated with the emission data used

- Activity data and emission factors for developing world are uncertain.
- NMVOC speciation is based on few studies for western conditions and basically not updated since the early nineties.
- Spatial allocation may be poor on a global scale. Missing information may mean allocation following population density causing unwanted correlation with the impact results presented here.
- Time profiles for emissions are important. Diurnal cycles especially for primary species, and meteorological dependency for all sources impact atmospheric formation, lifetime and mixing. Current simulations lack them (TM5) or are very basic (EMEP).
- Downscaled emission data tend to overestimate emissions in urban areas as energy use in cities is more efficient than in rural areas.
- Biogenic emissions of VOCs are very uncertain and impact the NOx and NMVOC response.

Robustness

- Spatially differentiated intake factors have not received a lot of attention so far.
- This is evidenced by the few references available and use of e.g. European value of one study for ozone applied to the whole world.
- The robustness of model responses to emission changes has received little attention and in Europe only has been assessed within the EURODELTA and HTAP studies
- The validation status of responses to emission changes is limited.
- The range in results, especially for PM, indicates the uncertainty associated with these simulations. Hence, the uncertainty is still large

Applicability

For Europe better than for the world.

Global simulations do not resolve the spatial scales relevant to assess incremental emission changes for AQ.

Sector differences may play a crucial unaccounted role (ozone formation potentials)

I do not know how to apply it in LCA, except when locations of installations used are known exactly. Do you use the steel from CORUS, China, Spain, Chili, or do you take the cheapest?

Maybe 56 regions is a bridge to far


Suggestion for improvements – New project

- Design a LCA relevant SRM approach:
  - Redefine the regions of interest and devise a smart sampling strategy to assess sensitivities to emission changes.
  - Or assess sub-region sensitivities on the different continents
- Incorporate more detail on the emissions used:
  - VOC speciation
  - Spatial allocations
  - Temporal variation
- Use several models to test robustness of the central estimate or use several studies to compile impact factors.


Thanks!


Suggestion for improvements - reports

Discuss the use of the CFs for PM and the city delta approach in the report.

Extend the chapter on uncertainties with CTM model uncertainties and potential impacts on results.

WP3.4
D3.6
Characterisation factors for midpoint human noise

CML, USTUTT

Stefano Cucurachi
Reinout Heijungs
Philipp Preiss
Sandra Torras

Noise and LCA

- Noise still outside main LCA databases and software
- No recommended approach from ILCD
- Exception: Latest version of Japanese LIME (comparable approach)

Background

- Deliverable D3.1 (published as Cucurachi et al., 2012)
- EASA report BANOERAC on background sound levels for EU27 (2009)
- EU report CNOSSOS (2012) on the calculation of the sound power level and propagation of sound emitted from various type of sources
- ISO standards on sound propagation and noise calculation

Fate factor

$$FF_{i,c,f} = \left(\frac{\partial P_{i,f}}{\partial W_{i,c}} \right)_{W_{i,c} = Wamb_{i,c}} =$$

$$= \frac{C_{ref}}{\sqrt{Wamb_{i,c}}} \times 10^{\left(D_{i,c,f} - A_{i,c,f}\right)/20}$$

distance

attenuations

background

A change of ambient sound power at **c** will result in a change of pressure at **f**

in [Pa/W]

Effect factor

frequency-dependency

$$EF_{i,f} = N_f \times 10^{(\alpha_i + \beta_f)/20}$$

time of the day

population density

in [number of individuals]

CF for Midpoint human noise

$$CF_{i,c} = \frac{C_{ref}}{\sqrt{Wamb_{i,c}}} \times \sum_f 10^{\left(D_{i,c,f} - A_{i,c,f}\right)/20} \times N_f \times 10^{\left(\alpha_i + \beta_f\right)/20}$$

[in person-Pa/W]

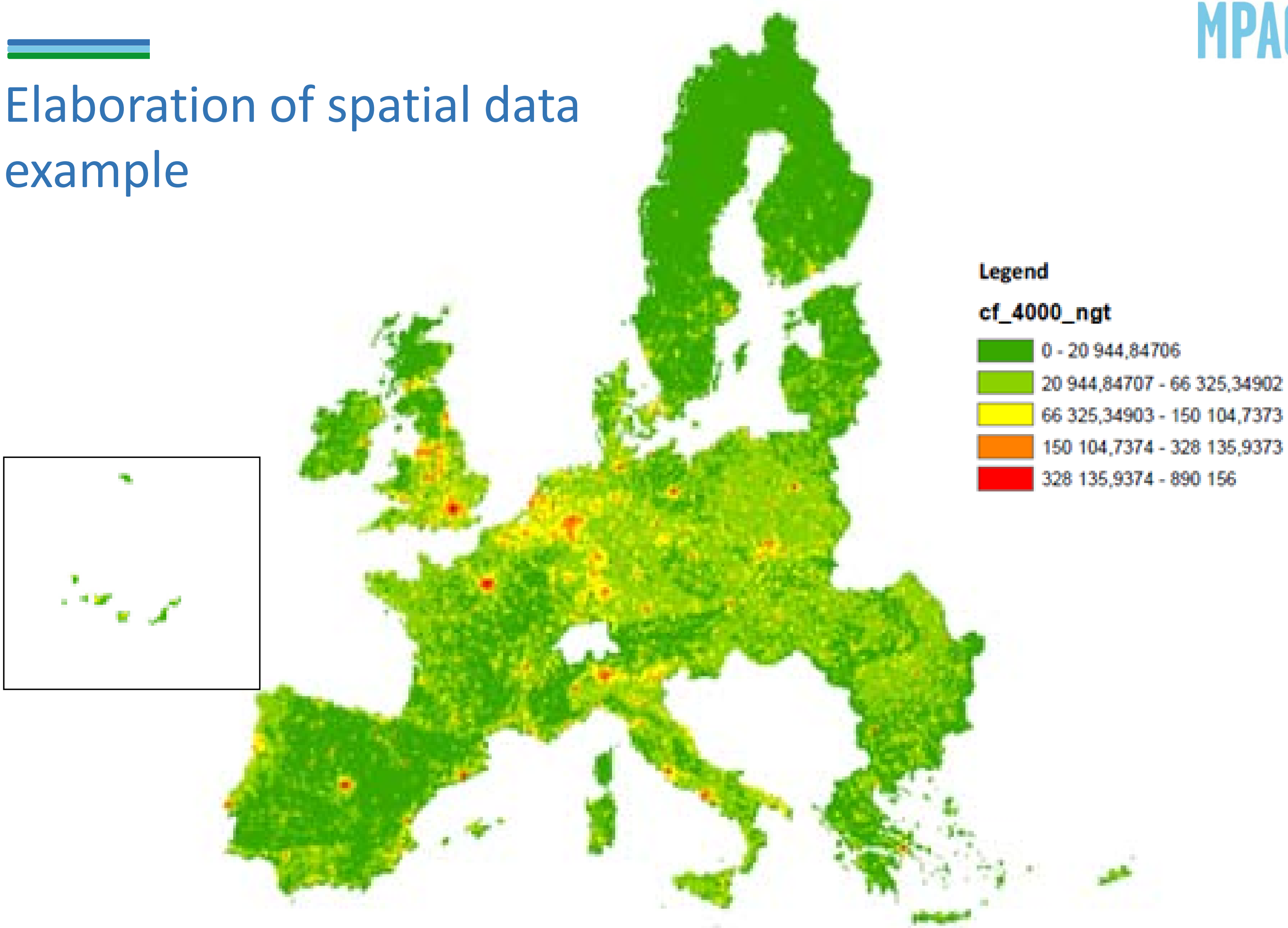
Elaboration of spatial data

- For EU27 we collected spatially-defined information for:
    - Background sound levels
    - Population
    - Meteo (e.g. temperature, pressure)
    - Elevation
    - Land cover

