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Field Theories without Fundamental Gauge Symmetries [and Discussion]

H. B. Nielsen and J. G. Taylor

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Field theories without fundamental gauge symmetries

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By using the lack of dependence of the form of the kinetic energy for a nonrelativistic free particle as an example, it is argued that a physical law with a less extended range of application (non-relativistic energy momentum relation) often follows from a more extended one (in this case the relativistic relation) without too much dependence on the details of the latter. We extend the lesson from such examples to the ideal of random dynamics: no fundamental laws are needed to be known. Almost any random fundamental model will give the correct main features for the range of physical conditions accessible to us today (energies less than 1000 GeV) even if it is wrong in detail.

This suggests the programme of attempting to 'derive' the various symmetries and other features of physics known today from random models at least without the feature to be derived.

As an example, D. Förster, M. Ninomiya and myself 'derive' gauge invariance in this way (Förster *et al.*, *Phys. Lett.* B **94**, 135 (1980)), and show that it has at least a non-zero probability for being effectively a symmetry. In fact we show that a certain non-gauge-symmetric lattice model has zero mass photons for a whole range of its parameters, so that it is not necessary to fine tune it to get massless photons. It comes about by means of a formal gauge symmetry achieved by introducing a superfluous number of field variables.

The achievements in our programme of random dynamics up till now are briefly reviewed. In particular, Lorentz invariance may be understood as a low energy phenomenon (S. Chadha, M. Ninomiya and myself).

An analogy between the development of physics as one goes to lower and lower energies and that of living species through the history of the Earth is put forward.

1. Introduction

The work that I shall present is part of an ambitious programme of random dynamics in which, in collaboration with various colleagues, I attempt to derive the laws of nature as we know them from fundamental laws that are assumed to be random and thus extremely complicated. (For a recent review see Nielsen 1983). I shall give as an example a piece of research work by D. Förster, M. Ninomiya and myself (Förster *et al.* 1980) in which we in a sense derive a symmetry law: gauge invariance (the same work also has been done by Shenker (1980) in an unpublished research report).

The picture of physics which I have in mind in this project of random dynamics is the following.

There exists some system of fundamental physical equations (or a fundamental action or the like) governing the time development of some fundamental fields. It may be difficult to say exactly in what terms it should be formulated, and it is part of my point that it may not be important to know this. Contrary to the speculation – that many physicists believe – that fundamental physics is simple, these fundamental equations are assumed here to be extremely complicated. Because of the high degree of complication assumed of the fundamental equations (the fundamental laws of nature we could say) we have to give up any hope of guessing their

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exact form. Our best hope is, then, to guess a very large class of possible fundamental equations (or actions) and a probability measure over that class. It would then make sense to assume that the actual fundamental equation system (or action or whatever) is randomly chosen from that class in the sense that after having added assumptions about how to connect the fundamental fields (degrees of freedom) to experimental observations one would find agreement with experiment within statistically expected accuracy.

At the present stage of science we have a series of regularities and theories that are well tested in some regions of application (energies per particle less than those corresponding to highest accelerator energies).

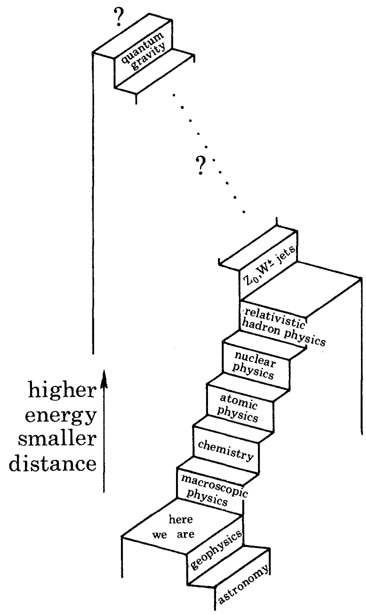

FIGURE 1. The quantum staircase illustrating how the various branches of physics – corresponding to the steps in the staircase – may be roughly considered as characterized by various scales of energy or inverse length (we put Planck's constant h and c equal to unity).

These regularities, such as the principle of relativity, may be called laws of nature, but we do not think of them as fundamental in the picture of random dynamics. Rather they should be derived in some limit as consequences of the fundamental laws of nature (i.e. the fundamental equation system or action or whatever) which are supposed random. This means that the success of the random dynamics idea hinges upon the possibility of deriving the 'non-fundamental' laws of nature from a major part, that is a part with high probability, of the complicated fundamental equation systems in the assumed large class.

