Acceptance speech - 1993 NKT Research Prize in Physics
Dansk Fysisk Selskab Årsmøde, Lalandia, Rødby, 18 maj 1993

I am very moved, very touched that the Danish Physical Society has chosen me as the recipient of the 1993 NKT Research Prize in Physics. I personally do not deserve such honor - I interpret it as Society's wish to recognize and encourage research in some of the new directions in physics, described equally imprecisely as "physics of complex systems", "nonlinear science", or "chaos". Singling out these topics is by no means an easy choice, as Danish physicists are contributing to front-line research in many areas no less exciting than this one. I thank the Society on behalf of my colleagues engaged in nonlinear science research at the Copenhagen University, Nordita, DTH, and elsewhere in Denmark, in hope that we will be worthy of your trust.

The research that is being honored by this prize has disquietingly vague designations, such as "nonlinear phenomena" or "complex systems", and the only fact that journalists seem to extract from all our attempts to communicate the essence of the subject is the claim that the weather is unpredictable. In recent years we have come under real-life pressure to present ourselves as a well defined group with a clearly spelled out mission, and roughly every two months we produce still another report or grant application, explaining the unparalleled importance of nonlinear research, with a five year plan for problems that we promise to solve, and so on.

As I want to help create a good environment for our young researchers, I too have been forced to ponder about ways of making wise use of the limited resources allocated to basic research. It is very natural that one attempts to do it by soliciting project proposals, funding centers with sharply defined goals. I can see why this is the way to fund - let us say - DNA sequencing or a geological survey. A plan is certainly necessary if one is building a component for an accelerator expected to become operational a decade hence. Even in my own research I can in retrospect discern a long term plan. For example, the periodic orbit theory that I talked about at this meeting yesterday has been on my mind since 1978. However, you will not find it in any NBI annual report until 1988, because there was nothing I could say; we simply did not know how to implement the idea. Today we do, but it would be a bad idea to plan to concentrate on it until 1998.

I think that by putting emphasis on planning, and on the short term impact (number of citations, number of invited conference talks) we are often missing the point - forgetting why people do science, and what is it that they do. I realize that different people have different motivations, and different aspects of research require different strategies, and that we might differ even on the definition of what our goals as physicists might be. So what I will say perhaps applies only to a small fraction of us - and all I will argue is that for a fraction of our resources we should throw caution to the winds, and gamble on supporting a few people with unclear agenda, but strong fascination with the phenomena of the physical world.

Physics, in the sense of "natural philosophy", is distinct from other more difficult human enterprises - such as engineering or medicine - in the sense that a physicist owns a "method", but does not own a "subject". While we are acutely aware that nature offers us problems much more difficult and much more fascinating than, let's say, unraveling of sub-nuclear structure, we work by selecting among the interesting problems those that we have a hope of solving with our methodology. If successful, we do not even do that - we actually discover entire new universes of problems that nobody had dreamed of before. Sometimes in the process we acquire the ability to do hitherto inconceivable things, and perhaps even change the very way we think. It is for this few magic moments that society funds us, and it is this that we should do. I believe that the society is better served by letting physicists take risks, go through fallow periods, follow their intuition to problems where they feel that they might strike gold, rather than by rewarding them for keeping up a steady stream of high quality workmanship within the narrow field of their expertise. And we should somehow make it clear to our students that they are NOT expected to turn the subject of their PhD thesis into a lifelong occupation, that this is only an exercise that qualifies them to enter the guild, and that they should henceforth pursue any problem that they believe is important. But we are making this almost impossible by fragmenting our academic structures into infinity of small sharp purpose centers. For example, can the new Grundforskningsfond's Astrophysics Center support an astronomer whose interest has shifted to understanding the principles underlying the operation of visual cortex? By its contract it cannot, and most likely it will not. But at the same time, we all know that we cannot make sense of data on billions of stars, or billions of particles in accelerator collisions unless we think in radically new ways.

I am aware that such dreamy proposals are controversial - what guarantee do we have that such people will actually accomplish anything? Aren't our universities already filled with dead wood? I want to contribute to this debate by reexamining that particular research career that I know best, my own. What I want to say is that, looking back, almost everything that I have done as work that I was funded to do, including large part of the work listed in the prize citation, will probably be of no lasting interest - while the things that I did on the side, for my own pleasure, have in the long run turned out to be the only insights of lasting value.

I'll describe three such sidetracks that for me made physics worth doing: the first two, having to do with finiteness of gauge theories and exceptional Lie algebras are so far only of value to me, while the third one, the period doubling universality, turned out totally unexpectedly to be of interest to many people.

