

Confessions of a Dean

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Valedictory Lecture

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Mr Rector Magnificus, Ladies and Gentlemen,

More than forty-five years ago, my interest was piqued by a book written

Book Gusdorf

by the French philosopher Georges Gusdorf, entitled *La découverte de soi* (Gusdorf, 1948). Psychology students studying in Groningen under Professor Kouwer were required to sit an examination on that book. From hearsay I knew roughly what the book was about, but I missed the examination because in 1968 Kouwer died suddenly and at a relatively young age. Since then I have committed to memory everything that I have heard about that book or seen quoted from it and so, in time, I believed I knew roughly what it said. Without having read them, I know rather a lot of books in this way. Is that a bad thing? Of course not: books are frameworks both made of ideas and for hanging ideas on. Incidentally, I do not know for sure whether that will be true of the current generations of *googlers*, and I pity them, in anticipation as it were. Having a work room

My extended identity

of your own full of books, as the tangible expression of your view of reality, as your materialised self-reflection, for a psychologist like myself, that is pure joy.

Finally, this past winter I read that book. It does exactly what the title promises: it describes the philosophical and cultural history of the discovery of the most deeply individual inner self. An inner self that American psychologists term 'The Self', and some Dutch psychologists who have no feeling for language or style as '*Het Zelf*'. Any expression in which a personal pronoun is used as a noun should, in my opinion, be prohibited, but this I realise is a hopeless undertaking. Years ago, when I was a student, I read in Linschoten (1964, p. 364) that this is an expression of a reprehensible inclination towards substantivization, and philosophy is the field in which this tends to occur frequently. Indeed: whole philosophies are based on improper nouns such as being, becoming, being-towards-death, etc. The work of Heidegger and other famous deep thinkers are good examples in point; I myself am never entirely sure whether the depth of their work is not partly rhetorical superficiality. In any event, as the critical study of Heidegger by Herman Philipse (1998) shows, I may permit myself to surrender to what he calls 'disillusionment'.

Back to Gusdorf's book about the history of the most deeply individual. *La découverte de soi* is suspended between the *Confessiones* of

Picture of Augustine

Augustine, the holy Church Father, written 397-398 and Rousseau's *Con*

Page from the Confessiones

fessiones written in 1770. In his youth, Augustine was a bon vivant, he lead a sinful life, filled with drink, women and theft. In his *Confessiones* he describes that

wicked youth. But entirely in general terms. Actually he reports his own deeds for the sole purpose of contrasting sin and mercy. Augustine is continually in conversation with God in his *Confessiones*. He is not seeking his own inner self; no such thing existed in his time. Augustine's *Confessiones* is a systematic search for 'le secret de son âme' and that is his deepest trust in God, the triumph of mercy over sin. How

Picture of Rousseau

differently things are with Rousseau. He adopted Augustine's title but the Christian dogma, 'le sens du péché' (sin) and 'la grâce' (mercy) are gone. The point of this book, says Rousseau, is that I show to my equals a person "dans toute la vérité de la nature, et cet homme, ce sera moi ..."

Picture of the Confessions

Thus: "I wish to show my fellow men a person as he really is, and that person is me, myself." Here, Rousseau has described, if not created, once and for all the most deeply individual inner self. He writes correctly in the opening sentences of the *Confessions*: "I am undertaking something that has never been done before ...". Rousseau marks the point at which the theology of the *Confessiones* of Augustine transformed into the psychology of the individual. I think that if you have read Gusdorf's book, you cannot but conclude that the social and behavioural sciences would not be conceivable without this turnabout.

The shortest summary of the difference between the *Confessiones* of Augustine and the *Confessions* of Rousseau can be found in the customary translation of these titles into Dutch (see Doorman, 2012, p. 24):

Picture of Belijdenissen en Bekentenissen

Belijdenissen [Statements of Faith] for Augustine and *Bekentenissen* [Confessions] for Rousseau. Today I have chosen the Rousseauian version and so I have entitled my farewell lecture *Confessions of a Dean*. But this does not diminish the fact that I remain keen to seek the middle way between Augustine and Rousseau. I shall place candidly before you my decanal experiences, but I will not do this without pausing to reflect on what you might call sinfulness. Sinfulness without theological or religious implications, without special references to the higher, but instead with close attention to relations between people, rather than a too Rousseauian concentration on my own inner self, on myself. I will do this by taking the seven cardinal sins as the starting point for my dissertation. They were splendidly portrayed by Hieronymus Bosch or one of his

Picture of Hieronymus Bosch

followers, sometime during the first quarter of the sixteenth century. The

Seven Cardinal Sins (painting)
Seven Cardinal Sins in Dutch

sins in question are intemperance, vanity, lust, greed, envy, wrath and sloth.