Elaboration of spatial data

- We combined spatial data in a GIS environment according to the theoretical framework defined in D3.1 and calculated spatially-explicit characterisation factors (CFs). 10 by 10 kms grid, EU27.
- Spatial CFs were defined for:
  - **centre-frequency ranges**: 63 Hz to 8000 Hz (i.e. 8 in total)
  - **time**: day, evening, night.

Elaboration of spatial data example



From spatial CFs to archetypal CFs

- We used the literature and the data elaborated in the spatial analysis to define central nominal values to be used for the calculation of the CFs:
  - **centre-frequency ranges**: 63 Hz to 8000 Hz, unspecified (i.e. 9 in total);
  - **place**: urban area, suburban (i.e. residential)area with no nearby traffic concern, rural area with no nearby traffic, industrial or commercial area, indoor, unspecified;
  - **time**: day, evening, night, unspecified.
  - **user-defined.**
- A total of 217 characterisation factor was calculated for the defined AC (i.e. 216 AC plus 1 user-defined characterisation factor)

From spatial CFs to archetypal CFs - Example

Elementary flow	Specifications			Characterisation factor for midpoint human noise (HN)
(in J)	Octave *	Time **	Place ***	(in person-Pa/W)
sound[octave_1,day,urban]	1	day	urban	3,11E+04
sound[octave_2,day,urban]	2	day	urban	6,97E+04
sound[octave_3,day,urban]	3	day	urban	1,67E+05
sound[octave_4,day,urban]	4	day	urban	3,11E+05
sound[octave_5,day,urban]	5	day	urban	4,48E+05
sound[octave_6,day,urban]	6	day	urban	2,93E+05
sound[octave_7,day,urban]	7	day	urban	2,75E+05
sound[octave_8,day,urban]	8	day	urban	2,43E+05

...

sound[octave_user-defined,user-defined,user-defined]	user-defined	user-defined	user-defined	-

From spatial CFs to archetypal CFs - Example

*	
Octave	Mid-value
1	63 Hz
2	125 Hz
3	250 Hz
4	500 Hz
5	1000 Hz
6	2000 Hz
7	4000 Hz
8	8000 Hz
**	
Time	Hours
day	7 am to 7 pm
evening	7 pm to 11 pm
night	11 pm to 7 am

***	
Location	Description
urban	urban area
suburban	suburban (residential) area with no nearby traffic concern
rural	rural area with no nearby traffic
industrial	industrial or commercial area
indoor	indoor

Conclusions

- Framework is made operational
- CFs are now available and ready to be used (to be tested in case studies)
- Results of global sensitivity analysis now available
- Emission data is a scarce resource

THANKS.

Review on D3.6

Bruxelles, 6 November 2012

Enrico BENETTO, Olivier BAUME

Public Research Centre Henri Tudor (CRPHT) - Resource Centre for Environmental Technologies (CRTE)

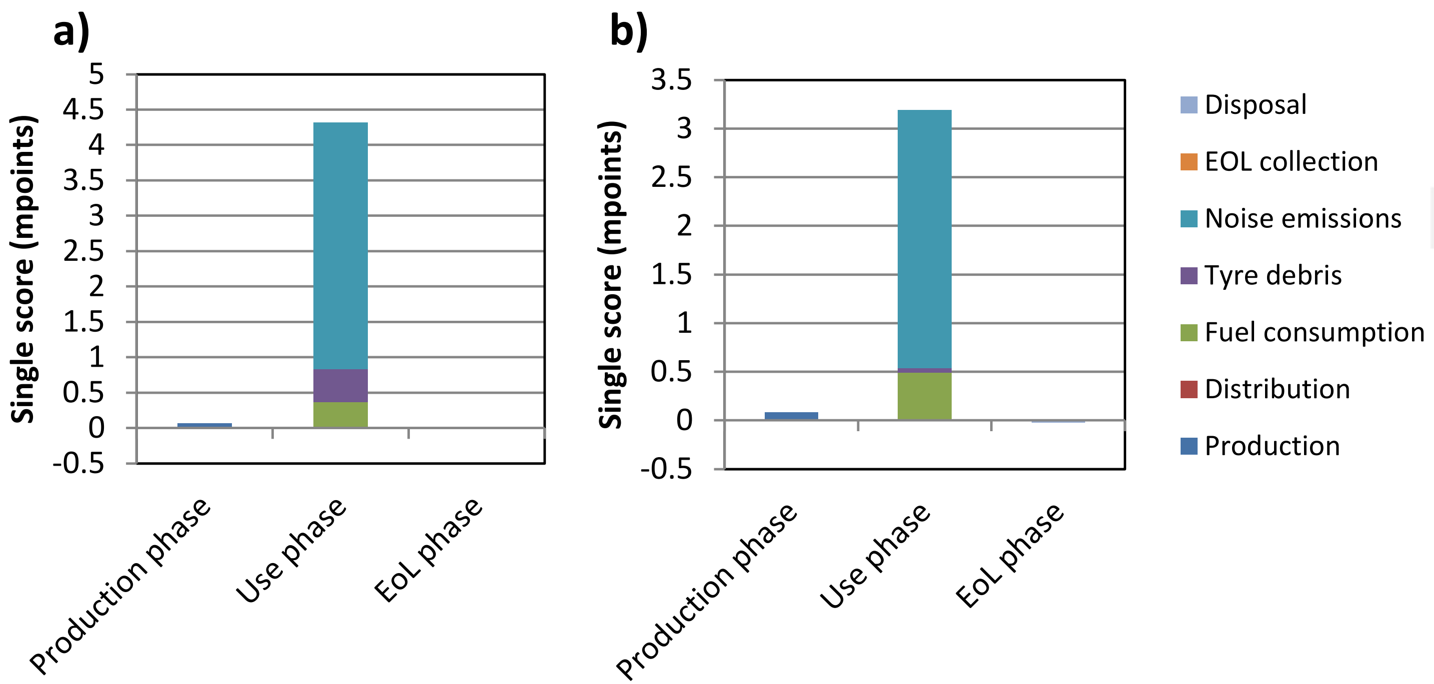
Esch-sur-Alzette - Luxembourg

Noise in LCA: is it relevant?