I started my physics career as a condensed matter experimentalist at MIT, and as such I was brought to Cornell as a Xerox fellow. I went once down into the bowels of Clark Hell, where there was a professor with an army of people slaving away in dark cubicles, and I promptly decided to join instead the field theorists who owned a beautiful rooftop view of the Ithaca hills and Ithaca skies[1]. One fateful day Toichiro Kinoshita came up with a Feynman integral and asked me whether I could evaluate it for him. No sweat, I worked

[1] Winter 1996: Doug Osheroff who arrived a year earlier, stayed on.

for a while and not only did I integrate it, but gave a formula for all Feynman integrals of that topology. It was only a bait. He came up with the next integral on which my general method miserably failed. Then he came with the next integral, and then it was like Vietnam - there was no way of getting out of it. I was spending nights developing algebraic languages disguised as editor macros so that synchrotron experimentalists would let me use their computer; we were flying in small planes to Brookhaven, carrying suitcases of computer punch-cards; and by four years later we had completed what at that time was the most complicated and the most expensive calculation ever carried out on a computer, and the answer was:

$$\frac{1}{2}(g-2) = \frac{1}{2}\frac{\alpha}{\pi} - 0.32848\left(\frac{\alpha}{\pi}\right)^2 + (1.183 \pm 0.011)\left(\frac{\alpha}{\pi}\right)^3 .$$

At the very end, I dreamed that I was a digit towards the end of the long string of digits that we had calculated for the electron magnetic moment, and that I died by being dropped as an insignificant digit. I was ready to move on.

Among my friends at Cornell were two called Feigenbaum. The first one moved to a factory town to do union organizing, and reached brief national fame when the Mafia bombed his house. The other one was amazingly fast in solving New York Times crossword puzzles, but he published nothing. Hans Bethe dispatched him to Blackhole, Virginia, where he languished publishing nothing until Peter Carruthers rescued him and took him to Los Alamos on the risky presumption that the man seemed very smart. In contrast to these good-for-nothings, I was advertised as the best thing since Roman Jackiw and sent off to Stanford, Princeton and Oxford with a mission to solve the QCD quark confinement problem.

So, what did I do? I found myself in California, my reading of Nietzsche came to an abrupt halt, to be replaced by volleyball, bicycling and scortatory love. I wrote dutifully a series of papers allegedly curing the infrared ills of QCD, and - well, we never did solve the quark confinement problem, not to this day, not in my book, at least.

But one day, terror struck; I was invited to Caltech to give a talk. I could go to any other place and say that Kinoshita and I have computed thousands of diagrams and that the answer is, well, the answer is:

$$+(0.92 \pm 0.02)\left(\frac{\alpha}{\pi}\right)^3 .$$

But in front of Feynman? He is going to ask me why "+" and not "-"? Why do 100 diagrams yield a number of order of unity, and not 10 or 100 or any other number? It might be the most precise agreement between fundamental theory and experiment in all of physics - but what does it mean?

Now, you probably do not know how stupid the quantum field theory is in practice. What is done (or at least was done, before the field theorists left this planet for pastures beyond the Planck length) is:

1) start with something eminently sensible (electron magnetic moment; positronium)

2) expand this into combinatorially many Feynman diagrams, each an integral in many dimensions with integrand with thousands of terms, each integral UV divergent, IR divergent, and meaningless, as its value depends on the choice of gauge

3) integrate by Monte Carlo methods in 10-20 dimensions this integral with dreadfully oscillatory integrand, and with no hint of what the answer should be; in our case ± 10 to ± 100 was a typical range

4) add up hundreds of such apparently random contributions and get

$$+(0.92 \pm 0.02) \left(\frac{\alpha}{\pi}\right)^3 .$$

So, in fear of God I went into deep trance and after a month came up with this: if gauge invariance of QED guarantees that all UV and IR divergences cancel, why not also the finite parts?

And indeed; when the diagrams that we had computed were grouped into gauge invariant subsets, a rather surprising thing happens; while the finite part of each Feynman diagram is of order of 10 to 100, every subset adds up to approximately

$$\pm \frac{1}{2} \left(\frac{\alpha}{\pi}\right)^n .$$

If you take this numerical observation seriously, the "zeroth" order approximation to the electron magnetic moment is given by

$$\frac{1}{2}(g-2) = \frac{1}{2}\frac{\alpha}{\pi}\frac{1}{\left(1-\left(\frac{\alpha}{\pi}\right)^2\right)^2} + \text{“corrections”}.$$

Now, this is a great heresy - my colleagues will tell you that Dyson has shown that the perturbation expansion is an asymptotic series, in the sense that the n-th order contribution should be exploding combinatorially

$$\frac{1}{2}(g-2) \approx \cdots + n^n \left(\frac{\alpha}{\pi}\right)^n + \cdots ,$$

and not growing slowly like my estimate

$$\frac{1}{2}(g-2) \approx \cdots + n \left(\frac{\alpha}{\pi}\right)^n + \cdots .$$

But do not take them too seriously - they trust authority, and very few of them have ever computed a measurable number. For me, the above is the most intriguing hint that something deeper than what we know underlies quantum field theory, and the most suggestive lesson of our calculation.