In practice we concentrate on explaining the (presumed not so fundamental) regularities (or laws of nature) already known. But now instead of attempting to derive them from a totally random system of equations or actions we would in practice take a system (a model) in which several regularities other than the one to be derived are already present. If it is possible to derive a regularity from a random action without any regularities it is normally easier to do so if one is allowed to use some principles that are already known.

As we progress we may hope to use successively fewer and fewer known principles, and hope,

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at the end of a long series of calculations, to abandon any assumption about the existence of time or that the dynamics should be given by a Lagrangian. We may hope to go that far some day but it is difficult to imagine even in which language to formulate a model without time and without an action principle.

It may not be good to use the phrase 'laws of nature' since it means two things: (1) a regularity, a principle such as rotational invariance, the principle of relativity, or the linearity between energy and momentum squared for a non-relativistic free particle; (2) a complete model of describing in detail the dynamics for a branch or the whole of physical phenomena.

In both of these definitions the physical law can be either fundamental or only (approximately) valid under some limited range of physical conditions (for example when no particle has more energy than 1 TeV).

According to the hypothesis of random dynamics there may exist a fundamental law of nature in sense (2) of a model although it is so complicated that we shall not attempt to guess it. In this sense of a physical law (a dynamical model) there is no contradiction in taking the law to be random. With the other meaning (1), that of a regularity, a random law would hardly make sense, but according to 'random dynamics' there should not exist any fundamental law of nature in this sense.

Many non-fundamental laws of nature in sense (1) (of regularities) are known. In sense (2) (of a model), the standard model, i.e. QCD combined with the Weinberg–Salam–Glashow electro-weak gauge model, is one that presumably explains all we know today except for gravitational phenomena.

In the following section I shall put forward the possibility that the fundamental laws of nature are random and so complicated that we could not attempt to guess them, using as an example a derivation of the kinetic energy of a non-relativistic particle.

In §3 we go to a lattice field theory model without any *a priori* gauge invariance. Nevertheless, it turns out that one can change the notation so that a formal gauge symmetry arises. In spite of this symmetry being formal it turns out that there is some chance that it can provide an explanation for the light quantum (the photon) being massless, a phenomenon due to gauge invariance.

Finally, in §4 I present a very condensed list of what we have achieved or are about to achieve in the random dynamics programme and give concluding remarks in §5.

2. The programme of random fundamental laws of nature

In the random dynamics programme we wish to derive the regularities known today, such as Lorentz invariance and gauge symmetry, but which we suppose not to be fundamental, from a more fundamental model. We must admit that we do not know the more fundamental model but rather we shall choose a large class of such models and a probability measure on the latter; then we assume that a random model is valid.

Now I would like to illustrate how a regularity – the linearity between momentum squared $\boldsymbol{p}^2$ and energy $E(\boldsymbol{p})$ for a free non-relativistic particle – can arise in a limit, that of low velocity, in a case where we also know the more broadly valid model, which in this case is special relativity. We consider this example of random dynamics, which could have been done before the advent of special relativity, both for illustration and to argue a case where we know, i.e. after Einstein, that the idea of a derivation in accordance with the random dynamics programme is indeed

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correct. The relation between energy E and 3-momentum $\boldsymbol{p}$ for a particle according to the special theory of relativity is shown in figure 2,

$$E = (\boldsymbol{p}^2 + m^2)^{\frac{1}{2}} \tag{2.1}$$

in units wherein the velocity of light, c, equals one. A research project of the random dynamics type which could be imagined before Einstein would be to 'derive' the non-relativistic energy momentum relation

$$E = \boldsymbol{p}^2/2m \quad \text{or} \quad E \propto \boldsymbol{p}^2. \tag{2.2}$$

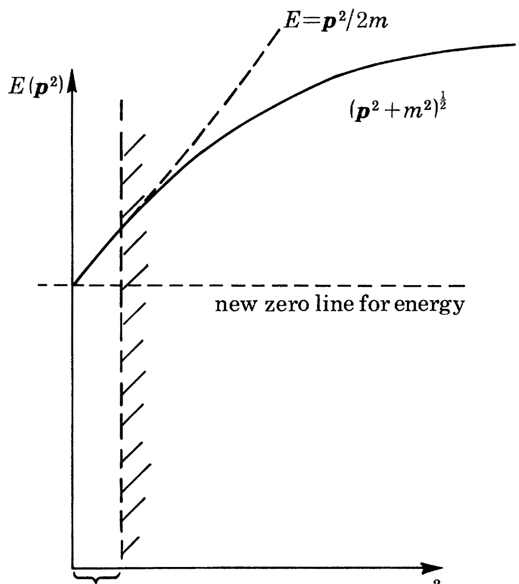

FIGURE 2. The relation between energy and (3-)momentum squared for a free particle. In non-relativistic physics (nuclear physics say) we only have access to the small momentum region where we can approximate the curve by a portion of a straight line.