Let me begin with *intemperance*. In a handsome photographic book (Ratcliff, 2010) about the life of Jean Piaget, the founder of developmental psychology, there is a wonderful photograph that shows Piaget sitting at his desk, which is strewn with handwritten sheets of paper and with untidy piles of printer's proofs, while all that paper on the left in the photo-

Jean Piaget and intemperance

graph is transubstantiating into an almost endless series of published books. As many as fifty in French, I reckon, but many, many more if you were to count the endless translations and reworkings. To this day, Piaget remains the most influential developmental psychologist in the world. But let's not forget that we needed the American developmental psychologist

John Flavell and his book

John Flavell to describe and clarify Piaget's work, and in particular the coherence in his work (Flavell, 1963). Piaget never got around to doing that himself. In fact, on one of his trips across the United States he is said to have acknowledged that it was due to Flavell's work that he himself was able to gain a comprehensive view of the structure of his own work for the first time (Wellman, 1991). He was simply too busy the whole time writing his next book to take stock in any profound way of his previous publications. I have always told my students that to understand the worldwide Piaget-oriented research, it is sufficient to study Flavell. Outside the Francophone world of developmental psychology, which has little influence, no one has read Piaget's highly impenetrable French. Even if Flavell's summary were incorrect, then still that summary has been more internationally influential than Piaget's own original publications. I infer that the famous Piaget simply wrote too many books. This is a case of intemperance, one of the cardinal sins.

Since Piaget, in my view, publication intemperance has gotten completely out of hand. Young academics who want to forge a career are constantly beavering away at their next article for publication, and worse still: they *have to be*. The quantity of articles that has been written, by way of example in my field alone, which is developmental psychology, is immense. I am sometimes afraid that we are making complete fools of ourselves historically speaking, and that a hundred years from now there will be no one left who understands why we all thought that we had to add extra droplets to the ocean of publications, and then preferably as often as possible. I have long hoped that it would turn out all right due to the impossibility of affording all those publications. Some 25 years ago it looked likely that publishers would increasingly demand that authors pay to get something published. In time that will become so expensive, I thought, that universities, say deans, will have to introduce measures. Every researcher would be given, say, once every three years, money to publish, provided he/she had carried out and replicated a series of groundbreaking studies, studies that guarantee some chance of successful influence on the development of the discipline. And take note: the next three years it is not your turn. That would have brought about a

sort of natural end to the excess. But, unfortunately, that is not what happened. Quite the opposite, we need to fear that the intemperance will increase even further thanks to the fact that virtual publication media are popping up like mushrooms. Now and then in my time as dean I have thought about introducing publication moratoria. Long periods in which publication is prohibited. But for which you would be rewarded with time for reading and thinking, and the development of truly creative thoughts. But, of course, I never dared. We have to score, be better than the others in benchmarking, come first in comparative external reviews, be cited, acquire subsidies. In short: take on the competition within and between universities and win. And in all that, unfortunately, rules apply that reinforce the excess.

Naturally, closely related to the intemperance described above is *vanity*, if not *lust* as well. I openly admit that now and then I have occasion to print off my own list of publications and then I enjoy its length and breadth. And I do my best to add at least ten new publications to that list every year. And that I manage this appeals likewise to my vanity. But if you were to ask me: which of your own publications do you think are fundamentally important to your field? Then not only would I have to rethink my answer on each occasion but the ultimate answer would also be a little disappointing. So right here and now I would like to dispense with any further analysis. The vanity described here, which naturally I prefer to reformulate as 'love for the discipline', is evidently widespread these days.

