An account of a symposium on *Research Methods* on the occasion of the retirement of Herman Adèr

Friday, 20 May 2005

During my welcome at the start of the symposium on Research Methods on the occasion of my retirement from the department of Clinical Epidemiology and Biostatistics at VU University medical center (VUmc), I vowed not to participate in the discussion. The reason was that I feared that anything I *would* say risked to be met with polite, if hesitant approval of the words of the guest of honor. That was the reason to show the joke in Figure 1. In this extended handout I take the opportunity to give a concise account of what I wanted to say but could not, due to this self-imposed restriction. In particular, I will extend on topics discussed during the Summary I gave at the end of the symposium on May, 20^{th}, 2005 [1].

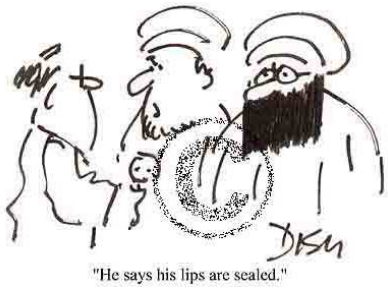


Figure 1: Promise made during the words of welcome.

[1] All abstracts, background articles and presentations can be found on the `Johannes van Kessel` website: `http://www.jvank.nl/Symposium/` (See also the Appendix).

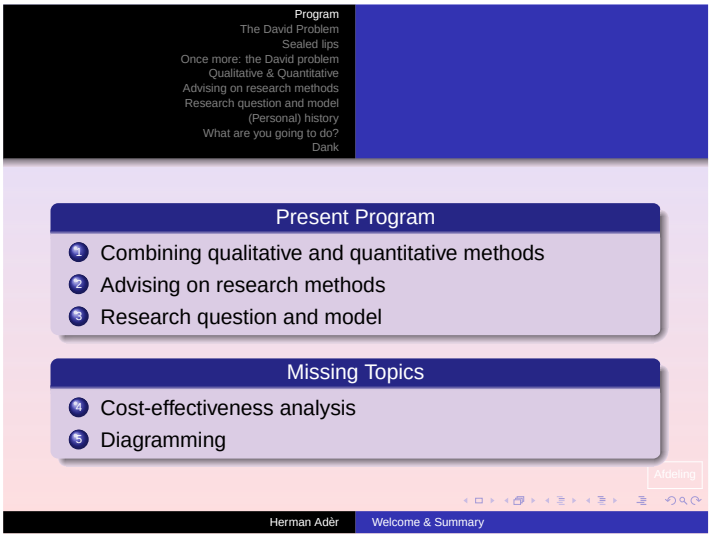


Figure 2: The Present Program and Missing Topics.

This handout has the following content:

1. The Program and missing topics.
2. The David problem
3. Statements: (i) On qualitative & quantitative methods (ii) On advising on research methods (iii) On research question and model (iv) Aristotle on research methodology
4. Consistency and Thanks

(The first two items came up during the Welcome address, the other two during my Summary.)

1 The program and missing topics

What you got

The symposium had three sessions, depicted in the upper panel of Figure 2. Below some elucidation why these themes were included.

Combining qualitative and quantitative methods.
I have always been intrigued by the gap that seemed to exist between those two worlds: the world of Qualitative Research and the world of Quantitative Research (see Figure 3). Of course, I have been primarily brought up to think about research in a quantitative way. But during a long period as a 'statistical' advisor, two things became more and more clear:

Figure 3: Tension between qualitative and quantitative approaches.

1. Advising on quantitative methods has much less to do with Statistics than measurement methods, research design, modelling and an astute interpretation of the results. In this respect there is no difference between qualitative and quantitative approaches.

2. Complex phenomena, in particular those involving dynamic changes, are extremely difficult to properly investigate (and, therefore, model) in a quantitative way. Although qualitative researchers usually don't claim that their methods allow to investigate such things, their way of data collection offers much more information on detail.

For instance, if one wants to study the decision process in an intensive care unit, it is hard to come up with a proper quantitative research design. With qualitative research methods you can get closer: the result will not be a complete description of the process, but an elicitation of the essential elements.

It seems only natural to combine both approaches, but at a practical level this is easier said than done due to the differences in culture and experience that exists between those two methodological orientations. My interest in diagrams (Adèr, 1999) inspired the idea to try and use diagrams to represent qualitative results: this may help to clarify what the results really are and at what points further research (be it qualitative or quantitative) may be needed. Together with Anne-Mei The

and Roeline Pasman we had a paper on this subject at the research methodology workshop in 2003 (Adèr, The, & Pasman, 2003).

Back to the present session. It was such a pleasure to get the opportunity to invite the two speakers in the session. Robert Pool has vast experience as a qualitative researcher interested in what is called 'mixed methods'. Riekie de Vet, who, as a researcher, used quantitative methods until now, became interested in applications of qualitative methods, for instance in Clinimetrics. She described possibilities and experiences with qualitative designs and results in her particular fields of research: musculoskeletal disorders.

Advising on research methods.

