This is a written version of my speech at Bert's Fest. I did indeed prepare a speech for the event, but it was not written out. It consisted mainly of bullet points. So what is written here was reconstructed afterwards, mostly based on those notes, and some of it as a reaction to things that were said.

As soon as the date turned out to be Friday, May 13, Beatriz and I started joking about what might possibly go wrong. Beatriz had painstakingly tried to close all imaginable loopholes, but the one thing nobody thought possible was two taxi vans driving to the wrong hotel to pick up the speakers, even though they had written instructions including not just the name but also the address of the hotel. For this reason the whole event started half an hour late. On top of that came the usual delays because of questions, and technical problems with zoom and the beamer. We anticipated those, but the extra half hour reduced the time left for the expression session so much, that the organizers urged me to shorten my speech. So here is what I probably would have said, if I had more time.

This has really been a fantastic day for me. One of the few meetings one ever attends where one gets *more* credit than one deserves. So I will start by thanking everyone who made this possible.

I will start with the main organizer, who is also my most frequent collaborator, and my wife. Beatriz, what an amazing gift you have given me!

When she first came with the idea for this meeting, in the fall of 2019, my reaction was that I was not important enough. A standard reception with some snacks and some beer, a speech by the director and the head of the theory group would be good enough for me. But she insisted, she never gives up, and neither did Robert Fleischer, the head of the theory group, who joined forces in the organization, and began instructing her what had to be done. So I gave in.

Then we started discussing speakers. They were going to be a selection of my collaborators. I told Beatriz: "All these people are distinguished professors now, or directors of research institutes. They are not going to come to Amsterdam for one day, just to honor me!" However, she began inviting people, and much to my surprise the majority said yes immediately. So she made a very impressive programme, found a hotel with the help of the Nikhef secretaries, and together we found a restaurant for an evening dinner. The date we chose was 10 July 2020, six days after my official retirement date. We were all set.

In the beginning of 2020 we started getting worried about Covid. The dark cloud was moving in our direction. We talked jokingly about it, but Bert's Fest was still far away, so we did not worry too much. Then on 26 February one of our speakers cancelled because of Covid, which had already hit his country, Italy, very hard. Beatriz replied to him:

We know the present situation, but the 10th of July is still very far ahead. If things get worse in the next months, then we will postpone the meeting a few months or even a year if necessary.

Spain had its first Covid case on January 31, and in early March the situation started deteriorating rapidly. On 27 February the Netherlands had it first case. Beatriz was

scheduled to come to Holland on April 1 to assist me after a minor eye operation. She was worried that she might not be able to leave Spain in time anymore, and she advanced her flight twice. She came to Holland on March 11, and I had advanced my eye operation to Friday the 13th. Despite the ominous date, it went well. Little did we know that she was going to stay in the Netherlands for one and a half year. She used the time to work on her book "Antimatter", and since I was at home I could help her by proofreading all the material, and making on the computer her handmade drawings. Sadly, on May 10 Bert's Fest was postponed to October 23.

I officially retired on Saturday July 4, and I went to Nikhef that week, because it seemed strange to retire while working at home. Covid had subsided a bit, and on my last working day, Friday 3, Beatriz and I met with a few PhD students who had found some cans of beer in the theory group refrigerator. We drank them sitting at two meters distance from each other, respecting the Covid protocol. That was my retirement party.

Then also the 23 October date was cancelled, and a new date of 25 June 2021 was chosen. Once again the hotel and restaurant reservation for 23 October had already been done, and all the work put into that was lost. Already on 29 December 2020 Robert suggested that a Bert's Fest in the summer of 2021 might not be possible, and proposed moving the date to the first half of 2022. The date of 27 May was chosen, and after it had been distributed to many theorists, finally an experimentalist pointed out that it was a holiday. And that is how we ended up with Friday the 13th. Obviously, all this rescheduling has cost Beatriz a huge amount of additional time. The easy way out would have been to just give up. I said I was already sufficiently honored by your acceptance. But giving up is not a word in her dictionary. And I will be forever grateful for her persistence.