The excess I have described means that we can no longer read each other's publications. We have neither the time nor the thirst to drink the ocean dry. So what do we do: we make up algorithms, formulas, that say something about publication success. It becomes a question of what is known as bibliometric analyses, citation analyses, impact figures, and the h-index. These are all algorithms that can be critical these days to the appointment of researchers. In a good but equally lamentable article in the

NRC article

science supplement of the NRC of 17 March 2012 it was explained to newspaper readers what the h-index stands for and everything that is wrong with it. The h-index was conceived by the physicist Hirsch, who

Hirsch's paper

wrote an article about it that has become one of the most frequently cited in recent years (Hirsch, 2005) and has done no wrong to Hirsch's own h-index. Until the invention of this h-index scientists calculated their impact by counting publications and citations. In essence with the h-index a weighing was introduced. Someone with, for example, an h-index of 50 has written 50 articles, each of which has been cited at least 50 times. The h-index is regarded by many as being a rather reliable indication of a scientist's track record. The h is quick and easy to establish and explain. And, I will add as an extension to the discussion about excess, it does not require us to read even one publication of the person being assessed.

There are, unfortunately, more faults to be found with the h-index than I wish to touch upon here and now, such as the impossibility of comparing the index between different scientific disciplines; the fact that a revolutionary breakthrough in a specialist area contributes less to the h-index than a small step forward in a large and popular discipline; the well-known fact that the few articles written by Albert Einstein that were truly groundbreaking lead to a low h, because the infinite numbers of citations of his few articles do not lead to any higher index than had they been cited a couple of times; etcetera and so on. Nonetheless, the lunacy surrounding the h-index is almost unstoppable. The NRC article referred to above reports that there are researchers "who first thing every morning check their h-index, as if it were viewing figures". I know this to be true. In my own faculty I know professors who know their own daily fluctuation in the h-index - and that of their immediate peers - and even on occasion, in anticipation of what is known as an Assessment and Development Interview with the dean, have represented that fluctuation in graphical form and taken it with them, in order to demonstrate their upward line. Incidentally, these are certainly the better researchers, generally speaking. And so I easily forgive them this non-argument, but ultimately I find it reprehensible vanity. The motivation for practising science is curiosity, is a quest for truth in all its revealed deficiency, but it most especially is *not* a competition that must be won about numbers of publications, citations and the highest h-index. The ultimate goal of science is not the winning of a medal or cup, as it is in the sporting world. Such vanity leads to extremely deplorable trivialisation.

I am never able to be unreservedly enthusiastic when it turns out yet again that Utrecht University holds a high position in a world ranking of universities. In spite of everything, the relentless propagation of such results remains a form of vanity, one that every now and then borders on *lust*. But as an administrator, who must have results, it is difficult to detach yourself from all that. For example, Utrecht University tends to score highly in the Shanghai world ranking of universities; and so in Utrecht we believe that is an important list. A couple of years ago, it happened that the UU had dropped a couple of places in this ranking. In the University Newspaper it was explained that this was due to the Social Sciences, which were weaker than in Tilburg and Rotterdam. *Weaker than in Tilburg and Rotterdam?* I found that hard to believe, and I looked into it. Well, it turned out that the methodology used by the Shanghai list did not provide for the inclusion of psychology, pedagogical sciences, and sociology, but instead chiefly allowed other fields including public administration and economics to weigh heavily. Thus the designation Social Sciences had an extremely limited and unusual meaning, and the term had absolutely no bearing on my faculty's fields of science; mercifully my faculty has absolutely no disciplines like public administration or economics. I reported this widely all over the university and demanded a rectification in the University Newspaper. In all those announcements I stated unequivocally that my faculty simply had not contributed in any way whatsoever, nor *could* have contributed, to our ranking in the Shanghai list. Two years later we had shot back up a couple of positions. A high-ranking administrator at our university - I name no names - concluded at the time, apparently based on a fundamentally positive attitude towards my faculty and myself, that we probably owed this mainly to the hugely successful 'policy' of the management of the Social Sciences. I don't mind admitting now that at the time I deluded myself into thinking that further expla-

nation would be pointless. So I did not explain, but of course I am well aware that my vanity and the apparent success of my faculty were deciding factors in my being silent. As an administrator you can allow yourself to be a little more human than you can as a scientist.

I hasten to make amends for this painful example to some extent by adding that Utrecht University scores highly not only in the Shanghai list but also in a number of other lists that are seen as being important. And, as I learned a long time ago in psychometrics, if you average out across all those various ranking lists, you lose all sorts of "error variance" and you do end up with something meaningful. So our administrators are justified in being a little bit proud. But I cannot help myself. After all, pride may well lead to vanity.

