Epilogue

"I read your book. It is long and it seems interesting, but - why? Why did you do this?" you might well ask. OK, here is an answer.

Looking back, almost everything I have done as work which I felt I should do 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 insights worth living for. One such sidetrack has to do with a conjecture of £niteness of gauge theories, which, by its own twisted logic, led to this sidetrack, birdtracks and exceptional Lie algebras.

One fateful day, when I was a graduate student at Cornell, Toichiro Kinoshita came up with a Feynman integral and asked me whether I could evaluate it for him. No sweat, I worked for a while and not only did I integrate it, but also I 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 failed miserably. Then he came with the next integral, and 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 ¤ying 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 [1]:

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

At the very end, I dreamed that I was a digit toward 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 insigni£cant digit. I was ready to move on.

I moved to California and wrote a series of papers allegedly curing the infrared ills of QCD. 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:

$$+(1.183 \pm 0.011)\left(\frac{\alpha}{\pi}\right)^3.$$

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 a fundamental theory and experiment in all of physics - but what does it mean?

Now, you probably do not know how stupid the quantum £eld theory is in practice. What is done 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

$$+(1.183 \pm 0.011)\left(\frac{\alpha}{\pi}\right)^3.$$

So, for the fear of Feynman 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 £nite parts?

And indeed; when the diagrams that we had computed are grouped into gauge invariant subsets, a rather surprising thing happens [4]; while the £nite 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 nth 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

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

The asymptotic series argument is essentially a diagram-counting argument, with no gauge-invariance and mass-shell conditions accounted for. For me, these numerically observed unreasonably effective cancellations remain a tantalizing hint that something deep, deeper than anything what we know today, lurks in the gauge invariance of quantum £eld theories.

I should not have bothered. I was fated to arrive from SLAC to Caltech precisely £ve days after the discovery of the J/ψ particle. I had to give an impromptu 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 £niteness of gauge theories.

Feynman liked it and gave me sage advice. I kept looking for a simpler gauge theory in which I could compute many orders in perturbation theory and check the conjecture. We learned how to count Feynman diagrams [5]. I formulated a planar £eld theory [6] whose perturbation expansion is convergent, but did not know how to combine this with gauge invariance. I formulated the theory of the group weights of Feynman diagrams in non-Abelian gauge theories [2, 9] but did not £nd a relative of local gauge invariance there, either. By marrying Poincaré to Feynman we found a new perturbative expansion [8] which appears more compact than the standard Feynman diagram perturbation theory. No dice. To this day I still do not know how to prove or disprove the conjecture.

I moved to Princeton. The Institute for Advanced Study is a quiet place at the edge of the woods and I loved being there. During the day I was solving the quark con£nement problem, 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. In the spirit of Teutonic completeness, I wanted to check all possible cases; what would happen if the nucleon consisted of 4 quarks, doodling

$$\text{🐰} - \text{🐨} = n(n^2 - 1) \ ,$$

and so on, and so forth? In no time, and totally unexpectedly, all exceptional Lie groups arose, not as Diophantine conditions on Cartan lattices, but on the same geometrical footing as the classical invariance groups of quadratic norms, $SO(n)$, $SU(n)$ and $Sp(n)$.

"OK, OK - everybody has a story," you say "But on you claim to have derived your Magic Triangle [2, 3, 7] already in late 1970's. What took you so long?"

Since 1970's diagrammatic methods have become the notation of choice for a select group of group theory practitioners. I like the Magic Triangle, table , where all exceptional Lie groups emerge in one big family, because it is one of those magic things that one discovers for no apparent reason whatsoever. However, nobody, but truly nobody showed a glimmer of interest in the exceptional Lie algebra parts of this work, so there was no pressure to publish it before completing it: by completing it I mean £nding the algorithms that would reduce any bubble diagram to a number for any semi-simple Lie algebra. This monograph accomplishes the task for G_2, but for F_4, E_6, E_7 and E_8 this is still an open problem. This, perhaps, is only matter of algebra (all computations in this monograph were done by hand, mostly on trains and in airports), but the truly frustrating unanswered question is:

Where does the Magic Triangle come from? Why is it symmetric across the diagonal? The Freudenthal-Tits construction of the Magic Square in terms of octonionic matrices is the best answer so far, but it is not a natural answer from the invariance groups perspective. Something is happening here, but I don't know what it is. Most likely the starting idea - to classify all simple Lie groups from the primitivness assumption - is ¤awed. Is there a mother of all Lie algebras, some complex function which yields the Magic Triangle for a set of integer values? This, it seems, requires a better idea.

And then there is a practical issue of unorthodox notation: transferring birdtracks from hand drawings to LaTeX took another 21 years. In this I was rescued by Henri-

Table 1: *Magic triangle.* All exceptional Lie groups de£ning and adjoint reps form an array of the solutions of the Diophantine conditions derived in the book [9]. 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 de£ning rep.

ette Elvang who mastered the art of birdtracking on her own, in her Master's Thesis, and introduced me to Anders Johansen, Copenhagen University undergraduate, who then undertook drawing some 4,000 birdtracks needed to complete this manuscript, of elegance far outstripping that of the old masters.

Predrag Cvitanović
June 29, 2003
Čijovo, Trogir, Croatia

References

[1] P. Cvitanović and T. Kinoshita, "Sixth order magnetic moment of the electron," *Phys. Rev.* **D10**, 4007 (1974).

[2] P. Cvitanović, "Group theory for Feynman diagrams in non-Abelian gauge theories," *Phys. Rev.* **D14**, 1536 (1976).

[3] P. Cvitanović, "Classical and exceptional Lie algebras as invariance algebras," Oxford preprint 40/77 (June, 1977, unpublished); available on www.nbi.dk/GroupTheory/refs.

[4] P. Cvitanović, "Asymptotic estimates and gauge invariance," *Nucl. Phys.* **B127**, 176 (1977).

[5] P. Cvitanović, B. Lautrup and R.B. Pearson, "The number and weights of Feynman diagrams," *Phys. Rev.* **D18**, 1939 (1978).

[6] P. Cvitanović, "Planar perturbation expansion," *Phys. Lett.* **99B**, 49 (1981).

[7] P. Cvitanović, "Negative dimensions and E_7 symmetry," *Nucl. Phys.* **B188**, 373 (1981).

[8] P. Cvitanović, C.P. Dettmann, R. Mainieri and G. Vattay, "Trace formulas for stochastic evolution operators: Smooth conjugation method," *Nonlinearity* **12**, 939 (1999); chao-dyn/9811003.

[9] P. Cvitanović, *Group Theory* (Princeton Univ. Press, Princeton NJ 2003); www.nbi.dk/GroupTheory .