Also many thanks to Robert, who always backed up Beatriz. The organization has cost him a lot of time as well, but he never seemed to have any doubt that we should go on. And I am also very grateful to Joan Berger, one of the Nikhef secretaries, who also did a great job with the local organization.

Then I also want to thank all of you for being present. It is a great pleasure to view an entire auditorium full of friends with whom I had such exciting times.

Also thanks to the groups where I did most of my work: the theory groups of Radboud Universiteit Nijmegen, Fermilab, Stony Brook, CERN and Nikhef. Perhaps I have been lucky, but I have fond memories of all of these places, because of their pleasant, open and active scientific atmosphere.

In Nijmegen I did my master and PhD, working under the supervision of Rogier Van Royen, but in practice with Willy van Neerven. Sadly, both have passed away at a much too young age. I wrote my thesis on Perturbative QCD and Lepton Pair Production.

In my first postdoc at Fermilab I changed course completely. This was possible in those days, because postdoc positions were not tied to specific projects. I fully exploited that freedom, and worked on composite models, technicolor models and magnetic monopoles. Fermilab had a very lively theory group, and I collaborated with Andrzej Buras, Sally Dawson and Cosmas Zachos, among others.

Then I went to Stony Brook and switched gears again. I started working on Kaluza-Klein theories. The Stony Brook theory group was dominated by Peter van Nieuwenhuizen, who turned every seminar into a memorable event. He was merciless with any speaker who tried to bluff his way out of a difficult situation. In the beginning of 1985 he asked me and another postdoc, Krzysztof Pilch, to present a Journal Club talk about one of the early string papers that re-started string theory in 1984: a famous paper by Candelas, Horowitz, Strominger and Witten about compactification of the heterotic string. Knowing that we could not get away with humbug, we examined the paper thoroughly,

and each of us found one mistake, which taken together ruined one of its arguments. To make a long story short, the net result was that our names were mentioned in the published version of the paper. The rumour spread across the US, although we had done our best to inform the authors discretely. On the basis of this I was invited to speak at the Argonne conference on anomalies and geometry, which was actually about string theory. Since we had nothing to say on the subject of the conference, Krzysztof and I decided to write a paper on string-inspired compactifications, and as a result we had entered string theory. I have never regretted that, and I am sure neither has Krzysztof, who is now professor at USC, Los Angeles, and is still a good friend.

My next position was at CERN. The theory group there was in a highly excited state because of string theory, and I quickly discovered that string theory was a lush orchard with a lot of low-hanging fruit ready to be harvested. In the year 1986 I worked with Hermann Nicolai and François Englert, with Nick Warner, with Krzysztof Pilch and with Wolfgang Lerche and Dieter Lüst. With Nick I constructed the "elliptic genus", which was not our name but definitely our idea, and with Krzysztof and Nick we showed how to compute that object using a path integral. I am convinced that this paper gave a big boost to our careers. With Wolfgang and Dieter I wrote one of the papers indicating that the initial expectations of uniqueness of string theory were misguided. We used a four-dimensional string construction we called "the covariant lattice construction". This paper also gave our careers a big boost.

I had started my career in the time of Grand Unification, and I loved that idea. The Standard Model is described by one particular quantum field theory, a seemingly arbitrary choice out of many possibilities. Surely one day we would compute this choice from first principles, so that Einstein's God had no choice when he created the universe. Grand Unification looked like a step, albeit small, in that direction, and string theory initially promised something even better. People already started fantasizing about computing the electron mass.

But string theory clearly pointed in a different direction. Perhaps the theory was unique, but the universes and their laws of physics it described might belong to a huge ensemble of possibilities. Einstein's God would still not have any choice of fundamental theory or any option when creating the multiverse, but that multiverse would dynamically evolve to a huge number of universes, all with different values of the electron mass and other parameters. This is a perfectly reasonable alternative to the Einstein point of view, that all the parameters should be uniquely derivable from some underlying theory. Once one understands that, the Einstein point of view, usually defended by phrases like "never give up" or "do not throw in the towel", turns out to be nothing more than a belief.

