Some reminiscences from a long friendship

PTEP

Prog. Theor. Exp. Phys. **2016**, 07B106 (8 pages)
DOI: 10.1093/ptep/ptw044

Nambu, A Foreteller of Modern Physics II

Some reminiscences from a long friendship

Lars Brink

Chalmers University of Technology, Dept of Fundamental Physics, S-41296 Göteborg, Sweden
*E-mail: lars.brink@chalmers.se

Received March 15, 2016; Accepted March 25, 2016; Published June 3, 2016

Subject Index B00, B29, B30, B46

1. My first encounters with Yoichiro Nambu

When I was in my first year of graduate studies, our department organized the first Nobel Symposium in physics in the spring of 1968. Essentially all the leading physicists in particle physics were there, and for a young student it was a great time to see and listen to all those famous scientists. The meeting later became very famous for Abdus Salam's contribution [1], which led to his Nobel Prize.

One of the speakers was Yoichiro Nambu. He talked about infinite-component field theories [2], a subject that is now solidly forgotten. I was in a small room in the back recording the sessions but could follow the talks, even though I did not understand much. I could see, however, the respect that the other speakers showed to him.

In the fall of 1969 I visited my advisor who was on sabbatical in Austin for a few months. Nambu came there to give a talk and he then lectured about his factorization of the Veneziano Model amplitudes. This was certainly a subject I was beginning to be drawn to.

In the summer of 1970 I went to a summer school in Copenhagen, mainly because Nambu was going to talk about dual models as it was announced. This was now the subject I had been starting to work on, not telling my advisor. Nambu did not make it there. Much later he told me that his car had broken down on his way to Chicago to go to Copenhagen. However, his preprint [3] for the meeting had arrived and it was distributed among the participants. This is the preprint in which he suggested the action for the bosonic string which came to be called the Nambu–Gotō action. In the preprint he dared to be braver than in an ordinary paper and there were lots of interesting ideas. He was even comparing the string to DNA at the end of the preprint. It eventually became a rarity and I used to keep it in a box in the attic of our department until someone cleaned out all my boxes of old preprints. The Nambu paper was the only one I really missed.

2. Zero-point fluctuations of strings

I spent the academic years 1971–73 as a fellow at CERN. I was gradually more and more drawn into the field of "Dual Models." In the final year I worked with David Olive on a huge program to compute the correct one-loop graphs, which we did by using Feynman's tree theorem. This amounts to sewing tree diagrams together, inserting a projection operator onto the physical states in one of the propagators. Feynman had mentioned this technique at a question session at a conference in Poland in 1963 [4], and we reconstructed it [5] showing that it indeed gives the correct perturbative unitarity and that there were no problems with conflicting $i\epsilon$ prescriptions, etc.

Having constructed the physical state projection operators in all sectors of the open and closed strings we could not only construct the one-loop diagrams [6] but could also check the gauge properties of the open–closed string (reggeon–pomeron) vertex [7], as well as the fermion emission vertex [8], showing that the Ramond and the Neveu–Schwarz models coupled unitarily to each other, and we wrote a long series of papers that year.

In the summer of 1974 John Schwarz organized a workshop in Aspen in Dual Models and at the same time Murray Gell-Mann organized a workshop in gauge field theories. A number of very famous people were around, and on one of my first days Bunji Sakita came up to me and said that Prof. Nambu wants to meet with you. I went in to his office and introduced myself. I thought he wanted to discuss my work with David Olive, but no, he wanted to congratulate me on my paper [9] with Holger Bech-Nielsen on zero-point fluctuations in string theory. Nambu had himself [10] been the first to see that the states in dual models resembled an infinite set of harmonic oscillators. In the bosonic model the mass spectrum was given by the formula

$$\alpha' m^2 = \sum_{n=1}^{\infty} n a_n^{\dagger} \cdot a_n. \tag{1}$$

If we do it in the light-cone gauge only, the transverse oscillators remain and you sum over $d-2$ components. Every oscillator will contribute a zero-point fluctuation of $1/2(\hbar\omega)$. Formally, the mass of the lowest-lying state would then be

$$\alpha' m_0^2 = \frac{d-2}{2} \sum_{n=1}^{\infty} n. \tag{2}$$

This is clearly a divergent result and must be regularized. We did it by regularizing and renormalizing the velocity of light, which is the speed of the phonons along the string. A simpler way to do it is to use ζ-function regularization. (This was later pointed out to us by Nando Gliozzi.)

$$\alpha' m_0^2 = \frac{d-2}{2} \sum_{n=1}^{\infty} n^{\zeta} \bigg|_{\zeta=1} = -\frac{d-2}{24}. \tag{3}$$

By knowing that the first oscillation is a massless vector particle we could draw the conclusion that $d = 26$.