Tire noise as compared to the tire life cycle

a) EI99

b) ReCiPe



...Yes, but current methods are too much focused on traffic noise and generic (no context specific)

Source: unpublished study – CRPHT/CRTE

Noise assessment based on Müller-Wenk, 2004

Deliverable 3.6. Recommended assessment framework, method and characterisation factors for noise impacts

Aim: further analyze the framework defined in deliverable D3.1 is and explain and define each of its parameters

Development of CFs:

- For archetypal situations (AC)
- At geographically defined scale (SC)

Review criteria:

- Completeness of scope: have all the relevant and pertinent aspects been included?
- Environmental relevance: is noise an environmentally relevant issue?
- Scientific robustness & Certainty: what are the possible methodological improvements/refinements?
- Documentation & Reproducibility: is the report transparent, complete and clear enough?
- Applicability: is the method applicable?

Completeness of scope

Incomplete				Complete
			X	

Comments:

Current noise assessment methods in LCA tend to overestimate noise impacts because of the too coarse level of detail and scope of the situations described.

- How far the proposed assessment scheme is closer to specific (spatially defined) situations (i.e. emission/targets conditions)?
- Would it be possible to include much more detailed situations, e.g. considering a number of archetypical situations AC, corresponding to actual conditions, within each cell of SC?

Importance of (existing) background noise to the assessment.

- Considering the limited scope of background noise assessment, what is the actual reliability of the proposed method?

Environmental relevance

Irrelevant				Relevant
				X

Comments:

Noise assessment is certainly relevant in LCA (despite most often ignored) and therefore the relevance of the proposed approach is very high.

Suggestion to authors:

- Better introduce the relevance of noise as impact category in this report but maybe this has already been done in other WPs.

Scientific robustness & Certainty

Weak				Robust
			X	

Comments:

SC approach allows a better representation of local specificities

- how far is this approach from the modeling of detailed spatial contexts (using dedicated noise models)?

A few assumptions are not justified/properly discussed:

- "value of background sound is considered equal across all centre-frequency bands"
- "Average distance of 1m between source and receiver"
- Pessimistic view by choosing the "maximum background sound power level"

Synergistic and masking affects are not treated and discussed enough in the report and would deserve better consideration

Suggestion to authors:

- Would it be beneficial to consider more scenarios/archetypal situations also in the case SC to account for the variability and cases from the above mentioned points?

Documentation & Reproducibility

Weak				Robust
		X (documentation)	X (Reproducibility)	

Comments:

Format and presentation:

- Nomenclature problem (inconsistencies, missing references, ..)
- Equations could be numbered
- Cross-references
- Inconsistencies on data and variables

Suggestion to authors:

- Present AC and SC with precise choices, data and approaches used
- Revise the presentation of data flowcharts (how AC and SC calculation frameworks work)
- An additional introduction on the objectives of the deliverable would certainly be beneficial for the readers, as a reminder of D3.1
- Executive Summary could be extended to be more informative for decision makers and possible users of the method

Applicability

Not applicable				Applicable
			X	

Comments:

Basic inventory data (sound powers) are still missing (independent from the work)

Tool for the calculation of user defined CFs will certainly be very useful.

How is the framework applicable to mobile sources, e.g. transports.

- Is the SC approach to be preferred?
- Is the grid dimension sufficient to catch the transport specificities?
- Or is the AC approach to be preferred, but still is it flexible enough?

Suggestion to authors:

Discuss further the applicability to mobile sources

Final conclusion

The operationalization of noise assessment is certainly very pertinent

The tool for the calculation of user defined CFs will be very beneficial to practitioners.

The framework could be easily extended if additional or complementary data and information are available.

Main improvement points

- documentation, i.e. the form of the report and the presentation of the operationalization.
- Clarification of a few methodological issues and assumptions (e.g. on background noise)
- Possible consideration of sensitivity analysis and combination of SC and AC approaches

PUBLIC RESEARCH CENTRE HENRI TUDOR

Thank you for the opportunity of reviewing the report

Dr. Enrico Benetto

R&D Manager

Resource Centre for Environmental Technologies

CRP Henri Tudor

Luxembourg

E-mail: enrico.benetto@tudor.lu

T3.1 Aquatic eutrophication

- Application of the limiting factor concept of N and P for marine and freshwater systems + need for temporal differentiation/averaging?
- Should we sacrifice fine resolution of the European models to gain global coverage?
- Can we include the eutrophic status (background information) of the receiving ecosystems? – to improve environmental/ ecological relevance

Acidification

- Main feedbacks:
  - Environmental relevance weakness of Type 2 midpoint (critical load exceedance method)
  - A scientific basis for recommending characterization model of Type 1 (soil modeling) over Type 2 (critical load exceedance) should be clarified
- Action points (short term):
  - Qualitative description of existing critical load methods
  - Verification of quantitative results of the current CL approach (Bouwman 2002)
- Action points (long term):
  - Generate midpoint characterization factors based on Posch et al. (2008) and perform a quantitative comparison with midpoint approach of Roy et al. (2012)

WP3.4
D3.6
Characterisation factors for midpoint human noise. Feedback and actions

CML, USTUTT

Stefano Cucurachi
Reinout Heijungs
Philipp Preiss
Sandra Torras

Short term actions

- Sensitivity analysis. Included in final report: results of global sensitivity analysis already available.
- Documentation: form of report improved in new draft version.
- Applicability of framework to mobile sources: it is already applicable, but it will be better specified (e.g. train journey: 20% urban, 80% rural)

Short term actions - Points of futher discussion

- Background sound level:
    - Equal value across all frequency-bands is due to limited availability of data.
    - BANOERAC is not only the best available report on background sound emissions data but also the **only one**.
    - Assumptions will be made more explicit.
    - Research priority.

Long term actions – Right level of spatial definition

- Soundness and limitation of the spatial approach to the definition of CFs:
  - Enlargement of the scope of the assessment → including more archetypal situations of emission? To be analysed if more data is available
  - Finding the right balance between spatially explicit CFs and archetypal CFs. → User-defined approach already developed in the report may be further explored
- Synergistic and masking effects: included at the level of background sound emissions but:
  - literature will be further investigated to verify if suitable for inclusion in the model.

Long term actions – From midpoint to endpoint

- It adds extra uncertainty but allows for reducing indicators:
  - DALY for annoyance?
  - DALY for sleep disturbance?
    - Available but criticised

7.7. Template public consultation

Public consultation
on
Life cycle impact assessment methods developed within LC-impact project

Draft document for public consultation can be downloaded at:

www.lc-impact.eu/consultation

Deadline for comments: 2.01.2013

Please send the filled form to: lc-impact@science.ru.nl

Important!
All the information provided in this document will be published.
If you do not agree to have your personal details (name, contact and email) published please indicate it clearly below.
Thank you for your feedback.