Here m is a constant, the mass. This could be done from a 'random model' in which

$$E = f(\boldsymbol{p}^2), \tag{2.3}$$

and the function f is the object that is chosen randomly out of a large class of functions, say the analytic ones.

Even though in the derivation of $E(\boldsymbol{p}) \propto \boldsymbol{p}^2$ we use a random model that turns out later to be the special theory of relativity, it does not mean that, if I had done this last century, I should have necessarily believed that this random model was the most fundamental one. It might well be that what is used at one level as fundamental is just an approximation at another level.

We could argue from the principle of translational invariance that the energy E of a free particle cannot depend on the position but only on the momentum $\boldsymbol{p}$. We could further argue from rotational invariance for it being only a function of $\boldsymbol{p}^2$ and thus of the form (2.3). It is typical in practice in research work in the random dynamics programme to assume some already known principles although one must of course avoid assuming what we want to 'derive'.

Almost any analytic function $f(\boldsymbol{p}^2)$ would have a Taylor expansion

$$f(\boldsymbol{p}^2) = f(0) + f'(0)\,\boldsymbol{p}^2, \tag{2.4}$$

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valid for small $\boldsymbol{p}$, and apart from the unimportant constant $f(0)$ (if the particle is not annihilated) it has the form (2.2) if we identify

$$f'(0) = 1/2m. \tag{2.5}$$

When I say that 'almost' all analytic functions provide the same form for the energy for low momentum as the non-relativistic expression (2.2), I have in mind the possibility that the Taylor expansion coefficient $f'(0)$ could be zero. But if one imagined that the energy momentum law (2.3) was described by a randomly chosen function f, it would with a reasonable probability distribution, be very unlikely for $f'(0)$ to be exactly zero. If one has a real valued random variable, such as $f'(0)$ would be if the function f were random, and it has a smooth probability distribution, the probability for it taking any special value, e.g. $f'(0) = 0$, is zero; i.e. it is unlikely for it to take any given value specified in advance. We may say that it is unlikely that $f'(0)$ should have the fine-tuned value zero.

We thus see that the specific form (2.1) is not important for 'deriving' the low momentum or low velocity form. In fact an essentially random functional form for f would suffice.

We have here seen an example of a situation that often occurs. Details at the more fundamental level are not so important for effective physics in a corner (of low energy, say).

Thus it may be more important to assume in which corner some experiments are done than what are the fundamental equations. This makes the separation of experiment and theory less easy. See Eddington (1946); Slater (1957).

3. A lattice field theory model without gauge invariance

As a 'modern' example of a random dynamics derivation we shall consider how Förster *et al.* (1980), and independently Shenker (1980) effectively obtained gauge symmetry.

We consider electrodynamics (Maxwell's equations) formulated in terms of the four potential $A^\mu(x) = (\Phi(x), \boldsymbol{A}(x))$ so that the second rank antisymmetric tensor $F_{\mu\nu}(x)$ composed from the electric field $\boldsymbol{E}(x)$ and the magnetic induction $\boldsymbol{B}(x)$ is written

$$F_{\mu\nu}(x) = \partial_\mu A_\nu(x) - \partial_\nu A_\mu(x), \tag{3.1}$$

where

$$\partial_\mu = \partial/\partial x^\mu. \tag{3.2}$$

See, for example, Bjorken & Drell (1964) for notation. One set of Maxwell's equations,

$$\partial_\mu F^{\mu\nu}(x) = j^\nu(x), \tag{3.3}$$

is then given by extremizing the action

$$S = \int -\tfrac{1}{4}F_{\mu\nu}(x)\, F^{\mu\nu}(x)\, \mathrm{d}^4x + \int A_\mu(x) j^\mu(x)\, \mathrm{d}^4x, \tag{3.4}$$

considered as a functional of A_μ, while the remaining set of Maxwell equations

$$\partial_\mu F_{\nu\rho}(x) + \partial_\nu F_{\rho\mu}(x) + \partial_\rho F_{\mu\nu}(x) = 0, \tag{3.5}$$

follows alone from the form of (3.1). The gauge symmetry is the invariance of the action (3.4) under the replacement

$$A_\mu(x) \rightarrow A_\mu(x) + \partial_\mu \lambda(x), \tag{3.6}$$

where $\lambda(x)$ is an arbitrary smooth real function defined on space–time. (Of course the four current $j^\mu(x)$ should be conserved, $\partial_\mu j^\mu(x) = 0$.)