Most of you will not be amazed that this session was planned, in particular those who have attended the first and/or the second workshop on research methodology. The first KNAW-organized workshop in early 2000[2] was held at the occasion of the publication of the book 'Research methodology in the behavioral, social and life sciences' in 1999 by Sage (Adèr & Mellenbergh, 1999). The second workshop (Adèr & Mellenbergh, 2003) had a session on Advising although this was considered a funny idea by some. Don Mellenbergh, David Hand and myself consider the topic of advising on research methods a direct application of ideas put forward in the Adèr and Mellenbergh book. These were later concisely summarized in Mellenbergh et al. (2003). Roughly put, we think that different areas of research use similar methods both to collect data and to analyze them.

We also agree that, although thorough knowledge of Statistics is quite helpful, it is not sufficient for the job of giving advice. All these considerations lead the three of us to start writing a 'Handbook for the advisor on research methods' entitled: 'Advising on Research Methods', which will be published by Springer NY this year (2006).

The difference between a purely statistical point of view and a research-oriented point of view became clear in the lecture by David Hand entitled 'Deconstructing research questions', a slight variation on his famous article 'Deconstructing statistical questions' held before the Royal Statistical Society (Hand, 1994)[3]

Don Mellenbergh delivered an excellent lecture on the construction and analysis of questionnaires. This topic gave some new perspectives that may be of particular interest in Epidemiology and the Health Sciences where the systematic development of questionnaires is a new research area. It seems that some profit

[2] proceedings: `http://www.knaw.nl/rm/index.htm`

[3] This article can also be downloaded from:
`http://www.jvank.nl/Symposium/Articles/dhdeconstructing.pdf` (See also the Appendix).

could be gained from the abundant research that has been done in Psychometry where Don has left his stamp.

Research Question and Model.
The third session was devoted to two important issues in modelling: (a) How to find a model that corresponds to the research question; (b) If more models seem plausible, how to either (i) combine them, or (ii) compare them, or (iii) investigate the complete model space.

On the first question, we wrote a paper for the 13^{th} workshop on Statistical Modelling, held on Crete in 2002 in which we tried to formalize the activity of methodological modelling (Adèr, Kuik, Hoeksma, & Mellenbergh, 2002).

Jan Hoeksma's provocative and entertaining presentation tried to show that, in the practice of giving methodological advice, the approach is different from what is assumed in research methodology: advisors (like myself) try to mould the research problem to one of the statistical models they know. Furthermore, he doubts if the enterprise of eliciting the general elements in research methodology as applied in different disciplines can ever succeed: approaches may be too diverse and the methodology applied may be too different.

The second topic ((b) above) is the subject of several papers extending the original proposal of Edwards and Havránek (1987) for a general procedure to search a model space (See Adèr, Kuik, & van Rossum, 1996; Adèr, Kuik, & Edwards, 1998; Adèr et al., 2002[4], but many others have contributed to the subject). In his presentation ('Model search and model uncertainty'), Edwards showed how to obtain estimates that are free from model selection bias. Important in this respect is the concept of *propensity scores* which can be used to control overt bias by using the association of covariates with treatment assignment (Rosenbaum, 2002) [5].

What you did not get

The lower panel of Figure 2 on page 2 gives the topics I would have liked to include, but did not't. This is due to a lack of time slots, not to a lack of interest.

As to the first subject, *Cost-effectiveness* analysis, I have always found this a fascinating subject. From a methodological point of view, it is a challenge to balance cost and effect considerations. Apart from that, before designing a study, it is absolutely essential to formulate the *perspective* from which it is undertaken and from which the results will be interpreted and used for decision making. In

[4] See also:
`http://users.keyaccess.nl/~aderh01/KEB/hja/down/leidenhandout.pdf`

[5] A handout on the way propensity scores are calculated can be obtained at: `http://users.keyaccess.nl/~aderh01/KEB/hja/down/Propensityhandout.pdf`

this way, subject matter considerations play an important part right from the start of any study in this area. Note that for the statistician this is an intriguing area, too, in which several very hard problems still remain unsolved.

I did some work in this area. I developed a computer program to calculate confidence bounds for the cost-effect ratio[6]. This was based on the work of a study group on the cost-effectiveness ratio at the Department.[7]

The second subject that was left out, is *Diagramming*. Since my thesis (Adèr, 1995, Chapter 5), this subject has interested me, in particular the question whether it is possible to develop a diagramming system with which methodological notions and procedures can be represented. The book by Adèr and Mellenbergh contains a chapter on this (Chapter 3), and in a presentation entitled 'Ambiguity in diagrams' given at the first workshop on Research Methodology, I discussed pitfalls in diagramming, in particular *ambiguity*[8]. My last paper on the subject was Adèr, Pasman, and The (2004), which emphasized the diagramming aspects of the RM2003 paper mentioned before (Adèr et al., 2003).