Even if string theory would turn out not to be a "theory of everything", it was at that time at least a *model* of a theory of everything. It showed what a consistent theory containing gravity and gauge theories might have to say about the groups and parameters of the latter. And everything we knew then and know today indicates that it does not determine them.

There was another thing in the back of my mind, a thought probably seeded into my head unintentionally several years before at Fermilab. It was the observation that life as we know it would be impossible if certain changes were made in the Standard Model. If that model was indeed a special choice that allowed intelligent life, then how could it possibly have been selected by pure mathematics? It would make far more sense that it was selected by sampling a huge set of distinct universes by some dynamical process.

I remember these two thoughts coming together as a true "Eureka" moment. It happened around May 1987, exactly 35 years ago. It was a pivotal moment in my

career. After that, I could never look at the Standard Model in the same way again. For about one year I was contemplating writing this up, perhaps in a paper entitled "the Anthropic Landscape of String Theory", but I had no formulas to present, nor the courage to write something so provocative at this stage of my career. Fifteen years later Lenny Susskind wrote a paper with that title, but it was based on a lot more than I knew in 1987. It discussed a fairly concrete landscape of de Sitter spaces sampled by eternal inflation, and a mechanism for dynamically neutralizing the cosmological constant presented by Bousso and Polchinski. I was delighted when that paper came out, and started dreaming that perhaps this whole idea would be firmly established by the time I retired. But unfortunately that has not happened. There is now an ongoing debate about the question if de Sitter vacua even exist in string theory. But since I came to my conclusion prior to the speculation about a de Sitter landscape, I see no reason to change my mind about my point of view. Even if the Standard Model comes out in a different way, there is still no more reason than in 1986 to believe that it will come out uniquely.

I used that point of view as the theme of my inaugural speech in Nijmegen in 1998. This was in Dutch, and Beatriz tried to convince me to translate it to English and send it to the arXiv, without any success. Only after Susskind's paper I had gathered enough courage to do that. A few years later I was asked by Peter Zerwas to write a review on the landscape for IOP, and some more years after that Paul Langacker also asked me to write another one for Reviews of Modern Physics. Despite my enormous initial reluctance to write about this, somehow my name got associated with the landscape concept, to which I actually contributed very little scientifically. It is still viewed with so much skepticism, by nearly everyone, that my name comes up very quickly if one is looking for someone to write a review about it.

Still, an unexpected "confirmation" of the landscape came out more than thirty years after my initial thoughts. In 2018, our current minister of Education, Robbert Dijkgraaf wrote an essay entitled: "There Are No Laws of Physics. There's Only the Landscape." I thought: "That sounds familiar!" Unfortunately he gave no sources, so I can only guess how he found out. But he states rather categorically that "modern physics allows for many different descriptions [...] connected through a vast landscape of mathematical possibility". *Roma locuta, causa finita.*

But now I will return to CERN. As Wolfgang mentioned in his talk, he convinced me to try skiing, and I loved it. I got so fanatic that one Sunday I woke up at 6 am (already an incredible feat) and I drove to Les Carroz to beat the queues, with the goal of doing all the black slopes in Le Grand Massif in one day. Apart from one slope that was closed, I succeeded. This does not sound at all like me, but that is also what I thought today, when every speaker was explaining how smart I am.

In subsequent years I gave many lectures and attended many conferences, and I continued some work based on the successes of 1986. The most exciting of these was "Anomaly Cancelling Terms From the Elliptic Genus" with Lerche, Warner and Bengt Nilsson. Evidently, we had already surrendered, and called the object Nick and I had constructed in 1986 "elliptic genus".

In 1989 I started another line of research, in collaboration with Shimon Yankielowicz. It was an algebraic tool we called "simple currents", used to construct string partition functions. Just as the elliptic genus, simple currents found their way to mathematics. I see this term still used occasionally in the mathematics literature, although the mathematicians usually have no idea where it came from. Later Shimon and I applied this tool to string model building, and we addressed a technical problem called "simple current fixed points". Our results were empirical, but later Jürgen Fuchs, Christoph Schweigert

and I put all of this on a solid basis.