The fact that the sum of all integers can be regularized to become $-\frac{1}{12}$ was a well-known result in mathematics, perhaps going all the way back to Jacobi (and his assistant J. Scherk.)

This was a paper that took us a few days to write, but which we were quite proud of and later had a lot of use for. Anyhow, this was the paper that Nambu congratulated me for. I think it shows his good taste in physics, if I may say so.

3. The σ-model action or how to supersymmetrize the Nambu–Gotō action

In his Copenhagen lecture [3] Nambu described how the spectrum of the states in the Veneziano model could be described by a free bosonic string with a subsidiary condition. Consider the action for a free string

$$I = \frac{1}{4\pi} \int d^2\xi \left(\frac{\partial x_\mu}{\partial \tau} \frac{\partial x^\mu}{\partial \tau} - \frac{\partial x_\mu}{\partial \sigma} \frac{\partial x^\mu}{\partial \sigma} \right), \tag{4}$$

where $\xi^0 = \tau$ and $\xi^1 = \sigma$. This action is conformally invariant. By demanding that the Noether current generating these conformal transformations be zero we can generate the Virasoro conditions, which eventually was proven to lead to a unitary spectrum.

The action (4) is not a geometric action, so Nambu said let us, for curiosity, construct a geometric action. A natural candidate is then the surface area, the two-dimensional world-sheet. The sheet is embedded in Minkowski space as $x^\mu(\tau, \sigma)$, so the surface element is

$$\begin{aligned} d\sigma^{\mu\nu} &= G^{\mu\nu} d^2\xi, \\ G^{\mu\nu} &= \partial \left(x^\mu, x^\nu \right) / \partial(\tau, \sigma), \end{aligned} \tag{5}$$

whereas the line element is

$$\begin{aligned} ds^2 &= g_{\alpha\beta} d\xi^\alpha d\xi^\beta, \\ g_{\alpha\beta} &= \left(\frac{\partial x^\mu}{\partial \xi^\alpha} \frac{\partial x_\mu}{\partial \xi^\beta} \right). \end{aligned} \tag{6}$$

The geometric action is then

$$I = \int \left| d\sigma^{\mu\nu}\, d\sigma_{\mu\nu} \right|^{1/2} = \int d^2\xi \sqrt{\left(\dot{x}x'\right)^2 - \left(\dot{x}^2 x'^2\right)}. \tag{7}$$

Since this is reparametrization invariant on the world-sheet we can impose two conditions such that the equations are linearized and agree with the equations that follow from (4). The subsidiary conditions chosen for this purpose are

$$\frac{\partial x}{\partial \tau} \cdot \frac{\partial x}{\partial \sigma} = 0, \quad \left(\frac{\partial x}{\partial \tau} \right)^2 + \left(\frac{\partial x}{\partial \sigma} \right)^2 = 0, \tag{8}$$

which just amounts to the Virasoro conditions.

In 1974 Paolo Di Vecchio had arrived at Nordita in Copenhagen as an assistant professor, and we started to collaborate. One problem we soon started to discuss was how to supersymmetrize the Nambu–Gotō action. At this time supersymmetric field theories were a hot subject and we wanted to use the knowledge from the four-dimensional theories for the supersymmetric string, as we called it. Already in 1971, some six months after Pierre Ramond's groundbreaking paper [11], Gervais and Sakita [12] had constructed a free supersymmetric two-dimensional action. We soon realized that by demanding that the Noether current from the superconformal symmetry be zero, we could generate the super-Viraosoro conditions. But why should they be zero? In this process we discovered with altogether a full football team of Italians that we could generalize this procedure to infinitely many super-Virasoro algebras [13] out of which two could be represented as extensions of the original ones. In this way we found two new supersymmetric string models [14,15], but none of them turned out to be close to the one we were searching for, a four-dimensional model with a hadronic spectrum. We were still looking for a model for the strong interactions.