I prepared the talk for Feynman, but was fated to arrive from SLAC to Caltech precisely five days after the discovery of the J/ψ particle. I had to give an impromptu irrelevant talk about what would the total e^+e^- cross-section had looked like if J/ψ were a heavy vector boson, and had only 5 minutes for my conjecture about the finiteness of gauge theories. Gell-Mann was on the phone to SLAC throughout, which was just as well, but Feynman liked it and gave me some sage advice.

Los Angeles is no place for a car hater, so I eloped for Institute for Advanced Study, Princeton. The Institute is a quiet pretty place at the edge of the woods, where most people go crazy. The mathematician who lived before me in my apartment was taken away by men in white coats because he never spoke, he only grunted. It turned out that the apartment was furnished by a large number of dictionaries, and nothing else. But I loved being there. During the day I was solving the quark confinement problem (Stephen Adler got me into some cockeyed quaternionic calculation), but the nights were mine. I still remember the bird song, the pink of the breaking dawn, and me ecstatically pursuing the next tangent:

QCD quarks are supposed to come in three colors. This requires evaluation of SU(3) group theoretic factors, something anyone can do. However, in the spirit of stubborn Teutonic completeness, I wanted to check all possible cases; what would happen if the nucleon consisted of 4 quarks, and so on....

slide: [some 2-quark, 3-quark singlets]

Totally unexpectedly, in no time all exceptional Lie groups arose, not as some kind of sick Cartan lattices, but on the same geometrical footing as the classical invariance groups of quadratic norms, $SO(n)$, $SU(n)$ and $Sp(n)$.

If I were to give myself a prize - as I told you, I do not feel I deserve the NKT prize, but if it were something more commensurate with my contributions to science, let us say a week vacation in Lalandia, I would have given myself a prize for the magic triangle given in the table 1, where all exceptional Lie groups emerge as one big family. I like this, because it is one of those magic things that one discovers for no apparent reason whatsoever. Now, I am a probably a fool that even though I have put more effort in this project than any other, I have never completed the final write up. This is due to sociological factors; first, nobody wants to hear about it, and second, I got sidetracked by the next equally frivolous side diversion.

In spring 1976 Mitchell Feigenbaum came to visit from Los Alamos, having published even less than before. He gave a seminar, but nobody understood a word. Starting point was a parabola, then things got incredibly complicated, and at the end it turned out that the theory might be applicable to fluctuations in forest moth populations. However, Mitchell and I were driven by a secret agenda - the thing was robust, you could make it very imperfect, and a universal superstructure would survive the imperfections:

slide [period-doubling tree]

In other words, just what you need to build a brain - all parts imperfect, and the thing functions anyway. But my first task was to help my friend, make his lecture compre-

						0 0	3 $\mathbf{A}_1$ 2
					0 0	1 $\mathbf{U}(1)$ 1	8 $\mathbf{A}_2$ 3
				0 0	0 1	3 $\mathbf{A}_1$ 3	14 $\mathbf{G}_2$ 7
			0 0	0 1	2 $2\mathbf{U}(1)$ 2	9 $\mathbf{3A}_1$ 4	28 $\mathbf{D}_4$ 8
		0 0	0 2	3 $\mathbf{A}_1$ 5	8 $\mathbf{A}_2$ 8	21 $\mathbf{C}_3$ 14	52 $\mathbf{F}_4$ 26
	0 0	0 1	2 $2\mathbf{U}(1)$ 3	8 $\mathbf{A}_2$ 6	16 $\mathbf{2A}_2$ 9	35 $\mathbf{A}_5$ 15	78 $\mathbf{E}_6$ 27
0 0	1 $\mathbf{U}(1)$ 2	3 $\mathbf{A}_1$ 4	9 $\mathbf{3A}_1$ 8	21 $\mathbf{C}_3$ 14	35 $\mathbf{A}_5$ 20	66 $\mathbf{D}_6$ 32	133 $\mathbf{E}_7$ 56
3 $\mathbf{A}_1$ 3	8 $\mathbf{A}_2$ 8	14 $\mathbf{G}_2$ 14	28 $\mathbf{D}_4$ 28	52 $\mathbf{F}_4$ 52	78 $\mathbf{E}_6$ 78	133 $\mathbf{E}_7$ 133	248 $\mathbf{E}_8$ 248

