Preface

It is customary for a volume like this to start with a preface. I have not had a course in preface writing ('preface-ology 101'), but I suppose the preface must be analogous to materials given out on freshman orientation day, where a new college student learns where the most important college institutions are to be found, such as the student union, the gym, the football stadium, the dormitories, the dining hall and – incidentally – the library, the bookstore, the lecture halls and the chemistry lab (where that hydrogen sulfide smell seems to be coming from).

The confusion is compounded by the fact that there are two of us, but only one (RUA) is writing this. Thus the pronouns will be seen to wander erratically.

I have put it off until the very last moment in hopes of some inspiration. But the sad fact is, nobody ever quotes from, or remembers what is written in, a preface. I suspect that nobody ever reads prefaces. I don't. Why, then, should I write one?

Thinking out loud (so to speak), is the preface needed to define the subject? We have assigned the opening chapter of this volume to two authors who clearly believe in formal definitions and statements of purpose, and who have been instrumental (with others) in creating a formal graduate program in industrial ecology, and a professional journal in the subject. Apparently IE is now a subject. Our own view of what belongs within the boundaries of IE is implicit in the structure of this volume. Readers will note that this tends to lean towards inclusion rather than exclusion. Enough said.

Should the preface be used to provide the historical background to the subject of the volume? Again I think not. There are two chapters (numbers 2 and 3) explicitly included for this purpose. They do the job better than we could. Shall I use the preface as an opportunity to pontificate on the future of our subject? The trouble is I can't believe the future cares what I think. (For that matter, I don't care much about what the future thinks.) So, I have no inspiration along those lines.

Is the preface needed to explain the origin of the volume itself? At one level, that is easily covered in nine words: the publisher asked us to do it. We agreed. Why did we agree? I'm still asking myself that one. Perhaps it was for the big money. (I don't want to embarrass him by calculating exactly what that worked out to per hour.) Well, there's also our leather-bound copy inscribed on vellum and signed by Edward Elgar personally in gold ink. He hasn't sent that yet, but we're saving a space for it.

Seriously, I suppose the main reason we took it on was because I think the subject is finally coming of age. If it is time for a journal and a professional society (in progress), then it is also time for a *Handbook* (perhaps, soon, even an *Encyclopedia*) to bring together the leading thinkers and practitioners in the newly emerging field and give them a chance to present 'the state of the art' between two covers. In short, somebody had to do it and I've been around the subject longer than most. The other reason was that I wanted to give myself an excuse to read and understand all the stuff I've been accumulating on my shelves for a decade or more, especially a number of relevant PhD theses

that have appeared in recent years. Summaries of several of them appear in the following chapters.

Having done it, have I learned anything worth passing on to the next generation of editors? I don't really know what is worthy of passing on, but a couple of subversive thoughts have struck me about what I would try to do differently next time, if there were a next time (perish the thought).

The first of my subversive thoughts is that the subject of data gathering and data processing is routinely under-represented. By the same token, 'modeling' is equally over-represented. For some reason young PhD students are brought into the world under the impression that their task is to massage a pile of putty-like data into a model, which can then be baked in an oven (so to speak) and take its place with hundreds of other oven-baked models stored in a warehouse (library), where they will be checked out from time to time and marveled over by model connoisseurs. Editorial observation: in reality, most soft models are used but once, usually to secure a PhD degree, and never seen or heard from again. On the other hand, lowly data are likely to be recycled many times, often by people who have forgotten or never knew the source. The great danger, in this, is to confuse 'raw' data with 'model' data.

Of course, in established fields, like electrical engineering or chemical engineering, much effort has gone into distinguishing the two categories. Raw data are obtained by direct measurement. Processed data are picked over to eliminate outliers and averaged or otherwise modified. Model data are calculated from processed data. There are handbooks consisting almost solely of data series, revised and updated at regular intervals by committees of experts.