Could one add in fermionic degrees of freedom to the Nambu–Gotō action and get the supersymmetric string? One natural attempt was to introduce a supercoordinate instead of the usual coordinate x^μ. That worked at the linearized level. However, we soon found out that to have Grassmann numbers under the square-root sign is not a good idea. Every attempt failed and we were stuck on the problem.

In the beginning of 1976 the first papers on supergravity appeared [16,17]. In principle, we should have understood immediately how to find a reparametrization invariant string action with Grassmann coordinates, but we were slow to understand it. The formalism with vierbein fields was unknown to us and we were doing other things. Finally, in the summer Paolo Di Vecchia, Paul Howe, and I got together at CERN for a few days and constructed the corresponding action for a supersymmetric particle, an action that leads to the Dirac equation. We wrote it up with Stanley Deser and Bruno Zumino [18]. I was about to move to Caltech for a year but we found some time in September and during some very hectic days we finally realized how to generalize the Nambu–Gotō action [19]. By introducing a two-dimensional gravity field we could simply rewrite (7) as

$$I = -\frac{1}{2}\int \sqrt{-g}\, g^{\alpha\beta}\partial_\alpha x_\mu \partial_\beta x^\mu, \tag{9}$$

where g is the determinant of $g_{\alpha\beta}$.

The extension to a supersymmetric string is then straightforward. One has to extend the two-dimensional reparametrizations to a local supersymmetry and to introduce zweibein fields ${e_\mu}^\alpha$ and their superpartners ${\chi_\mu}^a$ to couple the fermion coordinates ${\lambda_\mu}^a$ in an invariant way. The final action for the superstring is then

$$\begin{aligned} I = \int e \Big[& -\frac{1}{2} g^{\alpha\beta}\,\partial_\alpha x_\mu \partial_\beta x^\mu - \frac{i}{2}\bar{\lambda}_\mu \gamma^\alpha \partial_\alpha \lambda^\mu \\ & + \frac{i}{2}\bar{\chi}_\alpha \gamma^\beta \gamma^\alpha \lambda_\mu \partial_\beta x^\mu + \frac{1}{8}\left(\bar{\chi}_\alpha \gamma^\beta \gamma^\alpha \lambda_\mu\right)\left(\bar{\chi}_\beta \lambda_\mu\right)\Big], \end{aligned} \tag{10}$$

where e is the determinant of the zweibein.

By choosing gauge fixings properly one can linearize the equations of motion and obtain the super-Virasoro constraints. Finally, we had got the action we had looked for for such a long time. It was known, of course, for a long time that the critical dimension for this theory is ten. The easiest way to find it is to consider the mass-square operator which follows from the constraints and commute the zero-point fluctuations.

4. Nambu and the Nobel Prize

In 2001 I was elected as an adjoint member of the Nobel Committee for physics. The work in the committee is, of course, highly classified but anyone can conclude that Nambu became one of those that I had to consider. I was fortunate that the year before I got a copy from Valentine Telegdi of the book *Broken Symmetry, Selected Papers of Y. Nambu* edited by T. Eguchi and K. Nishijima [20]. The book contains all Nambu's important papers up to the 1970s, and it also contains some of the talks he had given over the years. As I will indicate later, some of these talks were decisive for me in understanding his work. The work in the committee is quite lonely. You cannot show an interest in a person outside a small circle. Over the years I had to restrain myself not to ask about Nambu's health too often.

Nambu told me once that he was very mystified when Bob Schrieffer came to give a seminar in Chicago in 1956 just before they were about to publish the famous BCS paper [21]. What happened to gauge invariance? The condensation of the Cooper pairs clearly leads to a new vacuum that does not conserve the charge. The BCS theory is essentially QED coupled to phonons and if the gauge invariance is broken, how can one trust any calculations? It took Nambu two and a half years to understand and solve the problem to his satisfaction [22]. In the meantime superconductivity was one of the hottest topics in physics, and since it has a field theoretic background both condensed matter and particle physicists worked on it. Anderson [23] introduced the collective excitations and Bogoliobov [24] introduced his quasi-states, but the results were attacked for example by Shafroth who firmly stated that the results cannot be trusted since the calculations are not gauge invariant.