Form for comments

Name	
Affiliation	
Contact	
Email	
Type of organisation	Please put **x** in the appropriate box:
consultancy	
company	
professional/trade association	
NGO	
think-tank	
academia/research	
public authority	
private person	
other (please specify)	
Date	
Consent for publishing (yes/no)	

Data: Name: Affiliation:

(1)	(2)	(3)	(4)	(7)
Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task

Thank you for your input!

The LC Impact Team,

7.8. List of stakeholders providing comments within public consultation on line

Name	Organisation
Lars-Åke Lindahl	Dragon Mining
Johannes Drielsma	Euromines
Pekka Suomela	FinnMin
Mikael Schauman	Lundin Mining AB
Antonino Marvuglia	Public Research Centre Henri Tudor
Cecilia Askham	Ostfold Research AS
Sandra Roos	Swerea IVF AB

7.9. Consolidated comments public consultation on line

The received comments and the answers provided buy task and work packages leaders are reported in the following pages.

Public consultation
on
Life cycle impact assessment methods developed within LC-impact project

Draft document for public consultation can be downloaded at:

www.lc-impact.eu/consultation

Deadline for comments: 23 12.2012

Please send the filled form to: lc-impact@science.ru.nl

Important!
All the information provided in this document will be published.
If you do not agree to have your personal details (name, contact and email) published please indicate it clearly below.
Thank you for your feedback.

Form for comments

Name	Johannes DRIELSMA
Affiliation	European Association of Mining Industries (Euromines)
Contact	+32 2 775 6305
Email	Drielsma@euromines.be
Type of organisation	Please put **x** in the appropriate box:
consultancy	
company	
professional/trade association	x
NGO	
think-tank	
academia/research	
public authority	
private person	
other (please specify)	
Date	19 December 2012
Consent for publishing (yes/no)	yes

Data: Name: Affiliation:

(1)	(2)	(3)	(4)	(7)
Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
D.1.4	All	Ge	Euromines supports the work of the LC-IMPACT and greatly appreciates the initiative of the European Commission to further investigate and attempt to improve the methods used to assess issues around supply of abiotic natural resources in LCA. The project team is to be applauded for having made a serious attempt to deliver a model that answers the main concerns of stakeholders: "having enough at an affordable price". **At this point, we believe the developed model should be more precisely packaged and presented as a potential tool that draws upon LCA techniques to assess this particular socio-economic issue, but it should remain outside the scope of environmental LCA**. In addition, we would welcome further work and collaboration to also make a serious attempt to deliver a model that can be reliably used within environmental LCA to answer the *environmental* concerns of stakeholders related to the extraction of ore. Recently, many authors have been attracted to the concept of ore-grades as a potential parameter to detect depletion potential. We believe this is due to a general misunderstanding of what drives ore grades up or down. As extraction technologies and efficiencies improve, lower grade ores are exploited and more energy is expended to extract the value-mineral. This represents an environmental challenge for the mining industry going forward, but the challenge is ably captured in existing Areas of Protection within environmental LCA (e.g., climate change, eutrophication, respiratory inorganics, acidification, summer smog).	Thank you for this support. We can to some extend understand your position, but our role in this project was to develop methods and characterisation factors (CFs) for abiotic resource extraction. We don't decide what is on an environmental LCA scope. However, in the ISO standard and UNEP/SETAC Life cycle Initiative framework this impact category is clearly mentioned.
D.1.4	Page 1	Te	A significant proportion of coal production is used as a reductant in thermal processes (e.g., steel-making) rather than solely for energy production.	We will mention it in the paper. It does not change the method, as the coal is eventually combusted.
D.1.4	Page 2	Ge	It would be helpful to explain the statement: "*There are some category 2 methods, which are based on the concept of a use-to-stock ratio, but this type of methods are not compatible with category 4 endpoint methods*." Why are they not compatible? And is that sufficient reason to not consider them further? Resource depletion should be thought of in terms of a particular well-defined	According to the LCIA midpoint-damage framework of the UNEP/SETAC Life Cycle Initiative published in the Int J LCA 9: 394-404 by

(1)	(2)	(3)	(4)	(7)
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			location, time and scale (e.g., "depletion of the flow of liquid natural gas to the EU chemicals sector over the next ten years"). The scope of the assessment should be clearly defined and consistently applied (i.e. to definition of the problem, data used, interpretation and application of the results). In order to discuss depletion and/or accretion of resources, one must first agree a way to measure trends in stocks of resources. A relative change in stocks or funds should then be calculated to assess whether resources are being accrued or depleted in the given system and whether the rates of depletion are significant enough to be of concern over the considered period. So, a use-to-stock ratio approach would seem to hold some promise. Another valid approach might be to use the ratio of extracted volume and change-in-proven-reserves over the same period, i.e., ($production_{2008}$ / Δ $reserves_{2008}$). In exceptional years (rapidly falling prices) the ratio might be negative, but the majority of times this ratio will be positive due to continuous prospecting and proving of new reserves. A ratio greater than one may signal some cause for concern (production outpacing identification of new reserves).	Jolliet et al. (2004), a constant mid- to endpoint factor needs to exist between midpoint CFs and endpoint CFs to assure that the ranking between the CFs is the same. However, there is not such a constant relationship between use-to-stock and surplus cost CFs hence this comment. Besides, use-to-stock gives an indication of depletion and surplus cost of scarcity, which may explain why they are not compatible.
D.1.4	Page 2	Ge	"*An indicator that describes this mechanism is considered relevant for stakeholders (Vieira et al., 2011).*" We think you should refer to this as a "concept" or at least an "assumed mechanism", because the accuracy of the concept has not been demonstrated.	Noted.
D.1.4	Page 3 Figure 2	Te	The caption of the figure should perhaps refer to "production" costs rather than "exploration" costs?	Agreed, it should be production costs.
D.1.4	Page 3	Te	Ester van der Voet (Uni of Leiden) usefully identifies three different interests of stakeholders concerned about the use of natural resources: **Depletion**: the amount of a specific resource is reduced (i.e., globally) **Scarcity**: the amount of a specific resource, that is used in society, is/will be insufficient **Criticality**: the resource may be scarce, and is also important (i.e., economically) In our view, the Surplus Cost (SC) formula is actually a potential predictor of the resource "scarcity" and *not* of resource depletion. As stakeholders are interested in current and future scarcity on the market, this is a worthwhile assessment to	We will use the proper terminology in the final deliverable. We are also aware of the difference between these concepts.