IE lacks any such tradition. There is no clear distinction between 'hard' data and 'soft' data. In recent years there has been an outbreak of national mass-flow studies, a number of which are represented in this volume. Missing from those studies, in general, is any discussion of the sources of the data, or the reliability of those sources. Particularly absent is any distinction between what is calculated (and how), what is measured (and how) and what other choices might have been made. Hardly any reader would realize that mass-flow data are rarely measured (or measurable) directly, and that each beautiful flow chart is, in fact, a model. No problem, except that the models used are presented as data. Detailed discussions of sources and assumptions are mostly absent.

As a rough generalization, one end of each mass flow is calculated or estimated based on some model, whether explicit or implicit. If the inputs to an industrial process are well known from measurements, the outputs (especially emissions) are generally estimated by means of some combination of mass balance and process analysis. If the outputs of a process are counted (or surveyed) accurately – for instance, food consumption – it is still very uncertain how the outputs are derived from raw material inputs. In principle, every mass flow should be verified both 'top down' (from aggregate data) and 'bottom up' from process-product data. In practice, we are a long way from this ideal situation. In short, as the field of IE matures, it will be necessary to do something about classifying and improving the underlying data bases.

Another observation: there is a remarkable tendency on the part of many authors to justify their work in terms of 'policy needs'. I do not doubt that policy makers at all levels need better models (and better data). However, the majority of the models discussed in this volume are far from being either widely enough accepted, or user-friendly enough, to be immediately useful to policy makers now. This is not a criticism. It is where the field

stands. What is missing, and badly needed in my opinion, is a greater focus on the gaps. What data are missing but needed? How might they be obtained? What models are in need of verification? How might it be done? What models are in need of radical improvement? How might it be approached? What models are likely to be misleading or misused? How can that be avoided?

A few other remarks of a more mundane nature come to mind. It is customary for an editor of a large multi-author volume such as this to appoint up to half-a-dozen associate editors. This group then either reviews the draft articles themselves, or farms them out for review, usually to other authors. Since the group is largely self-selected and mutually acquainted through years of meetings and conferences, authors can often guess who is writing the anonymous review. In consequence, reviews by colleagues tend to be quite bland and uncritical. Since cross-reviewing is an unpaid chore, it is often at the end of a long queue. Then the reviews are collected by the editor, who adds a few words of advice (but the article is already accepted, by definition) and are sent back to the authors.

Books edited in this way seldom appear in print in much less than three years after the original idea of the book is broached or accepted – as the case may be – by the publisher. The long lag time is, in itself, one of the disincentives to promptness on the part of experienced authors, who know that, however late they are, they will not be the last. Usually, they are right, since only one author can be the last and there is no booby prize.

In this *Handbook* we have tried hard to break the pattern. The *Handbook* idea was proposed by Edward Elgar about a year ago. We solicited our authors, suggesting specific topics, in March 2000. All but a few accepted quite promptly. A June deadline was proposed, on the argument that, if you are going to do it, do it, sooner is better than later. We expected some delays, of course. Only about half of the authors actually met the deadline, but in July we were able to start on the hard part, which was to read each of the drafts critically. In almost every case, significant cuts were requested (and, in a few cases, small additions.)

For the non-English speaking authors, we soon found myself doing what newspaper and magazine editors routinely do, which is to rewrite. Not to change the authors' intention, but to express it more clearly and more efficiently, in (many) fewer words. In a few cases the rewrite was fairly drastic. Of course, the rewritten articles were sent back to authors for approval or further changes. Most were polite about it, and some even thanked us for our efforts (for which we duly express our gratitude here and now). If there is any author who feels that we trampled over his/her 'rights' of free expression, but who kept quiet about it, we can only apologize for hurt feelings and point out that the space limitation was necessary and we didn't enjoy doing the extra work either. I do believe the result is more readable, however.

One other innovation, also to save space, was to move all the references to the end of the volume in a single composite list. This cost quite a bit of work, but makes a better result.

This is probably the place to mention my personal regret that several of the pioneers of industrial ecology are not adequately represented in this volume, except insofar as they appear in the citations at the end. I will not mention specific absentees for fear of offending others. A few of you were invited and begged off. (Probably you are already famous enough.) A few others I missed for various good reasons, such as lack of an address, or bad reasons such as plain and simple oversight. So, here's another apology to absent friends.

ROBERT U. AYRES