However, the results in the BCS paper were clearly correct. Nambu solved the problem in a tour de force calculation which is one of the toughest to follow in detail. He started by also introducing a vacuum with a vacuum expectation value for the electron field $\langle\psi(x)\ \psi(y)\rangle$ and not only for $\langle\bar{\psi}(x)\ \psi(y)\rangle$, and then he used a Feynman–Dyson formulation to compute the higher quantum correction; in the end he found that gauge invariance is conserved, although in a non-linear fashion. The collective excitations come out as solutions to his vertex equations and all the calculations done before can now be shown to be trusted. Unfortunately, Shafroth was not around any longer to see the solution. He and his wife had crashed in a small postal plane in Australia a month before Nambu's paper was available.

That paper is certainly one of the most important of the last century. Not only did it explain superconductivity in a consistent way, it also laid the groundwork for a lot of the particle physics that would come after it. Nambu was not a scientist who wrote everything in front of the reader's nose; one has to dig into the paper. One might ask how much of the BEH effect [25–28] there is? Anderson has often praised Nambu and the superconductivity paper, but alway insisted that Nambu did not have the effect in the paper. It is true that the model setup is non-relativistic since it contains a Fermi surface, but it has particles and antiparticles and gauge invariance. At the end of the paper, when Nambu discusses the Meissner effect, he writes: "We see that the previous collective state [which corresponds to mass zero] has shifted its energy to the plasma energy as a result of the Coulomb interaction." This looks to me as very similar to Anderson's ideas some years later [29]. What is clear, though, is that Nambu had already got his next great idea, namely to use this technique on pion physics. I am sure that somewhere he knew that gauge fields could have a short range and still be gauge covariant, but he did not push it. As the gentleman he was, he never claimed any priority for it either.

After Feynman's and Gell-Mann's introduction of the conserved vector current hypothesis (CVC) [30], the question was if there was a similar relation for the axial vector current. Without a field theory for the strong interaction it was hard to envisage how such a current would look. However, it was a tempting idea. Within four days in February of 1960 Gell-Mann and Lévy [31] and Nambu [32] published two classic papers that changed the world. Gell-Mann and Lévy set up several models that had partially conserved axial vector current (PCAC). They also introduced the non-linear σ-model and what came to be called the Cabibbo angle, and computed it correctly. Nambu's paper was very short and to the point. He simply suggested a form for the current (in his notation and normalization):

$$g_A \Gamma_\mu{}^A(p', p) = g_V \left[i\gamma_5\gamma_\mu\, F_1(q^2) - \frac{2M\gamma_5 q_\mu}{q^2 + m_\pi{}^2}\, F_2(q^2) \right] \tag{11}$$

$$F_1(0) = g_A/g_V = F_2(0)$$

$$F_1(q^2) \sim F_2(q^2) \quad \text{for } q^2 \gg m_\pi{}^2,$$

where M is the nucleon mass. The form factors $F_{1,2}$ carry the part of the interaction that is unknown. When the pion mass is zero the current is conserved. In real life it is partially conserved. The q_μ in the second term shows that the nucleon–nucleon–pion interaction has a derivative coupling, very different from the Yukawa coupling. It also implies that the neutron emits a massless pion which propagates and then decays to $e + \nu$ in the neutron β decay.

Table 1. Comparison of superconductivity and pion physics.

Superconductivity	Pion physics
free electrons	bare fermions (zero or small mass)
phonon interaction	some unknown interaction
energy gap	observed mass of nucleon
collective excitation	meson, bound nucleon pair
charge	chirality
gauge invariance	γ_5-invariance (rigorous or approximate)

From this formula Nambu derives the "Goldberger–Treiman relation" (again in his notations)

$$2Mg_A = 2Mg_V F_2\left(-m_\pi{}^2\right) = \sqrt{2}G_\pi g_\pi, \tag{12}$$

where g_π is the pion decay constant and G_π is the pseudoscalar coupling constant. This relation, which fitted surprisingly well the data, had been derived with some violent approximations by Goldberger and Treiman [33], and here it just came out of Nambu's assumption about PCAC.