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The existence of this symmetry may be taken as the reason for the mass of the photon being zero. In fact adding a term

$$\int \tfrac{1}{2} m_\gamma^2 A_\mu(x)\, A^\mu(x)\, \mathrm{d}^4 x, \tag{3.7}$$

to the action S would spoil the invariance under the gauge substitution (3.6), but this term is just what would give the photon a mass m_γ.

Intuitively one thus expects that in order to obtain a model showing a zero mass photon it is necessary to fine-tune in the sense of choosing a very special combination of values for the parameters of the model. For example one would expect that the parameter m_γ should be zero, a very special value. One would think that such fine-tuning is characteristic for any model that has some symmetry.

But surprisingly enough we have found that there are several models in which symmetries do occur without fine-tuning. So the intuitive argument that a symmetry cannot appear without the fine-tuning of parameters seems to be circumvented in some cases.

In fact we (Förster *et al.* 1980) have considered a model that could be called 'U(1)-lattice electrodynamics without *a priori* gauge symmetry' and found that in spite of this model not being invariant under a gauge symmetry when looked at superficially we can introduce one formally and even obtain from it a physical effect, the masslessness of the photon. This model can be crudely described as the result of putting on a lattice, in a naïve way, electrodynamics with a photon mass term corresponding to the action being (3.4) plus (3.7) (the second term in (3.4) being considered a source term and therefore neglected). 'Putting on a lattice' means constructing a model in which the space(–time) continuum is replaced by a lattice of points, so that one includes, say, only those points (events) that have integer coordinates, in terms of a unit of length a called the lattice constant. Then the fields in the lattice field theory are only defined on the included points. Often though – as in the model we consider here – it is more elegant to let the field be defined on the links. The links are (see figure 3) the pieces of line connecting two neighbouring points, with integer coordinates, i.e. two points having three out of their four integer coordinates equal while one coordinate deviates by one lattice constant unit (e.g. x^ρ and $x^\rho + \delta^\rho_\mu a$). With the approximation that the lattice constant is small, the link has a rather well defined position x^ρ in space–time and it further has a direction along one of the four axes, let us say the x^μ-axis. We can then introduce in the 'naïve continuum limit' a connection between the $A_\mu(x^\rho)$-potential and the variables $U(\bullet\!\!-\!\!\!-\!\!\bullet)$ of the model that are defined on the links and take complex values restricted to being of unit norm

$$U(\bullet\!\!-\!\!\!-\!\!\bullet) \in \mathrm{U}(1) = \{z \in \mathbb{C}\ |z| = 1\}. \tag{3.8}$$

This connection is the relation

$$U(\overset{x^\rho}{\bullet}\!\!-\!\!\!-\!\!\overset{x^\rho+\delta^\rho_\mu a}{\bullet}) = \exp\,(\mathrm{i} e A_\mu(x^\rho) a) \quad (e \text{ is the electric charge quantum}). \tag{3.9}$$

A technical detail is that we keep the time coordinate purely imaginary before going to the lattice.

The 'U(1)-lattice electrodynamics without *a priori* gauge symmetry' is defined by its action

$$S = \beta \sum_{\Box} \mathrm{Re}\, U_\Box + \kappa \sum_{\Box} \mathrm{Re}\, U(\bullet\!\!-\!\!\!-\!\!\bullet) \tag{3.10}$$

where the summation $\sum_{\bullet\!-\!\bullet}$ runs over all the links while $\sum_{\Box}$ runs over all the plaquettes, the unit squares formed from four neighbouring links. The plaquette variable $U_\Box$ is defined for each

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plaquette as the product of the four variables $U(\bullet\!\!-\!\!\bullet)$ associated with the four links making up the plaquette in question. Here one should use the convention of associating the $U(\bullet\!\!-\!\!\bullet)$ variables to oriented links in such a way that switching the orientation of the link corresponds to taking the inverse of the variable

$$U(\overset{x^\rho\quad y^\rho}{\bullet\!\!\rightarrow\!\!\bullet}) = [U(\overset{y^\rho\quad x^\rho}{\bullet\!\!\rightarrow\!\!\bullet})^{-1}]^{-1}, \tag{3.11}$$

then

$$U_\Box = U(\ldots)\,U(\ldots)\,U(\ldots)\,U(\ldots) \tag{3.12}$$

is the product corresponding to the succession of oriented links around the edge of the plaquette $\Box$. The dotted lines in (3.12) just symbolize the other three sides of $\Box$ than the one in question.