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			make, but we suggest that the method, indicator and associated Area of Protection should be re-named to accurately reflect what the method is doing. Even so, the extent to which SC indicates scarcity is related to how affordable that SC is, which is in-turn related to fossil fuel prices. The question of whether or not the scarcity of a particular resource on the market should be included in the scope of an LCA, we will address in later comments below.	
D.1.4	Page 4 Crosscutting issues	Te	The discussion of uncertainties seems a little incomplete. Additional uncertainties in the method could be: the use data, assumptions about production techniques, population data, economic growth rates, substitution potential, technology assumptions and missing factors (e.g., due to the relative simplicity of the pathway presented in Figure 4, we are not sure if all significant drivers have been captured in the model). All uncertainties should be acknowledged.	We quantified those we could and discussed those we couldn't in the extended deliverable.
D.1.4	Page 4 How to estimate available fossil resources	Te	The assumption that "first the least costly resources are produced" has some problems, to the point that we are not sure that it holds at all for mineral resources (see below). This is itself related to the difficulty of exactly determining available resources. It may be helpful to consider important differences between the fossil-fuel supply industry and the mineral supply industry. It is possible that the assumption is more reliable for fossil-fuel supply due to the relatively limited resource base and relatively well-known geological processes associated with it. However, geologists are still deciphering hitherto unknown geological processes that have formed viable mineral deposits and are using new models to re-evaluate existing exploration data. It is unlikely that the industry has been as ideal as the assumption suggests, in always *finding*, let alone exploiting, the least costly mineral resources. This is one reason why it might still be useful to retain the use-to-stock ratio concept and further explore how those methods might also be further developed.	We think it is defendable on a long-term frame but it is an over simplification on a short time frame. However, characterisation models present for other impact categories as well are a simplification of the reality we want to capture. This assumption was discussed in the extended deliverable.
D.1.4	Page 6 Overall Summary	Ge	"*In life cycle impact assessment (LCIA), there are indicators measuring the damage caused by mineral resource use but none was considered mature for recommendation (EC-JRC-IES, 2011).*" Actually, none of those indicators "measures damage caused". They each attempt to provide information about some aspect of scarcity of resources on the market or depletion of resource stocks, and none of them does it well enough to be termed "measurement".	We are simply citing the result of a research study of existing impact assessment methods published by the European Commission – JRC to justify the reason for the work currently being developed within the LC-IMPACT project.

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Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
D.1.4	Page 6 Overall Summary	Te	We are concerned about the statement "*it quantifies the marginal decline in mineral/metal concentration in the rock extracted as a response to the increase of mineral/metal extracted*", because we do not think there is really a direct relationship causing such a "response". Grades within a mine can increase *or* decrease. If this statement is referring to the global situation (one finite planet), the statement is only correct if you calculate the mineral/metal concentration in the whole of the earth's crust before and after extraction (if you remove 1kg of metal from the earth's crust, the average concentration of the remaining metal decreases by some marginal amount). However, if you use ore grades from individual mines, (or even the average grade from all currently operating mines) you are no longer measuring that response, you are measuring something else that is taking place at a smaller scale. At such smaller scale, the grade of the next bucket of ore, or the next mine to be developed, is independent of how much ore has already been extracted.	Such an approach would be highly academic, as we all know that mankind will not extract resources from the average earth crust. Our approach also does not deal with individual mines, but is based on the overall trends from all mines. Indeed fluctuations will occur, but the longer trend is not really disputed
D.1.4	Page 7	Te	"*,..the principle that mining easily extractable resources now will result in the extraction of resources in the future under more difficult conditions or with alternative technologies*". Again, as mentioned above, we do not think there is really a direct relationship causing such a "result". This is perhaps an "idea" worth exploring, but it cannot be described as a "principle" and we are concerned that this idea doesn't match particularly well with reality. The idea holds better if it is expressed as *",..**total depletion** of more easily extractable **ore types** now will result in the extraction of resources in the future under more difficult conditions or with alternative technologies*", but even then it is very difficult in practice to determine when a particular ore type is totally depleted. People are still mining pure native gold from rivers in Colombia today. In reality, all available ore types are mined simultaneously around the world in no particular order or preference of grades. Furthermore, this idea is challenged when considering the influence that input prices have on the difficulty of extraction (e.g., taxe rates, labour rates, oil price, exchange rates, etc.).	This comment has already been addressed in the comment 'Page 4 How to estimate available fossil resources'. Noted. This is a good point. We have looked into this and costs are often dominated by labour. We discuss this in the final deliverable and we will make recommendations for

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				future research regarding this topic.
D.1.4	Page 7 Cause-effect chain	Te	As mentioned above, we are concerned that the assumption that there is a cause-effect relationship between "metal extraction" and "decrease in ore grade" does not hold and will therefore lead to misleading results. Ore-grades (i.e., processed ore grades – the grade of the ore that is sent from the mine to the plant for processing) are a function of many different factors including metal prices, production costs, mining techniques, mining plans and the layout of the ore-bodies. The relative influence of each of these factors is probably dependent on the scale of the assessment. Layout of the ore-body is a major factor at the scale of the individual mine. Mining techniques are possibly the major factor at the global scale. Due to the cyclical nature of metals prices, we suspect that the SC over the long term may fluctuate around zero. New mining techniques must be competitive at global commodity prices and metal demand does eventually ease if prices become too high. So, a high Surplus Cost would indicate continued high prices ***and*** widespread use of expensive technologies, which in turn would be an indication of unusual scarcity of the material on the market. The question of whether or not the scarcity of a particular resource on the market should be included in the scope of an LCA, we will address in later comments below. We have submitted an alternative Figure 6 by email which we think better reflects real cause-effect relationships.	We have developed this cause-effect relationship with the knowledge of expert geologist from RMG, our partner in this task force. They acknowledge the limitations but still indicate this pathway as the most relevant from a geological point of view. We recognise that mining techniques are determinant for the production costs and production feasibility. However, we couldn't find data to address this. Also this is qualified for future research recommendation.
D.1.4	Page 7 Last paragraph	Te	The way the following statements are written makes them false. They should be corrected or deleted: "*Mineral resources have dissipative use meaning that they are available on Earth but often degraded or dispersed*" – a large proportion of mineral resources have continuous uses in built infrastructure and long-life products, which are readily available at the end of life for recycling. "*This way, mineral resources extraction will invariably continue and so will the decline of ore grades for all minerals worldwide, given no new discoveries (Mudd,*	We will adapt this sentence by changing "have" to "can have" at the beginning. The essential point we want to make is that there is dissipative use and thus demand for virgin material. This is again one big assumption of the method