But where did he get all this from? What more did he have in his hat? For someone reading the paper 50 years later it is incomprehensible. Fortunately, he also gave talks about it and at a meeting in Purdue [20] a little later he really expounded his ideas. This was very similar to what he did in his Copenhagen lectures in the sense that he was quite explicit. He once told me that in lecture notes he could be more detailed and also propose wilder ideas. Like at Copenhagen he did not deliver the talk at Purdue. It was delivered by Gianni Jona-Lasinio. These lecture notes disappeared, however, somehow, and it was impossible to read them until the book with the selected articles came out. For this volume it was re-typeset and it is now a gold mine of Nambu's ideas at that time.

From the start he shows how influenced he was by his understanding of superconductivity. He sets up a table (Table 1) that explains the correspondences between superconductivity and pion physics. Now it becomes clear how he thought. He thought of a vacuum with a condensation of fermion–antifermion pairs and with mesons built up by fermion–antifermion pairs where the mass of these fermions is a few MeV. This is four years before quarks but four years after Sakata's work [34], which was influential at the time. However, he had realized that the constituent particles should have a small mass. This is much more like the up and down quarks developed four years later. He then shows without knowing the details of the strong interactions how the spontaneously broken symmetry is restored in the limit of vanishing pion mass. This is again a non-linear realization of the symmetry. The new thing is that the spontaneous breaking of the symmetry forces a massless particle into the theory, in this case the pion—what was to come to be called the Nambu–Goldstone particle. This is different from the spontaneous breaking of the gauge invariance in the superconductivity case, a case I do not find that he comments on. In a stroke of genius he had explained how nuclear physics could work where the energy scales are of the order of GeV. With a spontaneous breaking of the chiral symmetry and with a small explicit breaking of this with a small quark mass, one can get a strong force of long enough range to bind nuclei together.

After these groundbreaking papers Nambu wrote a series of papers with Jona-Lasinio [35,36] and others [37] to extract useful information about pion physics even though the exact structure of the force was not known. This was very important at the time and became even more important when QCD came around.

Finally, in 2008 the Nobel Committee was ready to award the Nobel Prize to Nambu. The work started early in the year and I was nervous about his health. He was 87 years old. I met Bob Wald at a meeting in January and I asked him thoroughly about his colleagues— even though I was interested in their health it was mainly Nambu's health I was interested in. Finally, I asked about that and got a positive answer but there was still some nine months to go.

On October 7 we had the final vote in the Academy of Sciences and after that we went to our secret telephone and called Nambu. It was very early in Chicago but finally he answered. I am sure he was asleep and not worrying about telephone calls from Sweden. First, the permanent secretary of the Academy talked to him and told him the good news and then I got the receiver. Then I was so moved that I lost my voice, and I could only say "This is Lars. Congratulations!"

I had a long-standing dream that I should be giving the speech at the ceremony, and when I turned to Nambu I should be able to address him in Japanese. In the end he could not make it to the ceremony in Stockholm since his wife was not well enough. However, there were two more Japanese laureates, Makoto Kobayashi and Toshihide Maskawa, so I did prepare the last part of the talk, the address to the laureates, in Japanese. I had very good help from a local professor of Japanese. I did mention this to Murray Gell-Mann on the phone who immediately gave me a long course in Japanese pronunciation, which did not make me less nervous. In the end it went quite well and the only problem arose when I finished, since only the laureates and I knew that the speech was over. After a long minute the king rose and the ceremony could go on.

Since there was a time gap of seven hours between Stockholm and Chicago the video of my talk, which was given in Swedish except for the last part, could be transmitted and subtitles could be introduced, so at the ceremony in Chicago the audience could even understand the Swedish part of the talk.

One dream I had had for so long had finally come true.