All that scoring and bother with rankings does lead, of course, to a great deal of competition. Between universities, but also between faculties, and even between chair groups and between chair holders. And not infrequently this goes hand in hand with some *greed*. More money for *my* research and *my* teaching activities, for *my* laboratories, for more staff for *me*, and so on. There is a passage about greed in Matthew, Chapter 25, Verse 29:

Matthew, 25,29 in Dutch

*For unto every one that hath shall be given, and he shall have abundance:
but from him that hath not shall be taken away even that which he hath.*

It was this Evangelical phrase that prompted the conception of the Matthew effect. The scientific concept of the Matthew effect comes from the field of sociology. It was Robert Merton who in 1968 introduced this concept in the field of the sociology of science and who wished to use it to denote the following. The Matthew effect states that those persons who possess economic or social capital can use these sources to acquire *more* power and capital (Merton, 1968). He also meant that eminent scientists usually get more *credit* than relatively unknown researchers, even if the latter's work is equally groundbreaking. Furthermore that prizes are won mostly by senior researchers, even if the actual work was done by a teaching assistant or doctoral student.

For the umpteenth time in history we must make severe cutbacks within the universities. And as always, at all policy levels, from the ministries up to and including the universities themselves, it is thought that cutbacks provide a very good way of raising quality. Everywhere, two words are buzzing in the air: *Veerman* and *Profiling*. The name Veerman refers to the chair of the *Toekomstbestendig Hoger Onderwijs Stelsel* [Futureproof Higher Education System] committee, which published a much-quoted

Veerman's Report

and highly praised report entitled *Differentiation in triplicate. For the sake of quality and diversity in higher education* (Veerman, 2010). That report is primarily about 'profiling', in other words creating a distribution of tasks between higher

education institutes. Veerman and profiling mean: choices must be made; what is good stays, what is less good is disposed of.

And in our faculty, too, we contemplated our further profiling. Yet again, for the so-manieth time in the history of Dutch cuts to scientific education and research, it was and is becoming clear that it is no easy matter to agree exactly which criteria you need to apply to establish what is good and less good. Of course, we have all kinds of modern indices for research, as mentioned above: citations, h-indexes, the impact of journals, assessments by external review committees. We, by which I mean the Faculty Board, came up with the idea that you could very well take the research programmes as a starting point and let a number of quality criteria loose on them. Such as quality of the research in terms of external review assessments, acquisition power in terms of indirect funding and contract funding, the research's connection with high-quality teaching programmes and substantial student numbers at the Bachelor's and Master's levels, numbers of dissertations and, in particular, future possibilities in the light of the profiling of university research and education in Utrecht. All this results in a great deal of clarity and differentiation, which can be a very good starting point for policy.

But there are a number of problems. Comparisons across disciplinary boundaries cannot be made just like that. For example, it is the case that scientific research at the interface with the scientific-medical world is easier to value quantitatively than research at the boundary with the humanities world. Thus experimental psychology can be properly assessed using the current indices, but cultural anthropology, with a book culture rather than an article culture requires a more qualitative valuation. Sociology would seem to lie somewhere between the two. If you want to keep a social sciences faculty running, you will need to do justice to both the quantitative experimental and the more idiographic approach and you will be obliged to allow very respectfully for differences in scientific culture. In this sense the 'gamma' world of the social sciences is a *pars pro toto* for the university as a whole. And for that reason alone we have a duty to set a good example.

A very difficult extra criterion, one that once again is increasingly garnering attention, is *social relevance*. The interesting thing about this criterion is that it weighed heavily in the 1970s and 1980s, when its chief meaning was 'socially critical relevance', challenging the establishment. Now, by contrast, it means supporting the establishment, hence the language of *valorisation*, *knowledge transfer*, and the *knowledge economy*. Like others, we too in the Social Sciences attach great importance to social relevance, but then in both senses of the word: critical where necessary, supportive where necessary and possible. At the same time, we also know that although applied scientific research may have a significant social impact, it does not always lead to revolutionary scientific discoveries. As real balancing artists, we do our best to apply a set of criteria to our faculty's education and research, and ultimately to make choices in such a way that they are transparent and fair, although they are inevitably also painful.

