In Memory of Peter H. Schönemann

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I met Peter Schönemann only twice: the first time in 1993 at a retirement symposium for Jerry Hirsch at the University of Illinois; and a second time in 1995 when we were both on the same program at a AAAS meeting in Atlanta. On meeting Peter I knew at once that here was a strong, knowledgeable and principled ally in the struggle against biological determinism and scientific racism. His work and his commitment to fighting racism in the name of science are something I hold in the highest professional esteem. I am neither a statistician nor a very intuitive mathematician, so I appreciated all the more the work of people like Peter who challenged the statistical industry and also made the inadequacies of concepts like heritability intelligible to non-specialists.

Although face-to-face contacts were few, I did maintain an ongoing correspondence with Peter from the early 1990s until 2008, when we discussed at some length (in letters, e-mails and an occasional phone call) a variety of issues, including the memorial tribute he was preparing on Jerry Hirsch. Peter had asked me to look over his comments, and we had a lively exchange in July, 2008, the last, unfortunately, in which we were to engage. In his draft Peter stated that Jerry clearly opposed the use of heritability in any context as basically meaningless. In my response I said I thought Jerry accepted heritability as useful in dealing with animal populations where environmental variables could be controlled. In his usual, insightful way, Peter corrected me, stating that if it were really the case, as he and Jerry both believed, that phenotypes are the product of an interaction between heredity and environment, it was illogical to try and separate out the contributions of either as somehow separate entities. I realized then that he was absolutely correct about both the concept and Jerry's position. Peter had that kind of rigorous, penetrating mind.

Peter also kept a clear perspective on the whole I.Q. testing ideology, at once recognizing its flawed nature as any kind of measuring tool, as well as, historically, its integration into our educational system as a major (and financially lucrative) industry. I appreciated the fact that he always saw that the notion of a single factor of intelligence – indeed of "intelligence" as any kind of reified entity – was a myth, and that recognizing this makes the squabbles about heritability a minor issue: "Once one gets serious about the IQ problem," he wrote to me in 1995, "then the heritability issue becomes secondary. But since a lot more people have vested interests in maintaining the IQ myth than the heritability myth, the resistance will be that much stiffer."

In one of his letters Peter shared with me some aspects of his own background that helped me understand where his dedication to rigorous thinking may have originated. He discussed his "take on positivism, which I developed as a student in Munich in the early 1950s. This place was crawling with denazified Nazis and racists at the time and the psych courses were a total waste of time. Instead of attending lectures . . . I spent most of my time in my tiny room with my girlfriend and computed factor analyses with the help of slide rules. I was an admirer of Cattell at the time [I assume this is a reference to psychometrician J.McKeen Cattell, also a long-time editor of Science] and in awe of Psychometrika that I had discovered in the stacks of the America Center (formerly Hitler's residence during his visits [to Munich]). During those days I read a number of books on positivism which made sense to me at the time. I especially liked Reichenbach. Of course my present take is somewhat different. I no longer believe that physics has anything to teach psychology, and that those who claim it does do so mainly to promote themselves at the expense of their more ignorant peers. That is, positivism was a good idea but it simply does not work in psychology." [Letter, July 4, 2008]

Peter also shared with me a number of documents from his controversies with journal editors and reviewers who rejected papers (in this case his own) attacking fundamental concepts such as heritability. These illustrated clearly Peter's tenacity and thoroughness in responding to what he considered irresponsible editorial principles and practices, especially where he saw them as politically motivated. For example, in 1988 he had submitted an article, "A Note on Holzinger's Heritability Coefficient h2," to Biometrika. At the end of February the editor of Biometrika wrote back rejecting the paper saying it was "more suitable for one of the journals putting emphasis on biometrical genetics." No explanation was given as to why the paper was not suitable to a journal that had been founded (by Francis Galton and Karl Pearson in 1901) explicitly to publish papers dealing with statistical approaches to heredity. Peter then submitted the paper to the Journal of the American Statistical Association (JASA). A subsequent response from the editor of JASA (June 13, 1988) claimed that the paper dealt with a concept (heritability) that is outmoded. Quoting a reviewer, the editor, stated: “No one uses Holzinger’s heritability concept in human genetics or behavioral genetics any more. It is generally known that h2 does not make sense.” Peter quickly responded (June 20, 1988) by pointing out that Holzinger’s heritability index “has been used without interruption from the 1930s into the ‘80s.” He then cited two books, one from 1980 and one from 1981 both of which acknowledged they were using Holzinger’s heritability estimates to analyze data from twin studies. To clinch the matter, Peter did a citation index study showing that for each successive five-year periods from 1973-1987 the number of references to Holzinger’s h2 methodology had remained virtually unchanged (between 50 and 60 per period). Thus, it was not possible to claim that no one uses heritability any more. I have always appreciated Peter’s kind of care and exactness, especially when it exposes underlying untruths and unstated political biases masquerading as intellectual argument.