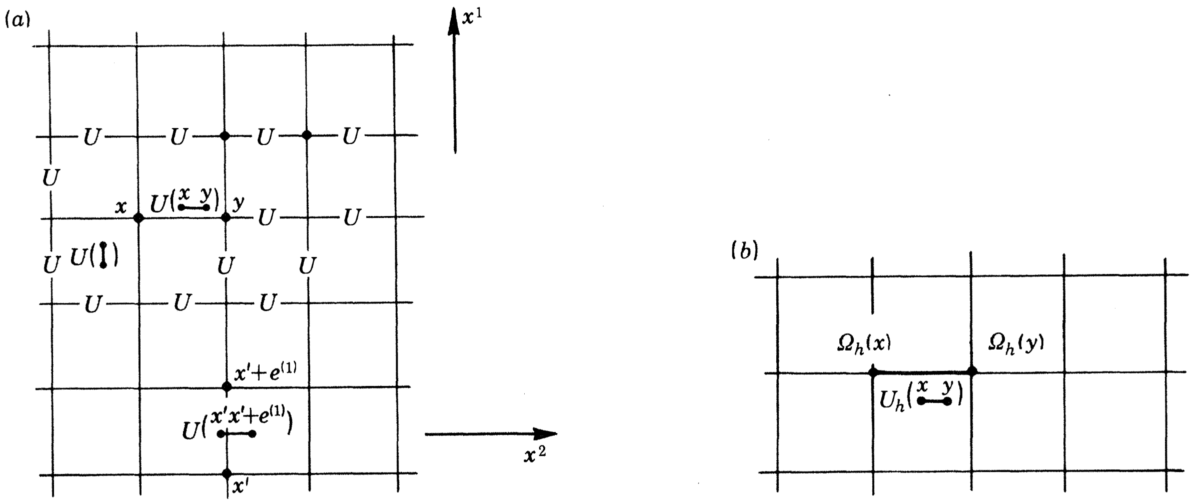

FIGURE 3. Symbolic drawings of a lattice model. In (a) we illustrate the fundamental variables $U(\bullet\!\!-\!\!\bullet)$, in (b) the variables in the formulation with superfluously many variables $U_h(\bullet\!\!-\!\!\bullet)$ and $\Omega_h(\cdot)$. The lattice should actually be 4-dimensional but has for simplicity been drawn as being 2-dimensional.

Expanding the action in a Taylor expansion to second order in $eA_\mu(x^\rho)\,a$ after insertion of (3.9), neglecting constant terms in the action and using the approximation of the lattice constant being small so that for example

$$a\partial_\nu A_\mu(x^\rho) = A_\mu(x^\rho + a\delta^\rho_\nu) - A_\mu(x^\rho), \tag{3.13}$$

we obtain in this (naïve) continuum limit the action

$$S = \int \mathrm{d}^4x(-\tfrac{1}{2}\beta e^2 F_{\mu\nu}(x)\,F^{\mu\nu}(x) - (\kappa e^2/2a^2)\,A_\mu(x^\rho)\,A^\mu(x^\rho)); \tag{3.14}$$

so naïvely it corresponds to a massive photon model. (Identify $\beta = 1/(2e^2)$ and $m^2_\gamma = \kappa/(2\beta a^2)$.) Also, there is no gauge symmetry under which the reasonable way would be to put the gauge transformation (22) on the lattice

$$U(\overset{x^\rho\quad y^\rho}{\bullet\!\!\rightarrow\!\!\bullet}) \rightarrow \Lambda(x^\rho\cdot)\,U(\overset{x^\rho\quad y^\rho}{\bullet\!\!\rightarrow\!\!\bullet})\,\Lambda(y^\rho\cdot)^{-1}, \tag{3.15}$$

because the term $\kappa \sum_{\bullet\!-\!\bullet} \mathrm{Re}\, U(\bullet\!\!-\!\!\bullet)$ in the action (3.10) is not invariant under this transformation (3.15). Here in the naïve continuum correspondence

$$\Lambda(x^\rho\cdot) = \exp(\mathrm{i}e\lambda(x^\rho)). \tag{3.16}$$

Only if κ is zero, or at least some specific value, one would expect to find that the photon mass calculated in this model would be zero even when quantum mechanical effects are taken into

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account. However, according to calculations done by Fradkin & Shenker (1979) and by Banks & Rabinoviči (1979) this is *not* so. Rather there is a whole range of values for the parameters β and κ of the model for which there is a massless photon. It is in fact sufficient that κ is smaller than, and β larger than, some critical values. If we think of the model in analogy with an instrument with buttons by which the values of β and κ are tuned, it is not necessary to fine-tune them to get a zero mass photon (like an automatic frequency control on a radio may make the most accurate fine-tuning superfluous).