In this respect, we are aware of the hazardous Matthew effect. I shall try to make clear what this means in this context. Imagine what would happen were we recklessly to apply quality criteria and were we to reward without restraint so-called 'success', something that people on the work floor often demand mercilessly of

us. What would have happened had we, 20 years ago, based policy on success in acquiring indirect funding and contract funding, and on scoring in high-impact journals? Had we done that we would now have a faculty consisting almost entirely of experimental psychology, because in the 1980s and 1990s the practitioners of that subject were the most successful with the Netherlands Organisation for Scientific Research (NWO), and initially they also had great success in acquiring contract funding. This led to and still leads to such lines of reasoning by the practitioners as: we have now acquired so many PhD candidates that we have insufficient staff to supervise them, so we need more professors and associate professors. Hopefully you grasp immediately that this leads to an unstoppable Matthew effect. I can tell you that *now* an unrestrained Matthew effect would lead to a faculty with experimental psychology, social psychology, developmental psychology, clinical psychology and sociology, and with very extensive, robust blocks of pedagogical sciences. And naturally all this would be at the expense of subdisciplines and groups that, for reasons of discretion, I will not name here. It is my firm conviction that we must not encourage this kind of Matthew effect. Certainly not in the sense of endless matching and the supporting of groups that are extremely successful.

Should success not be rewarded then? Yes, of course it should, but not without limits. And that is why we are now considering the profiling as the most important criterion for identifying where to make cutbacks. Where shall we direct our energies, given what is present in the way of basic quality and given the future possibilities within the university and within Europe. In addition, we are also considering the possibility of balancing size and focus. Bigger and broader is not by definition a prerequisite for any tour de force in terms of quality. Perhaps we can also identify where to make cutbacks by making groups that are performing very well even better by, paradoxically, reducing their size, and in such a way that their weaker elements, relatively speaking, are phased out.

This complex process of making choices will dominate my successor's agenda for some years to come. It will be difficult, and here and there extremely saddening where hard-workers and sympathetically-minded individuals are concerned who no longer fit optimally in the strategy to be chosen. Let us not give the greed that results in a radicalising Matthew effect a foothold here. Local greed, which is not in the faculty's general interest, leads to disruptive interpersonal relationships that go hand in hand with *envy* and *wrath*.

And with the above I have worked through the list of the seven cardinal sins, with the exception of *sloth*. And that is not by chance. In all my years I have come across no sloth in my faculty. The motivation to conduct scientific work stems from what I like to call *Funktionslust*, the joy of being engaged for its own sake. Perhaps I can be allowed to confess that I am familiar with forms of partial sloth. I will give an example. Announcements made via our electronic media, websites that it takes an effort to avoid, and via which, as administrators, we share our views and plans and intentions with the work floor, have a very low readership. People do not take the time. They want to get down to work on a dataset, on their next paper, on writing grant applications, on preparing teaching and conference appearances. I understand that, but find it hard to accept that this same partial sloth results consistently in complaints about internal communication. The

line of reasoning is often: what *I* do not know has not been communicated well enough. That is a big problem in this era of excessive information.

I opened today with a reference to the French philosopher Jean Jacques Rousseau, who was born 300 years ago this year. In his *Confessions* he is seeking personal authenticity, naturalness and spontaneity, which play an important role in our contemporary thinking. But Rousseau's authenticity is problematic and riddled with paradoxes. For a start, he wrote that he had written his *Confessions* only for himself. But that is, of course, at odds with its deliberate publication. To realise the authenticity, the genuineness of our own selves requires, strangely enough, all sorts of artifice. Maarten Doorman recently wrote (2012, back cover) that a straight line runs from Rousseau's longing for the genuine to the virtual smiley and

Rousseau and Emoticons

other emoticons.

Sentence in Dutch about Facebook and Rousseau

"Thanks to Facebook we understand that we must play ourselves, just as the authenticity of *Boer Zoekt Vrouw* [a reality TV programme] is orchestrated and the sincerity of the politician is the product of media training." Authenticity works best when it is acted. Against this background, is there anything sensible I can still say about the authenticity of a dean? Not much more than that I am convinced that a dean must be, first and foremost, a practitioner of science.