I do wish I had been able to have more face-to-face discussions with Peter, because I felt we were on the same track intellectually and in terms of the social misuses of the whole genetic determinist movement of the 1970s-1990s – except that he was far more knowledgeable and sophisticated about the psychological and statistical aspect of the story that I could ever be. After we were on the same program at the Atlanta AAAS meeting in 1995, Peter wrote: “Next time let’s make a point of setting some time aside for more extended chats.” I regret that the opportunity never came.

Better late than never

Moritz Heene
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I still remember the moment in fall 2005 when I had to copy an article from the 1996 edition of *Multivariate Behavioral Research* for my doctoral thesis. I have completely forgotten which article it was, but I recall the moment when I thought it might be worthwhile to turn some more pages to see whether there were additional articles of interest, even if unrelated to the topic of my doctoral thesis. Of course, I could not realize how important it was for my career as well as for my personal life to turn that page. The article that followed discussed something called “Factor Indeterminacy”. Frankly speaking, I had never heard of it, and since I was taught that factor analysis is one of the soundest methods in psychology, the simple fact that there was a contradiction between what I learned and the title of the discussion aroused my curiosity. I therefore copied the entire article, read parts of it while sitting in a train that day, and understood very little. Nevertheless, the next day I did an internet search for “factor indeterminacy” and, as luck would have it, the first hit was Peter Schönemann’s website, which included two contributions to the discussion characterized by an obvious deep understanding and a good sense of humor and irony. As I read the titles of his publication list, I grew increasingly more exited and simply could not believe what I had found. These were exactly the papers for which I always searched but thought to be impossible in psychology as I knew it; critical but also technically well-founded. Not only did factor analysis have at least one fundamental problem, but the factor indeterminacy issue undermined Spearman’s attempt to provide an objective definition of intelligence as "g" through factor theory; heritability estimates as well as measurement itself had fundamental flaws; and the peer-review system was far from being perfect and objective (“Better never than late”). So I had uncovered a trove of articles related to a multitude of problems within mainstream psychology. Needless to say, it hit me like a ton of bricks to discover that so much of what I had learned during my studies was on shaky ground, to put it mildly. Fortunately, I was so excited that I overcame any restraint and wrote Peter an enthusiastic email on the same day, thanking him for all those “eye-openers”. The next day, I found his email in my inbox and so began our late friendship and professional exchange.

Later we discovered that we were both avid blitz chess players. Consequently, our blitz chess "tournaments" formed an essential part of my subsequent visits with Peter in Indiana.

He supported me in all respects and provided me with new information essential for my doctoral thesis and remains so for my present and future work. Metaphorically speaking, he pulled the rug out from under me, and it took a relatively long time to reconfigure my way of thinking, but he also taught me to walk again for which I am deeply thankful. Thus, turning another page in *Multivariate Behavioral Research* and clicking on Peter's webpage was a watershed for me.

What impressed me the most, besides his immense technical and detailed understanding, was his clear understanding of a scientific problem as a whole. He never fell into the trap of technical details but always got to the point. For example, during a conversation he told me: "It took relatively long for me to understand the difference between a *hypothesis* and a *method*, that is, the difference between Spearman's falsifiable two-factor theory and Thurstone's non-falsifiable method of factor analysis". (Interested readers should read his chapter "Psychometrics of Intelligence", where he reflects on this problem). Furthermore, he pointed out that factor indeterminacy can best be understood as the algebraic problem of having more unknowns than knowns. As we learn in high school, with two equations and two unknowns, a unique solution can be found. However, with two equations and three unknowns, there is no unique solution – the solution is indeterminate. From him I learned that comparing random variables on the basis of distributional parameters teaches us nothing about the similarity/sameness of the random variables itself. (So think critically about the usual interpretations of multigroup factor analysis or the congruence coefficient about the equality of factors on the basis of, for example, the equality of factor loadings). Test validity should be assessed by taking base rates into account instead of relying merely on the correlational approach which does not give the entire information needed to assess the validity and utility of a test. (I was often asked by laymen about the improvement in terms of the number of correct decisions by a so-called incremental validity of, say 5%. Truth be told, by "hiding beyond methodology" (Andreski, 1972) and explaining that, for example, a certain admissions test accounts for 5% additional variance in the criterion variable, I actually could not answer the question. By going back to the roots, i.e., to fundamental statistics of base rates, success rates, true positives and false positives, etc., I learned that incremental and correlational validity often does a poor job judging the practical validity and utility of a test.)