[6]Downloadable from: `http://www.simtel.net/product.php?id=71029`

[7]Several other papers on related subjects can be found in the 'Downloads' section of my personal page: `http://users.keyaccess.nl/~aderh01/KEB/hja/root.html`

[8]See: `http://www.knaw.nl/rm/ader01.htm`

First name	Last name	Profession
Robert	Pool	Anthropologist
Riekie	de Vet	Epidemiologist
David	Hand	Statistician
Don	Mellenbergh	Methodologist
David	Edwards	Statistician
Jan	Hoeksma	Methodologist
Maarten	Boers	Epidemiologist/Rheumatologist
Tony	Hak	Sociologist
Gerrit	van der Wal	Social Scientist
Anne-Mei	The	Anthropologist/Jurist
Hilde	Tobi	Social Scientist
Bernard	Uitdehaag	Epidemiologist/Neurologist
Chad	Gundy	Methodologist/Psychologist
Joop	Kuik	Statistician/Methodologist
Hans	Berkhof	Statistician
Martine	de Bruyne	Epidemiologist
David	Cox	Statistician

(a)

First name	Last name	Profession
Anne-Mei	The	Anthropologist/Jurist
Bernard	Uitdehaag	Epidemiologist/Neurologist
Chad	Gundy	Methodologist/Psychologist
David	Hand	Statistician
David	Edwards	Statistician
David	Cox	Statistician
Don	Mellenbergh	Methodologist
Gerrit	van der Wal	Social Scientist
Hans	Berkhof	Statistician
Hilde	Tobi	Social Scientist
Jan	Hoeksma	Methodologist
Joop	Kuik	Statistician/Methodologist
Maarten	Boers	Epidemiologist/Rheumatologist
Martine	de Bruyne	Epidemiologist
Riekie	de Vet	Epidemiologist
Robert	Pool	Anthropologist
Tony	Hak	Sociologist

(b)

Figure 4: (a) Names and Profession of the active participants of the symposium; (b) same table, but sorted on first name.

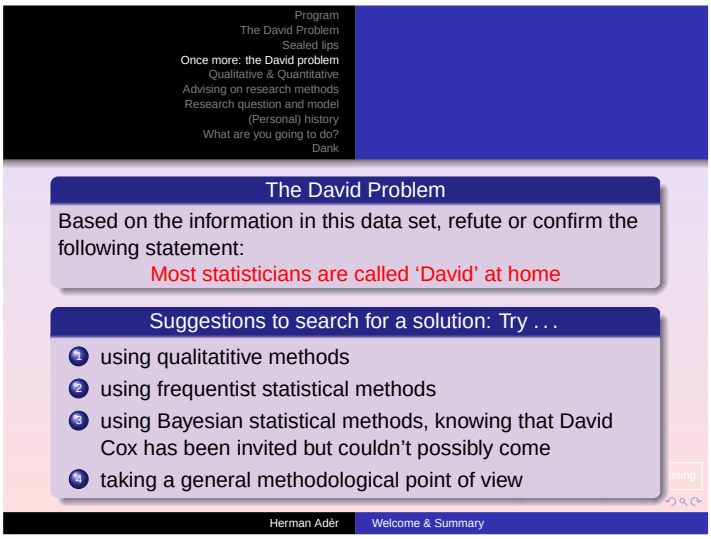


Figure 5: Formulation of the David problem.

2 The David problem

During my Welcome address I presented a problem which I called 'the David problem'. It was later on discussed in the Summary.

Figure 4 (a) gives an overview of the active participants of the symposium and their profession. The name of David Cox is set apart: he was invited and had liked to participate, but he could not come since he had other obligations. The other panel (panel (b)) gives the names once more but now ordered on first names. Funnily enough three of the five Statisticians are called 'David'! And on this observation the 'David problem' is based, formulated on the slide shown in Figure 5. As you can see I invited the audience to try to look for a solution using methods from different disciplines (the lower box of the slide).

Apart from David Hand who started his presentation with a funny story[9] that confirmed the David Conjecture (*'Most statisticians are called 'David' at home'*), none of the speakers discussed the problem. The general feeling of the audience seemed to be something like:

[9]The story was situated in a hotel at breakfast previous to a Statistics conference. It turned out that there were not three but at least *seven* statisticians called David sitting at the same table, which lead to Monty Python-like conversations: 'Slept well, David?' 'Yes, very well, and you, David?' 'Can you pass me the salt, David' 'Here it is, David' and so forth. . .

A fool may ask more questions in an hour than a wise man can answer in seven years.

It is exactly *this* feeling that the Statistical/Methodological advisor may experience when his or her client puts a, perhaps quite plausible, question that is, however, absolutely insoluble using the available data[10].

One may ask what approach practitioners from different disciplines would take when confronted with the David problem.

Any qualitative researcher would be smart enough to realize that he or she can not solve *any* question that involves quantifiers like 'most': this requires some combination of *counting* and *generalization* which is impossible to do qualitatively. If the 'Most' were not present in the David Conjecture, things would be easy, as long as the qualitative researcher would not consider his sample saturated at the moment two Davids have been selected. When only Joop is selected earlier, the Conjecture is easily refuted.

The frequentist statistician might start to grumble that this is not a representative sample from the population of statisticians, but judged by the quality of the presentations at the symposium, this viewpoint only complicates matters: maybe only the very gifted statisticians are called 'David' so that it turns out to be something like a title of honor: you *have* to be called David to be a good statistician[11]. Note however, that, compared to the Bayesian, the frequentist has a hard time proving or refuting the conjecture since the David Cox case is not in his data set and thus, even if he trusts his sample, his 'David estimator' is only 50%!