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			2007)." – It is not at all realistic to consider "no new discoveries" as a given. New discoveries are a constant feature of the mining sector. Proof of new reserves within a given year consistently exceeds tonnes extracted. There is no evidence to support the statement that ore grades will decline "for all minerals worldwide". Perhaps what is meant is that the average concentration of all extracted minerals in the earth's crust will decline as more of them are removed for use above-ground? "*Ore grade is an inherent property of mineral resources and yet it also expresses the depletion of a resource.*" – this is only true in so far as the overall average grade of an individual ore-body is a specific characteristic of that ore-body, but it is ***not*** an inherent property of the mineral resource. Ore grade is too dependent on operating costs and prevailing metal prices to allow such a statement to be made. Secondly, ore grades only express the depletion of individual ore bodies in combination with the layout of the ore body and the mining plan. It sometimes happens that lower grade ore has to be worked through in order to access higher grade ore. This entire paragraph appears to be confusing "ore grades" with "average concentration in the earth's crust" – they are two entirely different things. The ore-grades presented in the paper are ***not*** an indicator of average concentration in the earth's crust, or of the depletion of global stocks of that resource. They are discrete samples of the grade of the ore sent from currently operating mines for processing over a certain time-period. It is not even clear that the ore-grades used represent the overall average ore-grade of each mine. We would expect that they represent the average ore-grade at each mine during a given reporting year.	which was discussed both in the extended deliverable and in the paper recently published by Vieira et al. At ES&T. Gerst (Economic Geology 103930:615-628, 2008) proved that the trend in copper grade decrease remains the same with new discoveries for porphyry deposits. We disagree. Ore grade is the physical concentration of a metal in a deposit and thus a physical inherent property regardless of it's being extracted or not. Noted, we will adapt this.
D.1.4	Page 8 1st Sentence	Te	"*As a consequence of mining larger volumes the production cost per metal/mineral extracted will be higher, defined here as marginal cost increase (MCI).*" Such a scenario would only occur if sustained high prices allowed the mining industry the luxury of expensive or inefficient production, which is seldom the case. (Some argue that this has recently been the case for gold and mining companies are now under shareholder pressure to improve the productivity of their gold mines so as to maximise returns on their investment). More usually, as	This point has already been addressed in the comment 'Page 7 Cause-effect chain'.

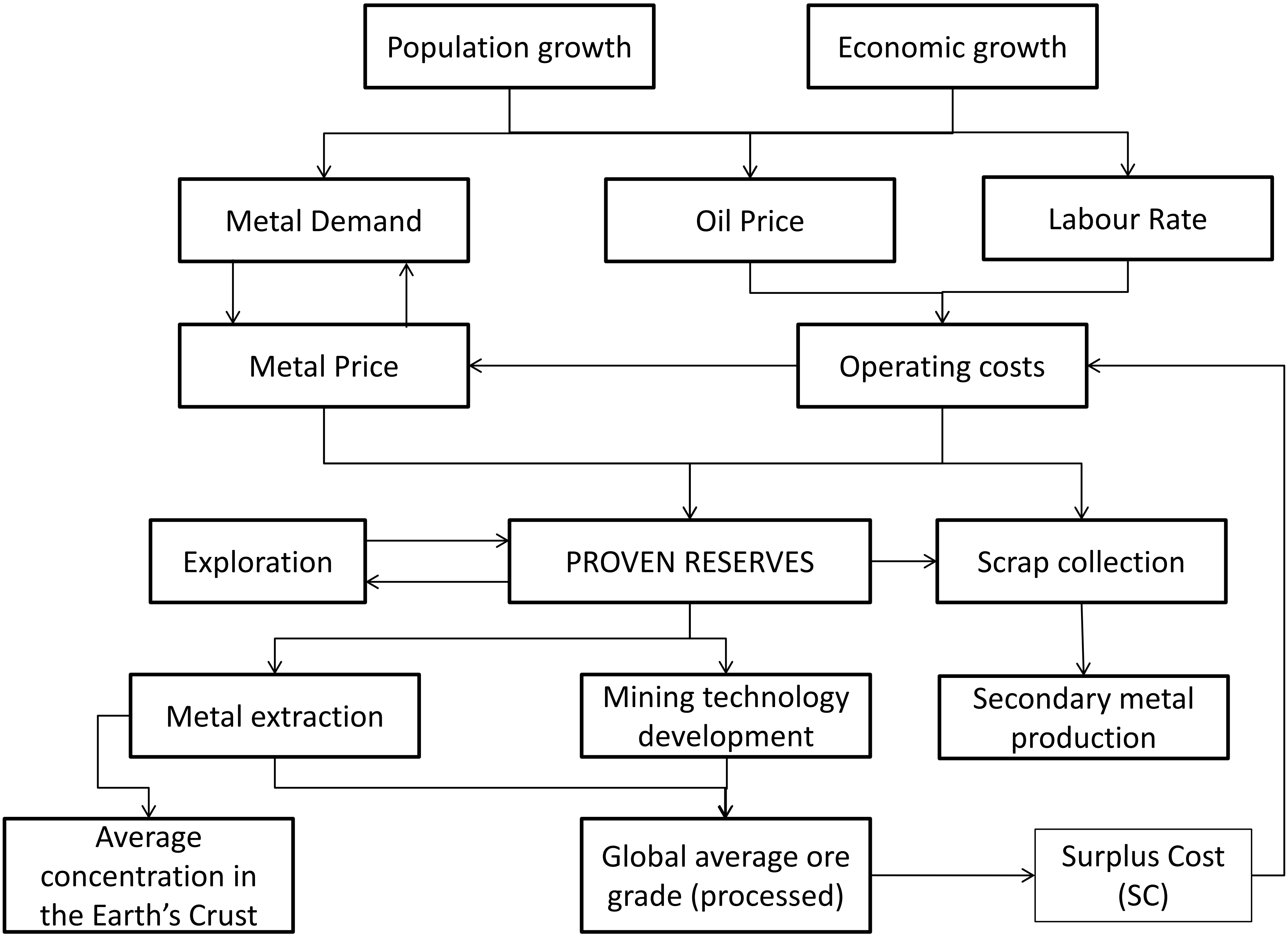
(1)	(2)	(3)	(4)	(7)
Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
			mining techniques improve, larger volumes can be mined at the same cost and accordingly lower grade ores can be sent for processing.	
D.1.4	Page 8 Method	Te	It follows from our previous comments that we think the wording of this section can be misleading. We suggest it would be more accurate to state: "***The first step in our method is to determine the overall decrease in ore grade at currently operating mines (OGD). The OGD is the marginal average decline in global concentration of a specific metal/mineral across all operating mines that would arise if the mines were worked out in order from the one with the highest average ore grade to the one with the lowest average ore grade***." When described more precisely in this way (assuming that we have understood the method correctly), it becomes clear that the method is assessing the consequences of overall depletion of the current set of operating mines *only*. In that sense, it might be providing some information about the **criticality** or the **scarcity** of the resource (adopting Dr van der Voet's definitions above). It is certainly not providing any reliable information about **depletion** of the natural resource stock globally. We suggest therefore that the method, the indicator and the associated Area of Protection should be re-named to accurately reflect what the method is doing.	Noted. As pointed out earlier, we will use the proper terminology in the final deliverable.
D.1.4	Page 9 1st Paragraph	Te	It follows from our previous comments that we think the wording of this section can be misleading. We suggest it would be more accurate to state: "*Midpoint CFs for mineral resource extraction were defined as* ***the marginal decrease in global concentration of a specific metal/mineral (in %) across all currently operating mines*** *with a marginal increase of amount of metal/mineral extracted (in kg) where x □g is the* ***marginal change in global concentration of a specific metal/mineral x (%) across all currently operating mines*** *and x □CMT is the* ***marginal additional tonnage of metal/mineral x (kg) extracted from all currently operating mines if the mines are worked out in order from the one with the highest average ore grade to the one with the lowest average ore grade***."	Noted.
D.1.4	Page 9 1st Paragraph	Te	Suggest rewording to: "*For the three major copper deposit types enumerated in Figure 7, we* ***tested for*** *two types of* ***correlation*** *(loglinear and loglogistic),*"	Noted.
D.1.4	Page 9 2nd	Te	It follows from our previous comments that we think the wording of this section	Noted.