Tying it all together, the most general principle in Peter's research was honesty. Just by scanning the titles of his papers, one can clearly see that he was not interested in promoting his own ideas. Whenever he discovered a fundamental flaw in the statistical methods he investigated, even if he spent years and built the basis of his career on it, he was able to revise his research, i.e., accept the "falsification" of his efforts. There is no better example expressing his deep commitment to this principle on the one hand and his deep skepticism about its adherence in psychology on the other hand than one of his "Andreski-like" personal quotes: "The only theory psychology needs is a theory of science". One can find examples of this honesty clearly reflected in "Factorial definitions of intelligence: Dubious legacy of dogma in data analysis" (1981) and "Measurement: The reasonable ineffectiveness of mathematics in the social sciences" (1994). Having said this, falsification seemed to be a virtue not a weakness of his research.

Peter and I regretted not having met earlier. And yes, there are still many questions I want to ask him which now may remain unanswered. He once said to me (after having sent me his autobiography): "As you can see, most things in life are chance". Well, I agree with him that we might have met *late* by pure chance. Although having found each other by turning a page in a specific journal in a specific issue seems to be strongly driven by chance, I have to disagree with him because the fact *that* we met cannot be attributed to chance only. So, Peter may forgive me for criticizing his "pure randomness model", but in my opinion there is a non-ignorable component of "communality" apart from a unique or even only a random component. Moreover, the five years we had was a rather short but intense and defining period of time for me which will continue to enrich my life and for which I am deeply thankful. I sorely miss him.

This memorial would be incomplete without also deeply thanking Roberta Schönemann for her incredible support and warm-hearted hospitality. I am proud of and happy about the friendship of these extraordinary people.

In Memory of Peter H. Schönemann

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National Chung-Cheng University,
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National Taiwan University, Taipei,
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When I returned from the Chinese University of Hong Kong to Taiwan to assume a teaching duty at National Chung-Cheng University in 1992, I met Peter at National Taiwan University. He should have been a visiting professor in the Department of Psychology at National Taiwan University in that academic year. Before moving to Hong Kong to take the responsibility of a founding chair for the Department of Psychology at the Chinese University, I had taught at National Taiwan University for about 30 years and had been keeping an office in the Department. Therefore, I had opportunities to meet Peter several times in that academic year.

Although the area of Peter's interest differs from mine, I found him quite different from most foreign visitors to many universities in Taiwan. He was a real scholar, respecting all views, and having a keen interest in Chinese culture. Moreover, Peter and I were both University of Illinois alumni. Therefore, our friendship had grown in the subsequent years.

Peter had come to Taiwan second time as a visiting professor at National Taiwan University. In that academic year, I invited him to come to National Chung-Cheng University to give a talk. When I attended an annual meeting of the Psychonomic Society once in the 1990's, he also invited me to give a talk in the Department of Psychology at Purdue University. In that occasion, I had an opportunity to visit his home in beautiful surroundings. He also told me from his meeting Roberta to courtship to marriage. I believe he must have had a happy family life.

After his retirement from the university, I have also retired from Chung-Cheng University. In the past years, Peter had kindly read some of my manuscripts and most importantly corrected my stylistic errors in English. In the front page of those articles published in "Journal of Experimental Psychology: Learning, Memory, and Cognition", "Quarterly Journal of Experimental Psychology", and "Behavioral and Brain Sciences", I always listed his name to express my appreciation. Although he has passed away, his name will remain forever in these front pages of my articles in those journals of my specialty as well as of his own articles in those journals of his specialty.