Also for the Bayesian things are far from easy, even if the fact that David Cox was invited but not present is used to formulate some a priori probability distribution. He is confronted with the question *How to select an a priori distribution based on one observation?* On the other hand, his evidence seems much stronger than that of his frequentist counterpart, if only the David Cox observation could be taken into account in some formal way.

[10] For example, I recently advised a PhD student who wanted to fit a confirmatory factor analysis to her data set, consisting in 1000+ cases, to randomly split her data in two parts and crossvalidate the solution. Once she had successfully done that, she wanted to report baseline statistics on the two data parts in her article because the gender distribution seemed not to be identical. She asked how this could have happened and what I thought of it. It took an awful lot of trouble to talk her out of the idea to include something on this in her final article.

[11] This conclusion seems quite unfair towards Joop and Hans, but probably (and commonly so) matters may function quite differently in the Netherlands: the David Conjecture may only be valid for native English-speaking statisticians.

Finally, the Methodologist is the only one who can "question the question". He is accustomed to ask: *'Why do you want to verify such a Conjecture? If it turns out to be true what are the consequences: what will you do with such a result? Is it publishable? Or just a silly result?'*

And he or she could go on: *'On the other hand, if the Conjecture turns out to be false, only something very silly is refuted and we have learned nothing we did not already strongly suspect, namely that there is no association between someone's first name and his or her profession.'*

In other words, the methodologist would probably try to convince us to drop trying to solve the David problem, because it just seems a waste of time.

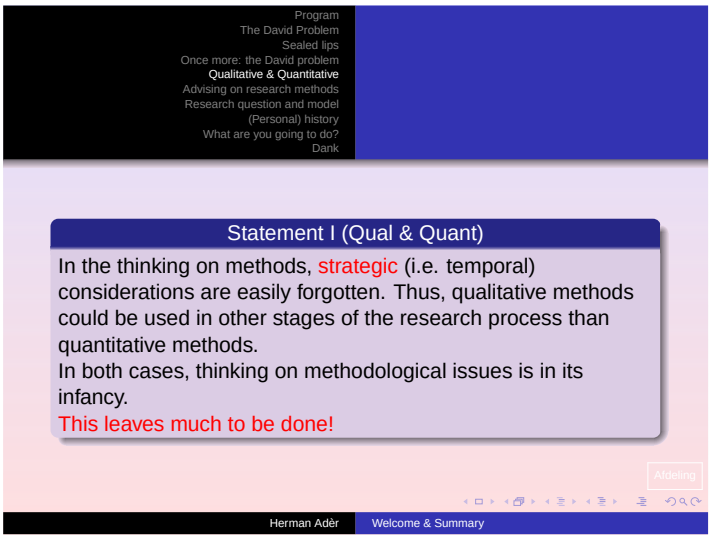


Figure 6: Strategic thinking on methods.

3 Statements

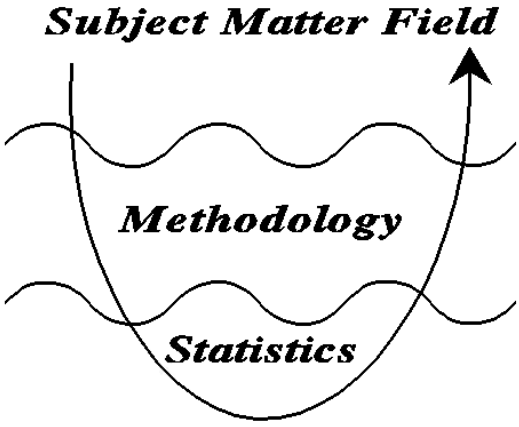
Instead of summarizing the lectures and the discussions that took place during the day, or, alternatively, describing discrepancies between what I had heard and had expected to hear, I decided to formulate a number of propositions on the different topics of the day. I meekly called them 'statements' in an attempt not to look too provocative. Figure 6 gives the first of these.

Combining qualitative and quantitative methods

Controversies between methodologists that use qualitative and those that use quantitative methods are due to different background paradigms[12]. The idea to combine the best of both worlds to try and solve research problems, although rather obvious, is only recently gaining grounds. And even so, the process is hindered by the fact that not many people are jacks-of-both-trades.

There is also a strategic side to this, in the sense that the process of doing research has several phases and to plan these phases some sort of strategic thinking is needed. I don't think it is often realized between methodologists how very dynamic and phase-dependent our trade is. As I heard Robert Pool say at some point

[12] Quantitative methods are mainly based on statistical conceptions, qualitative methods could be called phenomenological.