In 1990 I met Beatriz at CERN, obviously the most pivotal moment in my life. She already had a permanent job at CSIC, Madrid, but was on leave to work at CERN. I essentially had a permanent job at Nikhef already, although not formally. In 1987, when I was offered a fixed-term staff position at CERN, Nikhef promised me a permanent position afterwards, allowing me to stay at CERN until my position ended. So Beatriz and I could enjoy more than a year living together, knowing that after that we would go to our respective institutions, and travel a lot between Madrid and Amsterdam. In August 1991 we got married in St. Genis-Pouilly, the French village where we lived. Our wedding picture looks a bit like that of a physics conference, with many colleagues from different countries. Several prominent physicists were present; among them Dieter Lüst (who acted as witness) and Elias Kiritsis, both speakers at today's meeting.

Beatriz also became my most frequent collaborator. We worked on many different topics and wrote a total of 14 papers together. In collaboration with her I started working on the complete classification of simple current modular invariants. This was completed in a paper with Max Kreuzer, who died in 2010, and who became famous for his classifying work on Calabi-Yau manifolds.

As part of the deal with Nikhef I visited the theory group for a month every year. During the last of these visits, in 1991, I suddenly realized how I might solve a problem posed by Peter Goddard in 1989, the classification of the meromorphic conformal field theories with central charge 24. Although I had a physics excuse, the classification of ten-dimensional heterotic strings, it was really a pure mathematics problem. To solve it I used simple currents, the elliptic genus, and a lot of computer power, partly at Nikhef and partly at CERN. It resulted in a list of 70 such theories, which I hoped would one day find their proper place in mathematics. This finally happened very recently, in December 2021, as Nils Scheithauer explained to us this afternoon. Interestingly, thanks to Covid, this work was completed just before my retirement celebration. And the same can be said about the result reported today by Jürgen Fuchs, but I will not go into details now.

My first decade at Nikhef was invigorated by a collaboration with Jürgen Fuchs, then a Heisenberg fellow at Nikhef, and Christoph Schweigert, then a PhD student who was officially a student of Robbert Dijkgraaf, since neither Jürgen or I had a professorship. The great mathematical skills of Jürgen and Christoph allowed us to return to the simple current fixed point problem and really solve it. In doing so we introduced some novel concepts that once again found their way into mathematics: orbit Lie algebras and twining characters. There is even a twining elliptic genus. Around 1997 Jürgen and Christoph left Nikhef. They eventually became professors in Karlstad and Hamburg, respectively, and are still often collaborating.

Towards the turn of the millennium an important shift of focus took place. We started getting interested in conformal field theory on surfaces which had boundaries and/or no definite orientation. This was for a large part due to Augusto Sagnotti, then at Tor Vergata University in Rome. He was one of the pioneers of open string theory long before anyone understood the importance of that branch of string theory. He convinced us that our results on fixed point resolution should have important applications in open strings, and he was absolutely right. He invited me and Beatriz to Rome for a three month visit, and lectured us relentlessly about everything he knew. This was another pivotal moment in my career. Augusto is one of those people that I should have written a paper with, but never did. However, during that period I did write a paper with Yassen Stanev, from Augusto's group, a very smart and amicable physicist from Bulgaria, who passed away five years ago.

Christoph and Jürgen started working on boundary conformal field theory in 1997, after having left Nikhef. In the same year I became special professor in Nijmegen and then, finally, I could have my own students. The first two were Lennaert Huiszoon and Nuno Sousa, and I started working on non-orientable strings and boundaries with these two students in 1999. Then we combined our efforts with those of Christoph and Jürgen, and the whole project culminated in 2000 with a paper containing a single formula summarizing everything we had done. We were so pleased with it that we jokingly referred to it as the "formula of everything". But it did not only wrap up the past: we already anticipated that this formula would open the way for a systematic exploration of open string models.