In Memory of Peter H. Schönemann

Michael Maraun
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I can still remember as if it occurred yesterday, my first encounter with the work of Professor Peter Schönemann. I was in my first semester of a Master of Arts program at The University of Guelph. My supervisor, Professor Roland Chrisjohn, was an incredibly well-read scholar and was in the habit of assigning to me, each week, a great stack of papers that he deemed to be essential reading. The readings spanned philosophy, mathematics, psychometrics, and psychology, but each week's stack had, broadly speaking, a theme. I had just finished several weeks on Wittgenstein when, just before Christmas, Roland assigned me a stack of papers that contained some Guttman, some Schönemann and Steiger, some Tukey, and some Neyman and Pearson. As was my habit, I took the stack and traipsed off, in this case through a foot of fresh snow, to a nearby Tim Horton's Donuts. My preferred table was available and so I seized it and looked over the papers. The title *A History of Factor Indeterminacy* caught my eye, and two sentences into that paper, I was completely engrossed. The next morning, I went to the library and made copies of all of the Schönemann publications that I could get my hands on. Over the Christmas break, I immersed myself in these works.

These works were not the thin, spiritless gruel of mainstream psychometrics, but, rather, full-blooded scholarship delivered with style and in a tone that ranged from authoritative to the most drily sarcastic. They were the antithesis of cautious careerism, and manifested a brave, swashbuckling academics characterized by an unyielding pursuit of the truth that was cash-backed by brilliant analytical skills. To me, it was thrilling stuff, and settled for me the route that I wished my own scholarly life to take. Over the years, I have returned time and again to Professor Schönemann's work to learn a style of psychometrics that, in many ways, is spiritually kindred to European analytical philosophy, rather than anything originating in the social sciences.

When it came time to choose an external examiner for the thesis defense of my first Ph.D. student, I wrote to Professor Schönemann and asked him if he would play this role. His visit to Simon

Fraser would turn out to be the only time that I would have the privilege of discussing with him, in person, his ideas and his approach to scholarship. However, upon his return to Purdue, he sent me both his and Guttman's correspondences with the editor of Psychometrika over their highly critical reviews of McDonald's 1972 submission on indeterminacy, and his deeply frustrating correspondences with the editor of The British Journal of Mathematical and Statistical Psychology over his attempts to have published a paper by he and Steiger. I found to be highly inspirational Professor Schönemann's refusal, regardless of the cost, to yield the scholarly floor to dishonest and politically slick editors. That the saying of the truth should take precedence over all else, appears to me to be the fundamental code that underscores all of Professor Schönemann's work, perhaps never more saliently than in his brilliant, original, but unpopular, technical challenge to the factor analytically infused work of the intelligence testers, and, most notably, Arthur Jensen. In my view, in the history of the social sciences there have only been a very few great scholars of quantitative methods. With the passing of Professor Peter Schönemann, this rare species of academic may now be extinct.

In Memory of Peter H. Schönemann

Yoshio Takane
McGill University

It was with a great deal of sadness that I heard the news of Peter's death. He was not only a great psychometrician but also a very humane and considerate individual. Needless to say, Peter has made a number of significant contributions to psychometrics. His closed-form solution to the orthogonal Procrustes problem (Schönemann, 1966) is now part of our day-to-day operations. His algebraic solution to the weighted Euclidean distance model (Schönemann, 1972) for individual differences multidimensional scaling (MDS) has been a point of departure for many procedures developed thereafter for obtaining rational initial starts for fitting this model to fallible data. The idea of incorporating response models (models for generating particular forms of dissimilarity data) into MDS that I exploited in developing many MDS procedures was initially prompted by Schönemann and Wang (1972), who developed a similar procedure for unfolding analysis of preference data.

Despite many professional contacts with him, I met Peter only once personally. It was when I visited Purdue to give a talk a long time ago (early 1990's). It was quite a memorable event. Roberta and Peter kindly let me stay at their place for several nights. Not only that, Peter gave me a small stuffed gorilla doll as a souvenir for my son who was only 4 or 5 years of age at that time, but my suitcase was stolen on my way back to Canada together with the doll, and it never turned up. Having learnt later that it was completely lost, Peter kindly sent him another one by mail, which we still cherish almost 20 years after the incident.