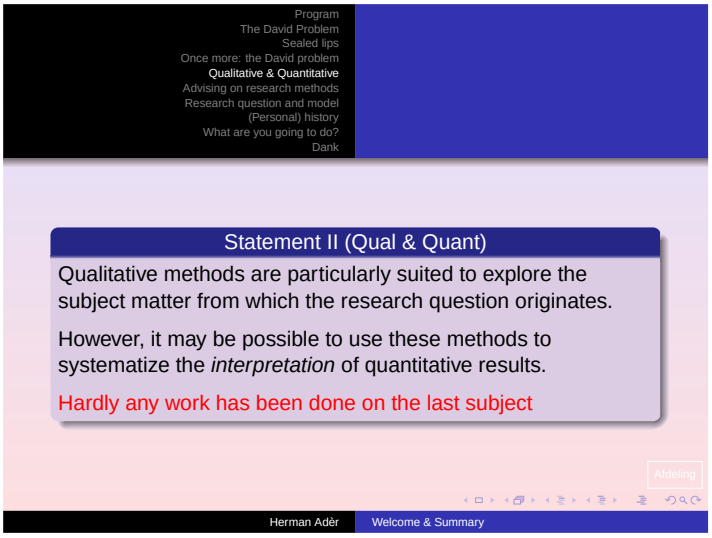


Figure 7: **Upper panel:** Correspondence between Subject matter field, Research Methodology and Statistics. **Lower panel:** Using qualitative methods for the interpretation of quantitative results.

during his visit, it is a bit naive to assume that qualitative methods can only be used at the beginning of the research cycle. In fact in each phase one has the choice to apply either qualitative and/or quantitative approaches.

During the presentation, I mentioned one complication in the implementation of mixed research. To get my point, first note that sampling is completely different between qualitative and quantitative methods. In qualitative research it is common to use what is called 'judgmental' sampling by some, 'theoretical' sampling by others: roughly speaking, in this strategy, new units are added based on some external criterion which may be influenced by earlier investigated observations. The buzzword here is *saturation*: we go on sampling until nothing new surfaces. In quantitative research, sampling is based on random selection and randomization, the buzzwords being *sample size* and *power analysis*. The fact that these sampling strategies are so different has a heavy impact when one tries to combine qualitative and quantitative methods, in particular when one wants to interpret the combined results. In fact these sampling strategies may come out contradictory: the first one strives for heterogeneity (since as much new information has to be elicited as possible), the second one for homogeneity (in a heterogeneous sample the statistical estimates are not to be trusted or one has to discern and study 'subgroups' that are homogeneous). Probably the best way to combine the two sampling strategies is to do the random sampling first and draw the cases for the qualitative part from this. It is hard to imagine how to do it the other way around and make sure that the 'qualitative cases' occur in the random sample. The above problem and its solution are by no means commonly realized. Normal practice is to do the qualitative data collection on other units than on those that are collected for the quantitative part.

My second statement is shown in Figure 7 (lower panel). Since the work on my thesis (Adèr, 1995), I have been intrigued by the fact that so much formalized knowledge exists on research methods, but nothing is mapped out on the subject matter field in which the research question arises. In my opinion, qualitative methods offer a more structured, formalized method to describe the subject matter field, the context of the research question.

A logical consequence of this perception of qualitative methods as the means to represent the subject matter field is that, maybe, it could also be used to do the 'back-translation' step and use these methods to interpret the results that are produced by the quantitative part of the study (See also Joop Kuik's figure 7 (upper panel) in this respect). As a practical example, think of a focus group of patients or specialists, to discuss the results of a study, even before the scientific article in which these results described are submitted to a journal for peer review.

Advising on research methods

The next statement was formulated as follows:

> *The theoretical background of practical advising is Research Methodology (RM). However, the knowledge on RM is incoherent and scattered.*
>
> *This is the time for some systematizing!*

Working at our new book ('Advising on research methods') during the last three years, we found that most of the Adviser's work concerns Research Methodology, much more than Statistics.

So, what does that mean, *Research Methodology*? In Mellenbergh et al. (2003) it turned out to be difficult to properly define this field: its body of knowledge is of a kaleidoscopic nature since its elements stem from a variety of sources: Applied statistics and probability theory, research design (f.i. Experimental design or the design of clinical trials), measuring theory, data analysis, but also Combinatorics, Graph theory and, last but not least, from the mother of all practical research, the Philosophy of science.

During the writing of 'Research Methodology' book (Adèr & Mellenbergh, 1999), we thought it would be possible to create some order in this chaos. It seemed that in fields in which empirical research is practised, researchers use similar methods and interpret results in comparable ways. I am sure we did not succeed in mapping out this new field, but we *did* bring together the material that is needed if we want to give a more formal description. And we learned a lot. In the book on Advising, we try to go one step further and describe general methodological principles to clarify the practical process of consulting. Maybe this will function as a run-up for more fundamental work on the principles of research methodology.

However, one should not hope for one 'Grand theory' that encompasses the whole field of RM. The business of doing research is very multifacetted, each facet representing a discipline of its own. For instance, one can hardly hope to unify fields like *Research design*, *Measurement construction*, *Data Analysis* and *Result interpretation*. However, Statistics is only helpful in very well-specified situations. One sometimes wonders whether Statistics is a monster that bullies us into choosing a statistic perspective instead of providing tools with which technical problems can be solved. In fact, one can think of a methodological equivalent for many statistical notions. We already did this for notions like *robustness*, *reliability* and *modelling* (See Adèr, 1995; Mellenbergh et al., 2003). But this can be done systematically. For instance, one could ask, what would be the methodological equivalent of: (i) Prior knowledge (ii) Precision (iii) Bias (iv) Uncertainty (v) Independence (vi) Correlation, or (vii) Missing information? to name just a few.