Table 1: Magic triangle. All exceptional Lie groups defining and adjoint representations form an array of the solutions a Diophantine condition. Within each entry the number in the upper left corner is N, the dimension of the corresponding Lie algebra, and the number in the lower left corner is n, the dimension of the defining representation.

hensible. My friend Betty Boop worked on a Hewlett-Packard assembly line, so I was able to acquire a programable pocket calculator for a mere one-fourth of my monthly salary. Mitchell needed no such display of dedication, Los Alamos was floating in money. I calculated away with such gusto that I was calculating even laying on my belly on an operating table, with a surgeon lancing a large bicycle-caused sore. Eventually I reduced the whole complicated mess[2] to one equation;

$$g(x) = \alpha g(g(x/\alpha))\,,$$

and went off to the math library to look it up. The Institute has an excellent math library, but I did not find it. As a matter of fact, we never found it to this very day - it had never been written down before.

As you would expect, nobody wanted to hear about it. To be fair, I remember that a total of four people did: Freeman Dyson, John Milnor, Bill Thurston, and Donna Lee. Donna Lee is a wonderful Chinese San Franciscan whom I love even more dearly than Dyson; the rest you should know.

Six years passed and I was laboring away at Nordita, when the word arrived from USA; there is chaos, and in 1982 I could muster an audience for my first talk about the period doubling universality. By that time I was already deep in trouble - once I learned that chaos is generic for generic Hamiltonian flows, I lost faith in doing field theory by pretending that it is a bunch of harmonic oscillators, with interactions accounted for as perturbative corrections. This picture is simply wrong - strongly coupled field theories (hydrodynamics, QCD, gravity) are nothing like that. So they kicked me out of the high energy theory group, and now I am in charge of a project. We are working out quantum chaos. The truth is, this is what I set out to do in 1978 - replace the path integral with a fractal set of semi-classical orbits, so I feel that I am closer to figuring out the quark confinement than ever before. Which is not to say that we are close. To keep myself sane, I have a new private sidetrack - I want to use classical chaos to altogether get rid of the Feynman path integrals and the idiotic Feynman diagrams. You say: nonsense! but a man has a right to hope...

What I have learned from these personal meanderings is that at least for some physicists pursuing a specific project might be counterproductive. In my case, most things of lasting value came from doing things that I was not supposed to do. For a post-doc this was plain foolhardy use of time; later it was made possible by the protective shell provided by Nordita. Now, as a consequence of the unanticipated success of one of these private pleasures, I work as a drone, trying to manage chaos research at NBI. But here is what I dream of: we should dedicate some significant fraction of our funding to a high risk undertaking: picking out a few men and women that really love doing science, and letting them do whatever their nose tells them to do. For this we do not need centers, we need a good broad Institute, and when I say “a few men and women”, I really think the best we can find. Think of the names of people who have done good work in Copenhagen; Bohr,

[2] For the record: universality in period doubling and the renormalization theory that explains it were Mitchell’s discovery, my contribution was to recast what was an infinite tower of renormalization equations into a more concise form.

Heisenberg, Hevesy, Klein, Landau, Møller, Gamow, Franck, Dirac, Rosenfeld, Kramers, Nishina, Casimir, ... We are a small country, physics is a common human enterprise, and if we keep up this very special tradition, we can keep physics here strong and exciting. This, and nothing else, will attract the best students to physics.

I would like to end on a personal note. I live here, and today I will vote; but I was born in Croatia, and it was that country and its culture that shaped me, and made it possible for me to go into a larger world. People of Croatia are a melancholy central European people, with a long history of armies marching through pillaging and raping; today the country is ravaged by Serbia's troops.

I remember the day Serbian soldiers overrun Vukovar. It was a beautiful sunny morning, I picked up the paper in the NBI expedition, looked at the pictures of the devastated city, sat down and cried quietly for a long time, tears dropping on the newspaper in my lap. My colleagues buzzed cheerfully in and out. Not so far away, at that very moment, the Vukovar wounded were taken from the hospital, driven to a field, executed, and piled up in mass graves.

I describe this morning not because I expect you to experience this with the same intensity as I do, but so that you understand why I have decided (with full moral support of my family and of Søren Isakson of the NKT) to contribute the prize to rebuilding of the educational structure of Croatia[3]. These are critical times of transition for all post-communist eastern Europe, and exceptionally difficult times for the war ravaged Croatia and Bosnia and Herzegovina; at a time like this an amount of money modest by Danish standards can do a lot of good. I hope we can help these countries through the transition, and help them help themselves.

Again, I thank the Danish Physical Society and NKT for this honor.

Thank you very much.

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Niels Bohr Institute

[3] July 1997: My colleagues in Croatia have advised me to help the library of Osjek University. Embarassingly enough, I still have not finished my work on this project.