In Memory of Peter H. Schönemann

Howard F. Taylor
Emeritus Prof. of Sociology, and
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American Studies Center
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I was fortunate in getting to know Peter about fifteen years ago, when we were both members of a special professional seminar to evaluate methods of estimating the genetic heritability of IQ and other human behavioral phenotypes. I only wish I had gotten to personally know him sooner. I had written a number of articles and chapters on the matter, and had become familiar with Peter's excellent and lucid work, particularly those works of his that involved the comparison of identical and fraternal twins. What I learned then was that Peter was among a very few behavioral scientists –four or five at most– who understood in extremely great detail the inner mathematical workings of the calculation of the heritability coefficient, particularly the role of empirical assumptions in such calculations. In particular, in a series of published papers he understood, and had in wonderful detail, empirically estimated and calculated the effects of varying the mathematical assumptions upon the resulting estimation of genetic heritability. His understanding of the quantitative structure of heritability estimation was precise and deep, and went beyond the work of even the best behavioral scientists in the field, including economists, psychologists, sociologists, and a good number of behavioral geneticists as well. I have learned immensely from him, and my own work has benefited greatly from his. His influence has been wide, and has become even wider in the last decade. Peter has taken to task many scholars in the fields of ability testing and heritability calculation. It is now known that to come to grips with the whole area of heritability estimation, one must first absorb the work of Peter Schönemann.

In Memory of Peter H. Schönemann

Gerald S. Wasserman
Professor of Behavioral
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Let me begin by thanking Roberta for asking me to speak at this celebration of Peter's life. I would note that Louise also contributed to what I have to say because the two them have been an important part of both of our lives for about 35 years ever since I first came to Purdue to be interviewed. Even during the first brief chat I had with Peter during my tour of the department, I had a definite sense that here was a kindred spirit.

Accepting the offered position marked a dramatic transition for me when I suddenly had about 50 new departmental colleagues, most of whom I had not previously known. Among the latter group, I still remember how particularly easy it was to form a bond with Peter even though our individual research interests were quite different. That happened because we shared a number of personal traits.

One was because Peter still frequently used the term "GI" in casual conversation. For the benefit of the younger members of our present company, I would note that GI is an acronym for Government Issue. It had been a very common term in my youth when many of my schoolteachers and summer camp counselors had just returned from their military service bringing that term home as part of their daily speech. Being about ten years younger than Peter, I had idolized them when I was still a little kid and it quickly became clear that Peter still did too. I do not recall hearing the term much after I had finished school and yet Peter still used it with some regularity, particularly when the two of us were cross comparing his memories with the stories I had once heard.

Many of these stories involved his journey to America as an immigrant. Being the son and grandson of immigrants, I had stories to tell about their migrations too.
Peter liked to hear these stories and I was glad to tell them. He even listened amiably to the one about my father being an illegal immigrant. They had both partially migrated by water but my father was merely a passenger on a ship while Peter actually swam across the Elbe River.

Our major bond was that Peter was clearly a genuinely independent thinker for whom a university professorship was the ideal position. At least that was certainly the case about a half-century ago when he and I both started our careers. It had then been almost universally understood that the special social function of the university was to provide a home for independent thought. As a result, universities then commonly used their own endowment income to provide basic support for the scholarly work of any of their own faculty who were working in unfashionable areas. The explicitly stated proposition was that anyone who is good enough to be on our faculty is good enough for us to support no matter what the rest of the world thinks. Today this idea is faltering badly.

Peter flourished in that earlier kind of university and successfully used his academic post to challenge and eventually overturn a dominant but false fashion that had gripped our field. It was called factor analysis and I remember constantly hearing about it back when I was a student and then an aspiring academic. I personally was not much affected by it in that my own research area happened not to be particularly given to using this approach. But it was very commonly used in some other areas in earlier days and not the least reason for its success was that it eliminated the need for thinking. Just collect a box full of numbers and pour them into a factor analysis and presto! A theory emerged ready for publication.

Peter proceeded naturally against this nonsense by formally demonstrating that factor analysis was indeterminate. That is to say that a given factor analysis did not produce a unique theory but rather that trivial procedural differences would necessarily produce vastly different analyses which could be made to fit any set of observations. In proceeding in this way, Peter was granting his opponents the presumption of rationality in his belief that a valid mathematical proof would compel their beliefs.