(1)	(2)	(3)	(4)	(7)
Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
	Paragraph		can be misleading. We suggest it would be more accurate to state: "*The additional volume of rock extracted* ***if the mines are worked in order from the one with the highest average grade to the one with the lowest average grade*** *is then multiplied by the* ***average operating cost of all currently operating mines per volume of rock extracted globally***."	
D.1.4	Page 9 Cross-cutting issues	Te	It follows from our previous comments that we think the wording of this section can be misleading. We suggest it would be more accurate to state: "*The cause-effect chain of abiotic resource use starts with the extraction of abiotic resources from* ***currently operating mines*** *but the issue of concern has been indicated by stakeholders "to have enough and at affordable price" (Vieira et al, 2011).*" The method is not measuring depletion of a single mine. Neither is it providing any reliable information about **depletion** of the natural resource stock globally. It appears to be describing the overall depletion of the current set of operating mines *only* and not attempting to say anything about the total natural resource base.	Currently and past. The method was built upon global historical data from USGS. The loglogistic model used information on the reserve base so we do believe that the depletion of the natural reserves stock globally is being considered. The reserve base was estimated using real geological models from Wilkinson Kesler (Geology 115(6):611-627, 2007) but the models with mining data have shown to not make a big difference.
D.1.4	Page 10 1st Paragraph	Ge	In our view, this Surplus Cost (SC) method is actually a potential predictor of the resource "scarcity" or shortage of supply and *not* of anything else. If stakeholders are interested in "having enough at an affordable price", this method may provide helpful information, but we suggest that the method, indicator and associated Area of Protection should be re-named to accurately reflect what the method is doing. We also think that "having enough at an affordable price" is a socio-economic concern and not an environmental one. Because global average concentrations in currently developed ore-bodies provide very little information about global stocks of the resource, and because processed ore-grades at individual mines are heavily influenced by metal prices, production	Already addressed earlier in this document. Addressed in the previous comment.

(1)	(2)	(3)	(4)	(7)
Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
			costs, mining techniques, mining plans and the layout of the ore-bodies we consider that the whole method is providing socio-economic rather than environmental information and is therefore entirely outside the scope of environmental LCA. If "abiotic resource depletion potential" is to be addressed at all in environmental LCA, the characterisation method should be based on estimates of the global resource base. The grade of processed ores is an economic parameter - not an environmental one ("ore" is defined in economic terms) and is likely to be more useful for assessments of the **criticality** or **scarcity** of resources on the market in the shorter term. It may be worthwhile revisiting use-to-stock ratio approaches in order to assess the true **depletion** potential of an abiotic natural resource.	Already addressed earlier.

Thank you for your input!

The LC Impact Team,



Population growth
Economic growth
Metal Demand
Oil Price
Labour Rate
Metal Price
Operating costs
Exploration
PROVEN RESERVES
Scrap collection
Metal extraction
Mining technology development
Secondary metal production
Average concentration in the Earth's Crust
Global average ore grade (processed)
Surplus Cost (SC)

Public consultation
on
Life cycle impact assessment methods developed within LC-impact project

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Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
		Ge	Our organisation fully supports and endorses the statements submitted by our European industry association – Euromines	

Thank you for your input!

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Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task

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Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
D.1.4	All	Ge	Our organisation fully supports and endorses the statements submitted by our European industry association – Euromines	

Thank you for your input!

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other (please specify)	RTO (Research and Technology Organization)
Date	21.12.12
Consent for publishing (yes/no)	YES

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(1)	(2)	(3)	(4)	(7)
Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
D1.1.1	Soil erosion model and CFs on the global scale	technical	It is stated that the endpoint indicator uses for the CF a combination of the local soil depth and the emergy needed to regenerate the amount of soil loss during the land use. However, as better explained in the publication by Núñez et al. (Inclusion of soil erosion impacts in life cycle assessment on a global scale: application to energy crops in Spain. Int J Life Cycle Assess. DOI 10.1007/s11367-012-0525-5), for the indicator of soil resource depletion emergy units were used, as in the work by Rugani et al. (Sci Technol 45(12):5426–5433, 2011). In the latter publication, the authors accurately precise that: "SED is not equal to emergy, although they share the same conceptual rationale". First, SED includes allocation between coproducts of different nature, while emergy is defined by special algebra rules […].Second, SED does not account for a number of process inputs usually included in emergy analysis, i.e., human labor, information, and most ecosystem services […]" The solar energy factors (SEFs) given in Rugani et al. (2011) can be used for the calculation of the Solar Energy Demand (SED), which differs from emergy because its calculation does not comply with rules n.2 and n.4 of emergy algebra, listed for example in (Brown and Herendeen, 1996. Embodied energy analysis and EMERGY analysis: a comparative view. Ecological Economics 19, 219 235). I think the same clarification should be provided here as well.	Future soil availability (soil as a resource) decreases due to current land use (impact pathway 1 in fig. 1). The endpoint indicator uses for the CF the local soil depth (in meters) and results are expressed with units of megajoule solar equivalents, as in the work by Rugani et al. (2011). It is correct that this refers to SED, i.e. the environmental work needed for the formation of a natural resource. The incorporation of this concept adds innovation to the approach. Rugani B, Huijbregts MAJ, Mutel C, Bastianoni S, Hellweg S. 2011. Solar energy demand (SED) of commodity life cycles. Environmental Science & Technology, 45(12): 5426–33.

(1)	(2)	(3)	(4)	(7)
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think-tank	
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private person	
other (please specify)	Private Research Company (non-profit)
Date	23/12/2012
Consent for publishing (yes/no)	Yes

