This contribution is a slightly modified version of my comments sent by e-mail in October 1998 to the Editor Team of SETAC’s Working Group on Impact Assessment (WIA-2), on their draft document about impact categories and category indicators that they had circulated in July 1998. People related to the Global LCA Village suggested to submit my comments to their website as well.

Stated in maybe too simple terms, my main remark is that the idea behind WIA-2’s draft document of July 1998 reflects a high faith that we can, somehow, perform a meaningful modelling of emissions, fate and final effects of substances. The main question seems to be how to get some kind of quantification of the relations between the interventions and the final endpoints/effects on areas of protection in figure 2 in that document. In this sense, the document seems to me mainly heading towards a refinement of the approach that has been used now for years in the LCA community.

Those who saw my lectures at SETAC conferences in Brussels and Bordeaux, or know my thesis work, will not be surprised I gradually lost my belief that we can ‘model the world’ in a uniform, and still meaningful way. For instance, in a world where we use 100,000 substances, but where we have even for the 2,000 most used substances only in 5% of the cases just screening data available for toxicity and fate, it seems to me quite optimistic to think that the DALY’s or PAF’s you calculate will ever have a high faith that we can, somehow, perform a meaningful modelling of emissions, fate and final effects of substances. The main question seems to be how to get some kind of quantification of the relations between the interventions and the final endpoints/effects on areas of protection in figure 2 in that document. In this sense, the document seems to me mainly heading towards a refinement of the approach that has been used now for years in the LCA community.

I will not repeat the findings of my thesis here, but the following point is noteworthy. Literature from policy science indicates that evaluation methods that lack a fundamental robustness will end up in a Catch-22 situation. As long as actors who have to use the result don’t feel the need to struggle over a certain topic, they will accept almost any outcome of the evaluation method. But when the situation is already very polarised and controversial, there is always an actor coalition who will attack the result – with success (Collingridge and Reeve, 1986; see also Eijndhoven and Groenewegen, 1991; Jasanoff, 1990). In sum: when there is no controversy, no one feels the need to scrutinise the evaluation method and its results are accepted; but when there is a real controversy, in which the evaluation method is necessary as a judge, it proves to be too weak. The latter was exactly what happened when I applied LCIA in tense studies on chlorine, PVC, and hazardous waste management. In these studies, only indicators that had their foundation quite to the left part of figure 2 of the document (i.e. emissions and large mass flows), proved to be so robust that they could be used to structure the tense debates on these topics.

Against this background, I question the emphasis on linking interventions with protection areas by mathematical models as the single best approach in LCIA. Of course one could say that the experience in my studies is only an experience in just a few studies. Yet, it were projects meant as all-out efforts to solve the problems and debates related to major societal question points like chlorine, PVC, and waste, so I believe these experiences should not be too easily rejected (Tukker et al., 1995, 1996a and 1996b). Therefore I think it is useful that WIA-2 discusses about hard questions like: where in figure 2 can we still rely on robust science? Where about ignorance becomes dominant? Do we really want to accept that we are ignorant in certain areas? How do we want to deal with ignorance? What does that mean for the basic philosophy and approach behind the LCIA method we want to develop? In this respect, it may be useful to indicate that a main conclusion in my Ph.D. thesis on the analysis of the chlorine controversy is that a main point of discussion between environmentalists and industry concerns the importance of ignorance in fate and effect modelling!

Personally, I believe it will never be possible to solve controversial discussions about products with an LCIA method that is based solely on mathematical relations between interventions and protection areas. There are simply too many uncertainties, there is too much ignorance, and they can only be overcome by all kinds of subjective, subtle, and basically value-laden choices. Policy sciences has convincingly shown that such choices in most cases hardly can be made explicit. Actor coalitions will implicitly choose a way of ‘making sense’ or ‘frame’ such situations which can not be unambiguously determined by scientific knowledge (cf. Fischer and Forester, 1993; Schön and Reim, 1994; Weinberg, 1972). As indicated by a reaction on José Potting (1998) on the document, this is
not only true for human toxicity and eco-toxicity (which dominate my work), but also for other environmental themes.

In my view, there are only three ways out of this problem:

1) The choice is made for a reductionist approach to LCIA. This means that LCIA modellers acknowledge they can’t come up with overall answers, and just produce a tool that generates limited data – but which are truly robust.

2) The choice is made to include the inevitable subjectivity in the LCIA method, by developing multipler indicator systems that reflect the views of the most important societal actor groups. Developing an LCIA method based on cultural theory is one option (cf. Romans and de Vries, 1997; Hofstetter, 1998); personally I believe that using cultural theory alone may be insufficient, and it will be very necessary to analyse the views of actor groups with regard to an environmental problem type before these views can be modelled (cf. Tukker, 1998c).

3) More participatory forms of LCIA are developed, that allow for learning processes between societal groups with differently (biased) views, resulting in a more balanced (thus better) and more societally accepted product evaluation (e.g. Bras-Klapwijk and Ensérink, 1997).

As for the solution to produce a method backed by an ‘authoritative’ forum, I feel quite reluctant to support that. In situations where science lacks robustness, there will be inevitably pluriformity in views in society. Such an authoritative forum will inevitably overrule this pluriformity. This is not only a problem from a democratic viewpoint (see e.g. Weinberg, 1972). More important, by using such a method policy advisers and scientists will most probably stay unaware of the fact that the pluriformity of basic views plays the key role in the controversy or problem they want to solve.

References


Potting, J (1998), E-mail with comments on the July draft document for WIA-2 on impact categories and category indicators. Probably still available from the author at: Jose.Potting@rivm.nl


Tukker, A. (ed., 1996a), Milieu-effectrapport Meerjarenplan Gevaarlijke Afvalstoffen 1997-2007 [Environmental Impact Assessment Multi-year Hazardous Waste Management Plan 1997-2007], Ministry of Housing, Physical Planning and Environment and the Inter-Provincial Union, the Hague, the Netherlands (400 p.). Review by an expert panel on behalf of the EIA Commission, Utrecht, the Netherlands


Weinberg, A. (1972), Science and Trans-science, Minerva, Vol X. Nr. 2