This generous and optimistic presumption was not fulfilled and so he brought the ultimate weapon to bear: ridicule. Thus Peter's oral presentations began to include utterly preposterous specific examples in those same areas in which he had provided elegant mathematical proofs of factor indeterminacy. Some of these made it into print. One was given in a 1987 paper[*] which he wrote with Haagen. That paper showed that a factor analysis of intelligence-test scores drawn from 300 middle-aged males and females could successfully predict the calendar dates of 20 successive Easter Sundays. Experience has shown me that it is necessary to repeat this proposition because people hearing it for the first time may have some trouble with it. So: That paper showed that a factor analysis of intelligence-test scores drawn from 300 middle-aged males and females could successfully predict the calendar dates of 20 successive Easter Sundays. I can still remember the absolutely delighted grin that would appear on Peter's face whenever I heard him present such a lethal conclusion after an utterly deadpan buildup to the mortal blow.

Adopting an even bolder approach, Peter began to duplicate and circulate copies of the correspondence he had had with editors of scholarly journals. I thereby obtained a paper file that is about two inches thick detailing such discussions. I particularly treasure his priceless correspondence with the Editor of the *Journal of the American Statistical Association* in which the Editor kept asking Peter to respond to the comments of a reviewer as excerpted by the Editor and Peter kept patiently saying he would be delighted to respond if the Editor would only send him a complete written copy of the Reviewer's comments. George Orwell would have been right proud of Peter. And Peter was deservedly right proud of his ultimate success in totally undermining the authority of this, and I use the word quite deliberately, crackpot theory.

In doing this work, Peter amply repaid the social investment that had been made in providing him with a university appointment. And, in this middle part of the pathway of his life, it was gratifying to see that Peter had the support of both his immediate colleagues and our university. Even though that support sometimes faltered a bit, it was always there and he was always treated properly in those days.

Unfortunately, I am sorry to have to say that my university faltered very badly in the twilight of Peter's career when it came to dealing with the problem of nepotism in the current world. I say this advisedly as a person who has recently seen how this problem has played out in our own family: We have a nephew on Louise's side who is a professor of law and he is married to a professor of anthropology. For some years, events dictated that he had to have a position at UCLA while she had to have one at the University of Chicago. This enormous burden was only lifted recently when she successfully competed for and took up a faculty appointment at UCLA in her *own* specific area of professional expertise.

I emphasize this last point because I had also seen an alternate way of dealing with this exact issue at the University of Wisconsin back when I took up my first faculty appointment there in 1967; that institution had then had an anti-nepotism regulation that enabled one specific spouse, whom I knew well, to enjoy a fully professional appointment that was directly in the area of his training while it forced the other spouse, whom I also knew well, to retool completely in order to fulfill the needs of a program for which she had not trained.

So I am fully aware that this is still a problem which lacks a completely satisfactory solution in any of the venues in which educated couples seek professional fulfillment. It is currently still possible for an educated couple to be either inappropriately favored or inappropriately handicapped by the situation. Without going into great detail, I am sorry to have to say that Peter spent the final years of his career dealing with the consequences of a spousal accommodation that had been inappropriately proposed for another person who might have eventually become a permanent member of his faculty area. I will not go into detail on the machinations that attended this case. Suffice it to say that Peter raised a principled objection to the accommodation being the outcome of an administrative procedure rather than of a review by disinterested scholarly peers. Sadly, instead of respecting Peter's integrity and honesty in this matter, some other members of his own departmental area withdrew from him. Not the least distressing aspect of this withdrawal was that it was both social and spatial in that his office was removed to a distant location. This was not an entirely unprecedented action because a peculiar and quite regular practice in our department has been the relocation of retired faculty to offices far away from those of the colleagues with whom they had spent a lifetime.

Having recently experienced a somewhat similar treatment myself, I have no doubt that this treatment was very hard on Peter but I am glad to be able to say that he fully supported me when the time came for me to undergo my own tribulation. Indeed, he never lost his cheerful approach to life right up to the very end. In this, he set an inspiring example as an independent thinker who had the courage and fortitude to follow the path of integrity and honor. I am therefore proud to have been his friend and colleague and to have enjoyed the pleasure of his company these many years. He will be missed.

[*] P.H. Schönemann & K. Haagen, On the use of factor scores for prediction. *Biometrical Journal*, 1987, **29**, 835-847.