By way of confessions I have shown you a number of interpersonal weaknesses that lie in wait for everyone, intellectuals in academia and myself included. The beauty of science is that in its very practice, we arm ourselves optimally against these weaknesses. Our weapons are our strict methodology, our transparency and verifiability, and our ineradicable urge for what my esteemed supervisor Willem Hofstee recently named 'ontkennis' – a Dutch word play on denial and knowledge: "... the urge to know how things are *not*, and the immediate onset of doubt at any display of conviction" (Hofstee, 2008, p. 38).

Hofstee quote in Dutch

Thanks in part to him, way back in the sixties of the previous century I learned and accepted that there are three ways in which you can practise science: teaching, research and administration. I have always borne that in mind as I have tried to be an administrator; always maintaining a basic scientific perspective. And so I have never become, I am firmly convinced, a 'manager' or bureaucrat, and I even go so far as to hope that here and there this has been appreciated. Taking this view of things, in all these years I have not been able to refrain from staying scientifically active, even if only to keep my own scientific attitude well exercised.

I must say, however, that science and, certainly, the development and implementation of science *policy*, ultimately ends up being a variation on

Myth of Sisyphus

the *Myth of Sisyphus*: we roll an extremely heavy and perfectly spherical rock up the mountain, only to not quite reach the top and to have to start all over again, and again. This is the third time in forty years that I have had to consider jointly with others how we are to deal with the impact of very severe financial assaults on the universities, and I am convinced that the end is not in sight, although for myself I hope at some stage, at a more advanced age, I will be less affected by it all. In the 1970s and 1980s, I somewhat undermined several of my family's summer holidays because I had to assess more than 100 research proposals for NWO, which was then still ZWO, in the months of July and August. This was in order to prove to ZWO that as psychologists we were actually able to assess competently. When, after a couple of years, we had evidently provided that evidence, we were allowed to set up the Psychon Foundation for psychological research. In any event, all that summertime work had ensured that I maintained a good understanding of the subsidy policy, and thus was able to do something for my discipline. And now we have a government that would prefer to see research policy being managed by Economic Affairs rather than Education and NWO funding is being transferred to the knowledge economy, where the optimisation of interpersonal relationships, to which the science in my faculty can contribute so well, has no priority. We must compensate for the disappearing NWO funding, that much is clear, by linking up with the Top Sectors the government has made up. But, I hear you say, how do you get social science knowledge to link up with energy, agro-food, market gardening, chemistry, water and my absolute favourite, the category 'head offices'? Clearly we will have to redirect our focus towards the European level of subsidy. But how do you operate on a European level, when the agenda-setting is done a long way from our work floor and the assessment procedures that we have accumulated in forty years of NWO experience must be reinvented all over again?

It is my firm conviction that science consists of masochism, i.e. joyful suffering, and that is why the extreme motivation of Sisyphus is exemplary of the true scientific attitude. And, in a paradoxical way, that attitude makes a person happy. Because, as Albert Camus wrote: "The struggle itself towards the heights is enough to fill a man's heart. One must imagine Sisyphus happy." (Camus, 1942). Based on my own experience in science I agree wholeheartedly with that sentiment.

Camus' quote in Dutch

Ladies and Gentlemen, at the end of this farewell lecture it would be ap-

Picture of final state of a meeting

propriate, I think, to express words of gratitude. But the problem is that I would have to thank an awful lot of people for very many things. I imagine that this would become boring for most of you and that with a few words of thanks I could not even come close to expressing what I would like to. Let me conclude in this way: I have experienced an abundance of trust and appreciation from the Executive Board and from my colleagues within the faculty of Social Sciences. I hope

that I have not betrayed that trust at any time. And if I have managed that, then it is thanks primarily to the collegiality of my fellow deans within our university and to the exceptionally loyal and dedicated support of the director and vice deans in our faculty, and not in the least to the support staff, ranging from my Personal

Nine pictures of groups that support the Dean of Social and Behavioral Sciences

Assistant and the Secretariats right through to Human Resource Management, Finance and Control, Communication and Marketing, the Research Support and Grant Office, the Teaching and Learning Support, international Office, and Housing and Services As I said: too many to mention. I have experienced a great deal of friendly collegiality and spontaneous cooperation. So much so, that I have often thought that it would all run smoothly without me, and even in the spirit I would myself wish. That was and remains a reassuring notion.

Should you feel the inclination to applaud in just a moment, then I request that you do so for the chief purpose of encouraging my successor, Werner Raub, and also to express that you join me in wishing the faculty of Social Sciences a rosy future.

I have spoken.

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