The first step in seeing this surprising result is to argue that we can in fact introduce a gauge symmetry into the model in a rather artificial manner.

The trick is to write the model in terms of an unnecessarily large number of field variables: $U_h(\bullet\!\!-\!\!\bullet)$ defined on the links just like the original variables $U(\bullet\!\!-\!\!\bullet)$, and $\Omega_h(\cdot)$ defined on the sites $\cdot$, i.e. the points of the lattice. Both $U_h(\bullet\!\!-\!\!\bullet)$ and $\Omega_h(\cdot)$ are norm unity complex numbers like $U(\bullet\!\!-\!\!\bullet)$. Indeed we write

$$U(\overset{x^\rho}{\bullet}\!\!\rightarrow\!\!\overset{y^\rho}{\bullet}) = \Omega_h(x^\rho\cdot)\, U_h(\overset{x^\rho}{\bullet}\!\!\rightarrow\!\!\overset{y^\rho}{\bullet})\, \Omega_h(y^\rho\cdot)^{-1}. \tag{3.17}$$

There are now infinitely many choices of the fields $\Omega_h(\cdot)$ and $U_h(\bullet\!\!-\!\!\bullet)$ which give the same field configuration for the fundamental field $U(\bullet\!\!-\!\!\bullet)$ and thus one may transform around the former without changing the original field variables $U(\bullet\!\!-\!\!\bullet)$. In fact, one has invariance under the formal gauge symmetry

$$\left.\begin{aligned} U_h(\overset{x^\rho}{\bullet}\!\!\rightarrow\!\!\overset{y^\rho}{\bullet}) &\rightarrow \Lambda_h(x^\rho\cdot)\, U_h(\overset{x^\rho}{\bullet}\!\!\rightarrow\!\!\overset{y^\rho}{\bullet})\, \Lambda_h(y^\rho\cdot)^{-1}, \\ \Omega_h(x^\rho\cdot) &\rightarrow \Omega_h(x^\rho\cdot)\, \Lambda_h(x^\rho\cdot)^{-1}, \end{aligned}\right\} \tag{3.18}$$

where $\Lambda_h(\cdot)$ is the gauge function, like $\Lambda(\cdot)$ in (3.15), i.e. a norm unity complex function on the sites. But it is still surprising that a symmetry, which is as (3.18) purely due to a notation with extra many variables, can ensure a physical result, the masslessness of the photon for a whole region, a phase, of (β, κ)-combinations.

Essentially the way this comes about is that the site-defined field $\Omega_h(\cdot)$ has very strong quantum fluctuations when κ is small enough. It can be shown that there are no long range correlations in $\Omega_h(\cdot)$ in this case, so that the $\Omega_h(\cdot)$-field can be ignored as far as low energy or long distance properties of the model are concerned. Then it becomes natural instead of (3.9) to use the identification

$$U_h(\overset{x^\rho}{\bullet}\!\!\rightarrow\!\!\overset{x^\rho + a\delta^\rho_\mu}{\bullet}) = \exp\,(\mathrm{i}eA_\mu(x^\rho))$$

and one would then have gauge invariance (3.6). The mass term (3.7) will be forbidden and the masslessness of the photon be explained.

How exactly the dynamics of the model work might be complicated to see. The important point is that even with a random choice of (β, κ) there is a finite non-zero probability for obtaining a zero mass photon electrodynamics even with a gauge symmetry, although the latter is from the point of view of the fundamental model introduced just by notation and thus only formal.

This is a consequence of the very reliable estimates of Fradkin & Shenker (1979), Banks & Rabinoviči (1979) and by ourselves (Förster *et al.* 1980) and can be tested on a computer (see Ranft *et al.* 1983).

One may of course ask if we have 'derived' the Maxwell electrodynamics with its zero mass photon if it only appears with finite (non-zero) probability. I think though that one just has to imagine that the fundamental model should consist of several (probably interacting) sets of link fields of the type described and then most likely some of them would produce massless photons. Then we might instead have the problem of why we got only one of them, a question on which

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we have some premature speculations in a model in which we do not at first include translational invariance.