On the other hand, I hardly need to mention how many methodological terms are already in use. Think of the terms used in research design (randomization, treatment group, failure), instrument development (latent trait, facet) or in data analysis (crossvalidation).

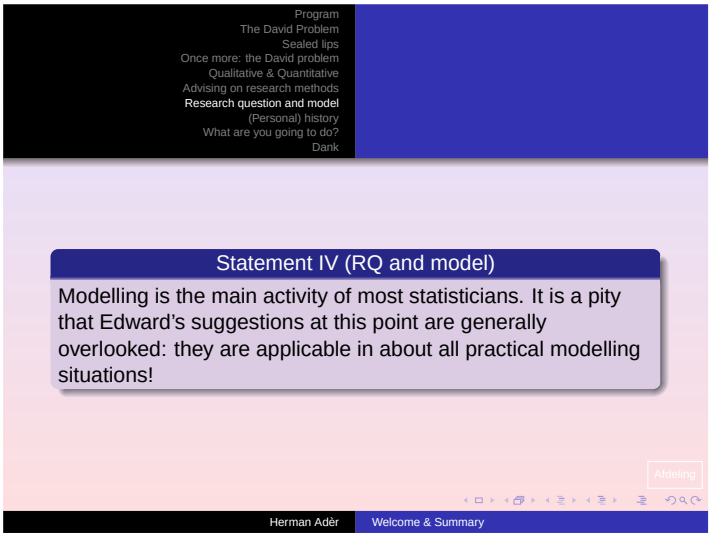


Figure 8: First statement about modelling, advocating Edwards and Havránek's method to explore a model space.

Modelling

In the presentation two statements on modelling were included. The first statement (see Figure 8) has to do with a smart method to explore a large model space, developed by Edwards and Havránek (1987). I coauthored several papers on the subject, exploring applications in linear and logistic regression analysis (Adèr et al., 1998; Adèr et al., 1996) and Cox' regression [13].

Lately, we considered applications to other iterative methods: (a) to the (iterative) detection of 'item bias' (Van der Flier, Mellenbergh, Adèr, & Wijn, 1984) (also called 'Differential Item Functioning' by some (Holland & Wainer, 1993)), and to (b) (iterative) Mokken scale analysis (Mokken, 1997) but both ideas are only 'in the design phase'.

[13] Presentation at the 'Workshop On High-Dimensional Data' (Leiden, 2002): 'Parallel supervised model search for Cox regression'. See:
`http://users.keyaccess.nl/~aderh01/KEB/hja/down/leidenhandout.pdf`

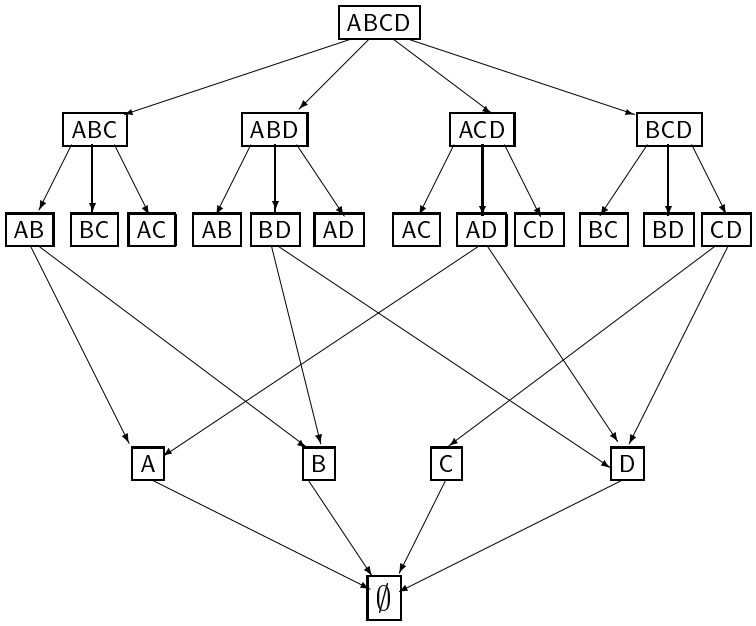


Figure 9: Diagram of a four variable model space.

Figure 9 gives an example of a model space of models in which four terms (A, B, C and D) play a role (think of regression models with four predictors). The technique explores each branch of nested models in turn, either beginning at the top (where the saturated model resides) or at the bottom (starting from the empty model). The essence of the technique is the smart way of pruning.

In Table 1 an example is given of the kind of information the program provides. Careful analysis of the accepted models reveals that variable **SWA** is needed in all models and that **MIC** and **ANF** have an interchangeable role in the model space but never occur both in one model.

A second statement on modelling, ran as follows:

Statement V (Research question and model):

> A much more general view on modelling is possible ('Methodological modelling': we have even written a paper about that).
>
> *Modelling should be taken away from the statisticians and put into the hands of the Methodologists!*

This refers to a paper entitled 'Methodological aspects of statistical modelling: some new perspectives' (Adèr et al., 2002) in which a view of modelling is advocated in which the activity of the statistical modelling is only a part. In general terms, nobody, not even the most hardline statistician, would oppose the view that modelling is an activity that can only be properly conducted with the help of a lot of knowledge about the field where the research question arose.