Data: Name: Cecilia Askham Affiliation: Ostfold Research AS

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Task number	Paragraph/ figure/table/ note (e.g. table 1)	Type of comment (ge: general; te: technical)	Comment	Comment' answer by task
		General	The LC-Impact documents give a good overview of the work, as well as state of the art for the different impact assessment methods, with literature references. This overview is very usefull for those working with LCA and using impact assessment methodology. The documents are a good length, giving an appropriate amount of information about each subject for this purpose.	We appreciate the encouragement.
All activities where spatial differ-entiation is considered		General and for D2.5	As with all impact assessment methods it is important that the inventory data available can be used in combination with the impact assessment methods. Spatial differentiation is clearly important for the magnitude of the impacts. This is the case for some impact categories more than others. The variation can be large within impact categories, depending on the ability of the given region to cope with the given emissions/impacts. It is clear from the documentation on the LC-Impact site that spatial differentiation is extremely important if accurate results are to be achieved. Spatial differentiation of impact assessment requires special differentiation in the inventory data. This is possible in theory, but hard in practice today. An example could be a company producing an item for sale, where their suppliers source their materials on the international market. The materials list for the given product is provided, and all of the producers of components can be contacted and inventory data from their production process gathered. However these suppliers source their raw materials (e.g. plastic granulate) on the international market and cannot say where exactly it is produced. Inventory data is available in databases in the software tool used by the LCA practitioner, but this is general data, representative of the plastics industry (which themselves does not provide country-specific, or site-specific production data). It is important that the methods are developed for the ideal data availability situation, as this drives development of adequate detail in inventory data, but there should also be a set of pragmatic factors to use if the spatial differentiation data is not available for the inventory.	We are of course aware of the current limitations in LCI databases when it comes to spatial differentiation. The new version 3.0 of ecoinvent will be spatially explicit and contain information on the geographical placement of an elementary flow, hence opening the door to spatially explicit LCA. Since not all elementary flows of a LCA should necessarily always be treated in the same spatial detail, but rather only those with major contributions to a relevant impact score, the practitioner should focus on providing spatially explicit LCI data especially for those highly contributing flows. This requires in any case that generic (global average) CFs are available. These will be complemented by spatially

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				explicit CFs for one or several spatial resolutions depending on what is meaningful and feasible.
D 2.5			In the EDecIDe research project (which Ostfold Research has, as part of the activity financed by the Norwegian Research Council's Climit Programme) is attempting to perform some case study work on spatial differentiation in collaboration with industry and the Norwegian institute for air research (NILU). A specific site is being considered. This could be interesting to know about for the further work in the task. Mark Huijbregts is aware of this work and has made some contribution to this.	We appreciate the information. However, the developments in this task are now finalised and no further work is foreseen within the LC-IMPACT project.
D2.1		general	Toxicity of metals is still very much under discussion and there are often difficulties in presenting the results of toxicity assessments, due to the order of magnitude difference in the metals impacts compared to the organics. Moving the USEtox method forward from an "interim" set of metal characterisation factors would be very useful.	We agree very much, but this decision is in the hands of the USEtox developers. We will certainly submit our developments to them for consideration. However, please note that our work concerns terrestrial ecotoxicity, which is currently not modelled in USEtox.
D2.1		general	Time perspectives are important and addressing finite time horizons (as mentioned in the document) is important.	For metals this is very difficult, since there is no scientific basis for any sort of discount rate on their long-term impact. It remains a subjective value choice whether or not to consider longer time perspectives and how to interpret them relative to shorter time horizons. Proposing new developments for this particular point is beyond

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				the scope of LC-IMPACT, but is covered by other currently running research projects (e.g. at DTU).
D2.1		general	The text on page 3 asks for stakeholder's feedback on approaches to derive global generic values where emission location is unknown. This relates to my comments above about the spatial differentiation of inventory data. I agree with the authors that it is important to address this issue.	We appreciate the encouragement.
D2.1		te	Some small grammatical issues I noticed (although I did not read through the documents with this in mind, so there may be others): Page 1, 2nd paragraph. There is a small grammar error on line 2, which should read: "...of which the majority are recognized to be...." Page 3, under Cross-cutting issues: "...The regression equation can be used to calculate impact potential from emission of airborne metal in specific location, as.." should be "...potential from the emission" or "potential from emissions..." I also think it should be "metals" and please consider changing to "locations" or to "at a specific location".	We appreciate the comments and will strengthen our efforts to avoid grammatical issues in the final deliverables. It is however not foreseen to revise D2.1.
D2.2		General	This approach (TOC related) can help to include effluent streams that previously could not be included, due to their general nature (specific substance emissions not given). Thus it is very useful that this work is being done.	Thank you very much for your comments. We look forward to sharing with you the results of the test-of-concept study of this methodology.
D1.4		General	LCIA methods do not consider the difference between resources that are used for energy purposes (e.g. oil as fuel) and resources used for materials (e.g. oil for plastic). The fuels are consumed and not available for further use, whereas materials can have several lifetimes through re-use, or recycling. Environmental footprints and EPDs being produced in many countries require the LCA to produce results where these types of resource use are shown separately. I can provide references in this issue if needed. This is relevant for the LCI step, or "Resource use" part of Figure 1. This comment is perhaps more relevant for the database providers and inventory data providers from organisations like Plastics Europe and EcoInvent. The method presented in the D1.4 document is based on financial indicators and the focus is impact assessment, of course, not inventory, but consideration of the difference between resources used "in a destructive or	Thank you for your pertinent comment. In the inventory phase, it indeed matters whether recycled content or virgin material is used as an input. This is considered by most commercial/public databases. Additionally, we are aware of inventory data reporting. However, this is not a result of impact

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			dissipative way" as described on page 1, instead of resources used for other purposes is an important issue.	assessment thus it was not in the scope of the LC-IMPACT project. However, this reporting can be done by LCA practitioners in any case. For the final deliverable we will include one comment stating that this difference in life cycle inventory data must be done.

Public consultation
on
Life cycle impact assessment methods developed within LC-impact project

Draft document for public consultation can be downloaded at:

www.lc-impact.eu/consultation

Deadline for comments: 23 12.2012

Please send the filled form to: lc-impact@science.ru.nl

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D.2.2	Conclusions	Ge	The WET approach is a good simplification that will enable toxicity to be included in a larger number of studies. My sole concern is that the usefulness of the generic data depends on the goal of the LCA study. At Swerea IVF we do a lot of LCA studies to support product and process development for companies. If the goal with the study is e.g. process development or product development including substitution of hazardous chemicals, I assume that the result with the generic WET data for the case before substitution will be the same as after the substitution. The same will be the case for reduced amounts of emissions of chemicals, if industry average is still used both before and after reduction. I therefore would like to suggest that the modelling will be editable, maybe in a similar way to unit processes in Ecoinvent. Then they would be open for modifications so that single chemicals can be excluded from the total and then added in different amounts in order to create own estimations for specific sites with different technology level and to make environmental improvements visual in the results.	Thank you very much for your comments. The process of single chemical substitution aiming at product/process development support that you describe can already be done within the framework of LCA, e.g. via the USEtox model, which provides freshwater characterisation factors for over 3000 chemicals. The main strength of this work task though is that all the chemical interactions in the effluents are taken into account – this cannot be done via consideration of chemicals on an individual basis. The aim of this work is to provide industries with a way (albeit rough) to assess the ecotoxicity of their effluents within LCA via a process parameter (TOC) that is already widely adopted, thus circumventing the need to measure all the single chemicals in their complex effluent (costly and laborious!). Within the greater frame of life cycle impact assessment, this

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				would then allow industries to assess their waterborne chemical emissions in a more comprehensive manner, and potentially compare it to the impact from their emissions in other impact categories. It will also eventually allow for a more holistic comparative assessment of the ecotoxic impact of bulk organic waterborne emissions from different industries (that have more or less constant processes and hence effluent profiles: this approach would probably fail in the case of e.g. certain chemical manufacturing industries whose emissions profile change even on daily basis)

Thank you for your input!