4. Achievements of random dynamics

Towards the end, let me mention briefly more progress that we have made or are making in the programme on random dynamics: in fact, Antonianidas *et al.* (1983), and Iliopoulos *et al.* (1981) (see also Iliopoulos 1981) have an alternative way of deriving gauge symmetry. They use the renormalization group and find that in a model with no gauge invariance at high energy as one restricts oneself to lower and lower energies and momenta, gauge invariance becomes more and more accurate but never exact. Their model deviates from ours by having no lattice and they are not allowed to introduce a photon mass term but only certain other gauge symmetry breaking terms.

The same method of renormalization group calculations was used by Chadha & Nielsen (1982) and by Ninomiya & Nielsen (1979) to show that for field theories with gauge symmetry already assumed, Lorentz invariance (including rotational invariance) if not valid, is however more accurately satisfied the lower the energy and momentum at which it is studied.

However, I tend to favour an explanation for Lorentz invariance which M. Lehto, M. Ninomiya and I are working on, and which is analogous to the method sketched in the foregoing section. In fact, one may consider gravity as a sort of gauge theory where it is either the Lorentz invariance or translational invariance or both that is gauged. If a theory of gravitation is then obtained without fine-tuning, we will also get Lorentz invariance in the neighbourhood of a space–time point as a side result.

In connection with non-Lorentz invariant models, Chadha and myself (Nielsen 1977, 1978) also find that a space–time with three space and one time dimensions is singled out. A fermion will have much smaller velocities in any further dimensions.

My work with I. Picek (Nielsen & Picek 1982 *a*, *b*) is more phenomenological seeking possible deviations from Lorentz invariance, i.e. deviations from the principle of relativity, and thus does not strictly speaking belong to the programme of random dynamics. I should like to mention here that there is an experiment by Aronson *et al.* (1982) showing that the parameters of the K^0–$\bar{K}^0$ system in the beam energy range 30–110 GeV are slightly energy dependent, meaning – if taken seriously – that Lorentz invariance is broken. Since it is only a few standard deviations of relativity principle breaking we should be cautious.

Lehto *et al.* (1982) also considered a generalization of electrodynamics with the $A_\mu(x^\rho)$-potential replaced by an antisymmetric tensor 'potential' $A_{\mu\nu}(x^\rho) = A_{\mu\nu}(x^\rho)$, and 'derived' the probable existence of a massless particle analogous to the photon.

In all these investigations quantum mechanics is a very important assumption.

However, S. Chadha, C. Litwin and myself (see Nielsen 1978, 1981) have argued that a random differential equation for the time development of a point in a high dimensional space is likely to approach a fixed point, become approximately linear, and thus be interpreted as the Schrödinger equation. This gives hope of even quantum mechanics resulting from a limit, that in which a long time has elapsed. However, this only resulted because we used as random velocity fields Fourier series with a finite number of random coefficients and these series had a lot of zeros. Taking random differentiable functions, there is only one set of measure zero of motion with a fixed point so that our 'derivation' of quantum mechanics is incorrect with the 'correct' measure (we are grateful to the referee for this comment).

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Also it should be mentioned that the discrete symmetries, parity, charge conjugation and time reversal of strong and electromagnetic interactions are well understood in the standard model and that isospin and the old Gell-Mann SU (3) are explicable if one just adds the assumption of the quark masses being small (Weinberg 1979, 1981, 1982; Zee & Wilczek 1979).

Attempting to get details about the standard model N. Brene and I (Brene & Nielsen 1982, 1983) have argued from a model with a random action breaking translational invariance, that gauge groups not having a connected non-trivial centre are likely to break down spontaneously so that the gauge particles (the analogues of the photon) obtain masses. By arguments of this type about what properties of a gauge group endanger it by a breakdown, we tend to favour that at low energies one should find the gauge group $S(U(2) \times U(3))$ (of special i.e. unit determinant 5×5 matrices composed of $U(2)$ and $U(3)$ matrices) which is just the one of the standard model to which one has arrived by more phenomenological considerations.

Froggatt & Nielsen (1979*a*, 1979*b*, 1981) have in a random model for mass matrices obtained very inaccurate predictions of, for example the ratio

$$\ln(m_\tau/m_\mu)/\ln(m_\mu/m_e) \approx 0.6 \pm 0.5,$$

where the masses of the three lightest charged leptons are denoted m_e (electron), m_μ (muon), and m_τ (tau lepton). It agrees almost too well with experiment.