	MAL	SWA	HDU	MAC	MIC	ANF	U/A/R
One independent							
1	×						
2		×					
3			×				
4				×			
Two independents							
5	×	×					R
6		×			×		U
7			×		×		UR
8			×	×			
Three independents							
9		×	×		×		A
10		×		×	×		A
11		×	×	×			A
12		×	×			×	A
13		×		×		×	A
Five independents							
14	×		×	×	×	×	

Table 1: Example of output of a space of survival models with six predictors. *Legend*. **MAL, SWA, HDU, MAC, MIC** and **ANF**: predictor names; **U**: model included by user intervention; **R**: rejected model; **A**: accepted model; **Blank**: Neither accepted or rejected or violating parsimony requirements.

Aristotle on research methodology

As a preparation for the state of retirement, I have been reading Aristotle (in the English translation of Barnes (1995)) stimulated by, and together with, the philosophy student Robert Goené. The statement depicted in Figure 10 is taken from the Posterior Analytics. It is amazing to see the relation between the researcher in the field and the research methodologist (here called 'the mathematical scientist') so clearly described. Since he lived from 384 to 322 B.C.[14], one may say that there is nothing new under the sun. Here is another statement by Aristotle which I didn't include in the presentation, but that is equally worthwhile: *All sciences associate with one another in respect of the common items (I call common those which they use as demonstrating from them — not those about which they prove nor what they prove); and dialectic associates with them all, and so would any science that attempted to prove universally the common items – e.g. that everything is affirmed or denied, or that equals from equals leave equals, or any things of the sort. (Aristotle, Posterior Analytics Book I, 11, 26ff)*

[14] See `http://www.iep.utm.edu/a/aristotl.htm`

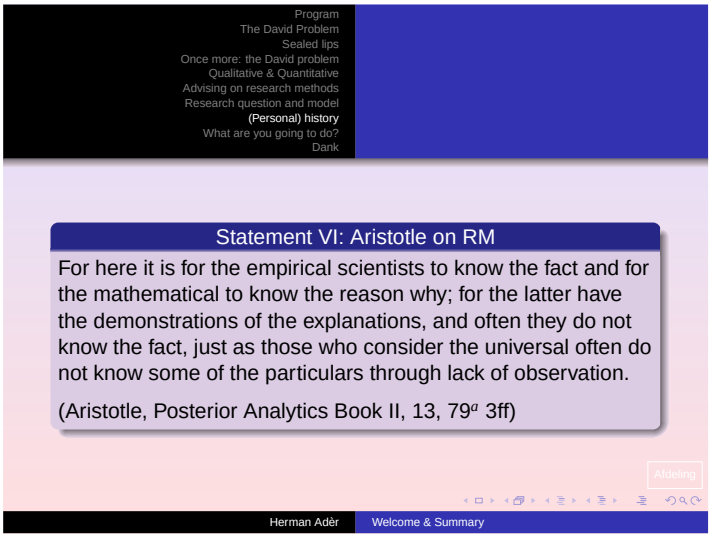


Figure 10: Aristotelian description of the relation between the methodologist and his/her client researcher.

Again, in the same astonishing way, the statement hints at one of the basic assumptions of RM mentioned before, namely, that empirical sciences may have some common ground in their methods. It even hints that a discipline like RM could be thought of ('any science that attempted to prove universally the common items'), although I am sure that Aristotle envisaged something more abstract than the multifaced field we have now.

This ends the more serious part of my presentation.

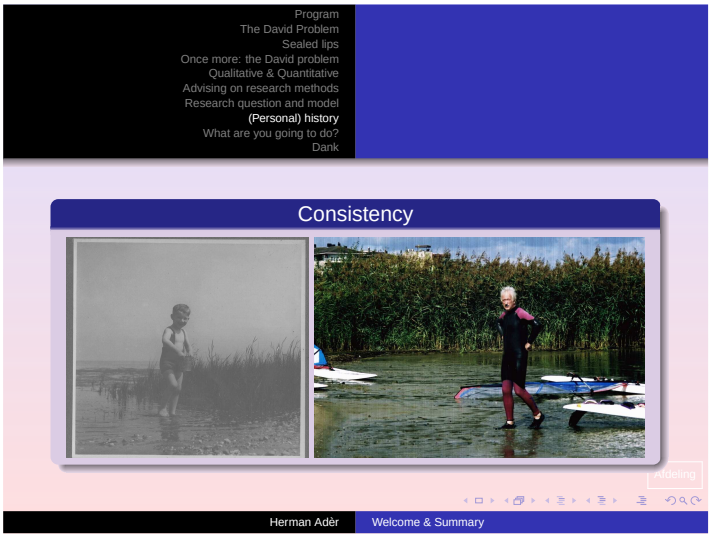


Figure 11: Consistency in my life.

4 Consistency and Thanks

After this I conveyed the thought that, although my scientific activities may seem rather diverse, there has always been some consistency in that they had to do with research methods. This consistency may be an element in my character that is recognizable in other activities, too (see Figure 11).