References

[1] A. Salam, Conf. Proc. C **680519**, 367 (1968).
[2] Y. Nambu, in *Elementary Particle Theory, Proceedings of The Nobel Symposium held 1968 at Lerum, Sweden*, ed. N. Svartholm (Stockholm, 1968), p. 105.
[3] Y. Nambu, Duality and Hadrodynamics, in *Notes prepared for the Copenhagen High Energy Symposium*, Aug. 1970.
[4] R. P. Feynman, Acta Phys. Polon. **24**, 697 (1963).
[5] L. Brink and D. I. Olive, Nucl. Phys. B **56**, 253 (1973).
[6] L. Brink and D. I. Olive, Nucl. Phys. B **58**, 237 (1973).
[7] L. Brink, D. I. Olive, and J. Scherk, Nucl. Phys. B **61**, 173 (1973).
[8] L. Brink, D. I. Olive, C. Rebbi, and J. Scherk, Phys. Lett. B **45**, 379 (1973).
[9] L. Brink and H. B. Nielsen, Phys. Lett. B **45**, 332 (1973).
[10] Y. Nambu, Quark model and the factorization of the Veneziano amplitude, in *Proceedings of Conference on Symmetries* (Detroit, 1969).
[11] P. Ramond, Phys. Rev. D **3**, 2415 (1971).
[12] J. L. Gervais and B. Sakita, Nucl. Phys. B **34**, 632 (1971).
[13] M. Ademollo, L. Brink, A. D'Adda, R. D'Auria, E. Napolitano, S. Sciuto, E. Del Giudice, P. Di Vecchia, S. Ferrara, F. Gliozzi, R Musto, and R. Pettorino, Phys. Lett. B **62**, 105 (1976).
[14] M. Ademollo, L. Brink, A. D'Adda, R. D'Auria, E. Napolitano, S. Sciuto, E. Del Giudice, P. Di Vecchia, S. Ferrara, F. Gliozzi, R Musto, R. Pettorino, and J. H. Schwarz, Nucl. Phys. B **111**, 77 (1976).
[15] M. Ademollo, L. Brink, A. D'Adda, R. D'Auria, E. Napolitano, S. Sciuto, E. Del Giudice, P. Di Vecchia, S. Ferrara, F. Gliozzi, R Musto, and R. Pettorino, Nucl. Phys. B **114**, 297 (1976).
[16] D. Z. Freedman, P. van Nieuwenhuizen, and S. Ferrara, Phys. Rev. D **13**, 3214 (1976).
[17] S. Deser and B. Zumino, Phys. Lett. B **62**, 335 (1976).
[18] L. Brink, S. Deser, B. Zumino, P. Di Vecchia, and P. S. Howe, Phys. Lett. B **64**, 435 (1976).
[19] L. Brink, P. Di Vecchia, and P. S. Howe, Phys. Lett. B **65**, 471 (1976).

[20] T. Eguchi and K. Nishijima (eds.), *Broken Symmetry, Selected Papers of Y. Nambu* (World Scientific, Singapore, 1995).
[21] J. Bardeen, L. N. Cooper, and J. R. Schrieffer, Phys. Rev. **108**, 1175 (1957).
[22] Y. Nambu, Phys. Rev. **117**, 648 (1960).
[23] P. W. Anderson, Phys. Rev. **110**, 827 (1958).
[24] N. N. Bogoliubov, J. Exp. Theor. Phys. **34**, 58 (1958).
[25] F. Englert and R. Brout, Phys. Rev. Lett. **13**, 321 (1964).
[26] P. W. Higgs, Phys. Rev. Lett. **13**, 508 (1964).
[27] A. A. Migdal and A. M. Polyakov, Sov. Phys. J. Exp. Theor. Phys. **24**, 91 (1967) [Zh. Eksp. Teor. Fiz. **51**, 135 (1966)].
[28] G. S. Guralnik, C. R. Hagen, and T. W. B. Kibble, Phys. Rev. Lett. **13**, 585 (1964).
[29] P. W. Anderson, Phys. Rev. **130**, 439 (1963).
[30] R. P. Feynman and M. Gell-Mann, Phys. Rev. **109**, 193 (1958).
[31] M. Gell-Mann and M. Levy, Nuovo Cim. **16**, 705 (1960).
[32] Y. Nambu, Phys. Rev. Lett. **4**, 380 (1960).
[33] M. L. Goldberger and S. B. Treiman, Phys. Rev. **110**, 1178 (1958).
[34] S. Sakata, Prog. Theor. Phys. **16**, 686 (1956).
[35] Y. Nambu and G. Jona-Lasinio, Phys. Rev. **122**, 345 (1961).
[36] Y. Nambu and G. Jona-Lasinio, Phys. Rev. **124**, 246 (1961).
[37] Y. Nambu and D. Lurie, Phys. Rev. **125**, 1429 (1962).