In the second half of the eighties many string theorists, including myself, had been partly successful in getting Standard Model like spectra from closed strings, but open strings seemed less promising. That changed with the second string revolution around 1995, and especially the discovery of D-branes. This started a period of model building with intersecting D-branes, or equivalently open string theory. We knew we were ready to join that effort, thanks to the "formula of everything", but the field seemed already overcrowded, and groups from Madrid, Munich, Philadelphia and Crete, among others, were already reporting partial successes towards getting the Standard Model spectrum.

In early 2004 my ex-student Lennaert Huiszoon (then a postdoc in Leuven) pointed out to me that all these successes left a few things to be desired, and that we might have something interesting to add to that. With Lennaert and my student Tim Dijkstra we started a systematic search, and within a few months this was successful. Thanks to a decade of mathematical work, encoded in my computer program called "`kac`", which I have worked on since the eighties, we had an unbeatable advantage in this area. Two years later Elias Kiritsis approached me with an idea of a more general class of Standard Model embeddings, which he had defined together with his student Pascal Anastasopoulos. Together with Tim Dijkstra, we performed a successful systematic search for such models.

In the following years we exploited our advantage in this kind of string model building in further work with Elias and Pascal, with George Leontaris and Robert Richter and others. With Beatriz I tried to build non-supersymmetric models (all previous ones were supersymmetric). With Luis Ibañez and Angel Uranga I worked on instanton-generated neutrino masses, and discrete symmetries related to axions, a massive computational effort. With my fourth student Michele Maio I made the first steps to extend the class of open string models by including permutations. Furthermore, I returned to heterotic string model building with Beatriz.

One of my last papers so far, written with her, is entitled "GUTS without guts". Because of my anthropic landscape point of view, I had grown increasingly skeptical of the entire SUSY-GUT idea. In 2008, just before the LHC started (though it turned out to be a false start) I gave a talk at CERN where I showed all the arguments in favor of low-energy supersymmetry and crossed them out one by one. In the paper with Beatriz we showed that all the celebrated successes of Grand Unification, such as family structure and the peculiar charge quantization, could all be derived from a simple D-brane picture and some anthropic arguments, basically the existence of nuclei. These models looked like GUTs, but they did not have an internal GUT structure,

Finally, a few words about Nikhef. Nikhef is primarily a particle physics institute. You might think that this is a strange place for a string theorist. But deep in my heart I am a particle physicist, and I wanted to stay close to my roots. In fact, in 1998 the two opposites in the Nikhef theory group joined forces, when Jos Vermaseren, Timo van Ritbergen and I wrote a paper on group theory factors for Feynman diagrams. It is to

my knowledge the only paper co-authored by two Nikhef theory staff members, and it became my second most-cited one.

When I started at Nikhef I assumed that the gap between string theory and particle physics was going to become a lot smaller during the rest of my career. String theory was still moving ahead, although it was not always clear in which direction, and many experiments were planned that might shed light on physics at shorter distances, in particular at the LHC. Even the SSC was still on the table. So, there was hope that low energy supersymmetry might connect particle physics to Planck scale physics.

Sadly, the gap has not been reduced. In the meantime, the Nikhef theory group moved in the direction of particle physics phenomenology and cosmology while string theory did not get any closer to experiment. Nevertheless I have always felt comfortable here. Nikhef is an exciting and delightful place, and there is always something to celebrate. It was great to be here when the Higgs was discovered and the first gravitational waves were observed. The experimentalists appreciated and respected me, although they could not resist the occasional joke about string theory or the landscape. But it was always in good fun, and I always thought: just wait, and I will have the last laugh.

Well, even today, on the day of my retirement, I cannot say that yet. I am not sure what the future holds for string theory. I just know that it is the most magical, mystical and majestic (paraphrasing Ed Witten) theoretical edifice mankind has ever stumbled upon. It contains gravity and all ingredients needed to build the Standard Model, and it seems to be consistent.

It would be a surprise, and a pity, if nature did not make use of string theory. I have benefited during almost four decades from the richness it had to offer, but I was successful where I least expected it and not where I most desired it: in particle physics. But I hope that I, and you, my friends present here today, will get old enough so that we can have another meeting where I *can* have the last laugh.