At the end of my summary, I thanked a lot of people (See Figure 12). But I stupidly and painfully forgot to list Riekie de Vet with whom I very fruitfully cooperated in several projects. *Riekie: thanks a lot! I wish you many prosperous years to come.*

I thanked Maarten Boers who has been extremely supportive in general and made possible that the symposium took place at all. Anne Marie Fallon, the secretary of the Department Clinical Epidemiology and Biostatistics took a large number of administrative actions that are indispensable to organize this kind of meeting and, in her very kind personal way, took care of the human aspects as well.

Finally, I thanked Louise, my wife, who was ill at the time the symposium was held. We both regretted very much that she couldn't be present[15].

[15] At the moment of writing, she is very much alive and kicking again.

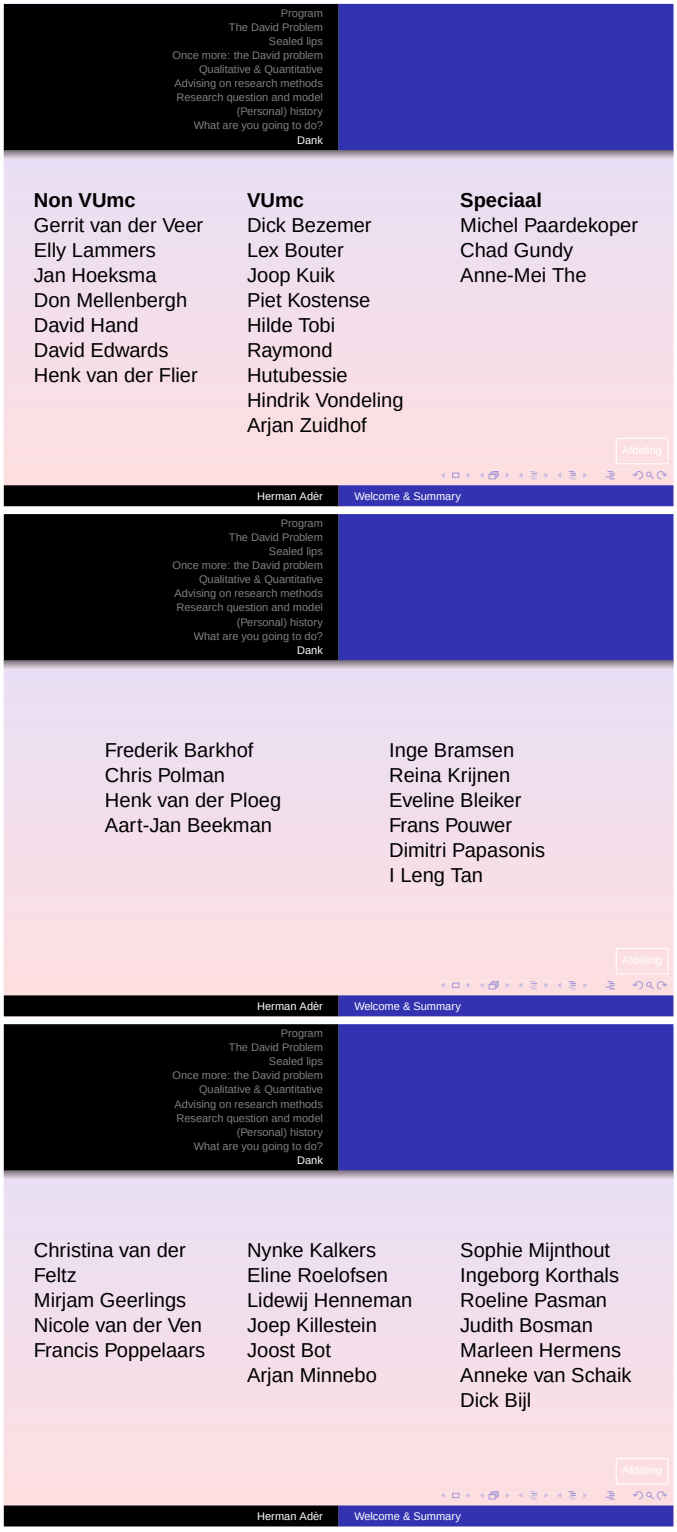


Figure 12: Thanks to all.

A Appendix

All material relevant to the Symposium is collected on:
`http://www.jvank.nl/Symposium/`
For instance, an overview of the program can be found in `Announcementprogram.pdf`. All abstracts are collected in `Allabstracts.doc`. Background articles can be found in the directory `Articles`:

For Riekie de Vet's presentation: `artikel rony evans.pdf` and `beaton and bombardier.pdf`;

Robert Pool: `RPscience.pdf`;

David Hand: `dhdeconstructing.pdf`;

Don Mellenbergh: `dmha.pdf`;

Jan Hoeksma: `jh01.pdf`.

David Edwards: `DE01.pdf` and `DE02.pdf`

In the directory `Presentations` most presentations can be found and a foto session will start when you click:
`http://www.jvank.nl/Symposium/Fotos/Fotos.htm`
Finally, the present account (this pdf file) can be found at:
`http://www.jvank.nl/Symposium/AccountHermanAder.pdf`.

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