Recently we have been speculating on how to obtain geometry out of a (random) gauge theory model. Fu Ying Kai and I found that in an anisotropic lattice electrodynamics it is possible to produce what we call a layered phase. This is a state in which a charged particle would be able to move only along some layers but not across them. The idea is then that it is the layers (or one layer curled up) that make up the four dimensional geometry we know, the geometry outside being unimportant. Also we hope then to 'derive' a principle of locality which says that there is no direct interaction except over very small distances: no action at long distances. This also looks promising.

5. Concluding remarks

It seems that there are indeed some ways of deriving many of the most typical features of what we know today from more random fundamental laws of nature, e.g. gauge symmetry, Lorentz invariance, linearity of the Schrödinger equation (in jeopardy though), discrete symmetries, and $(3+1)$-dimensionality space–time. We may even get some information on which gauge group to expect and some very crude information on lepton and quark masses, and Cabibbo angles.

The picture of random dynamics – which we find somewhat promising – may be said to be roughly analogous to the development of species of animals and vegetation throughout the history of the Earth. Also – for me at least – the Darwinian ideas of development of species have provided inspiration for the presently described work.

The analogy should be this: the geological time for the development of life corresponds to the logarithm of the length scale (or to minus the logarithm of the energy and momentum scale) for physics. The various geological periods may correspond to various branches of physics, many of which are probably yet to be discovered. The idea of random dynamics that various features – symmetries, linearity properties, etc. – arise at various levels corresponds to the various inventions or development of organs or mechanisms such as the RNA amino acid code and production system, muscles, brain, and social behaviour. Maybe there is even a correspondence in that the

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development of life out of non-living chemistry (and physics) is as especially speculative as the development of physics out of chaotic fundamental laws of nature may also seem.

It is a pleasure to thank all my collaborators whose work I have presented and Zheng Hai-Bin for finding the reference to Ranft (1983). P. Mansfield is thanked for help in improving the English.

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Discussion

J. G. Taylor (*Department of Mathematics, King's College London, Strand, London WC2R 2LS, U.K.*). There seems to be considerable arbitrariness in Dr Nielsen's talk in spite of his claim to obtain much from few assumptions. In particular he uses a regular lattice, with only one lattice spacing. A more general approach would be to use a lattice with many spacings, or even a random lattice. Yet in the latter case recent work by Saclay (C. Itzykson and his collaborators) has shown that for the free field (in one dimension) the spectrum of states does not have a

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natural cut-off. Nor is the problem of fermions easy to resolve; recent questions on the degeneracy of massless fermions on the lattice are apparently completely open on such lattices. Surely these problems have to be satisfactorily analysed before any claims to having constructed 'random dynamics' in a general way can be substantiated.

H. B. Nielsen. I thank Professor Taylor for calling my attention to work by C. Itzykson, T. D. Lee *et al.* He is certainly right that the lattice electrodynamics model without gauge symmetry which I described has several arbitrary features such as the lattice being a regular hypercubic one. The excuse for this is that it is one of the basic ideas in the project of random dynamics that the detailed features of a model are not essential for what results in the limit of, say, low energy, to which we have experimental access today, so we hope that the arbitrary features are not essential.

For instance it is almost certainly not important whether the lattice is a simple cubic one or whether we would take some more complicated but still regular lattice structure. That we took the lattice to be a regular repetition of the same unit is an assumption justified as a mild form of the principle of translational invariance. That makes it less arbitrary, but of course, to complete the random dynamics project we should then also provide a 'derivation' of translational invariance. This M. Lehto, M. Ninomiya and myself have attempted to do in a model having a lattice the structure of which is dynamical and therefore normally highly irregular due to quantum fluctuations.

It is correct that the problem of putting fermions on a lattice when they belong to a parity non-invariant system of representations is a severe difficulty for random dynamics. This difficulty with chiral fermions on a lattice is especially a problem for our understanding of gauge invariance (the work which I treated in some detail). The reason for this being so severe a problem is (1) that we need a cutoff to be taken seriously, preferably a lattice, and (2) we obtain gauge invariance by definition, and therefore exactly, even at the lattice scale. With these requirements we have no-go theorems (by M. Ninomiya and myself) that seem to leave no way open for describing the phenomenology of parity violation by the weak interactions without giving up other principles, such as locality, in a drastic manner.

The best way out might be to allow the quarks and leptons to be bound states of some system of fermions belonging to representations with equally many species of right and left handed Weyl particles. Maybe also the openness of the fermion problem on the Itzykson–Lee type irregular lattice – which Professor Taylor mentions – gives some hope that species doubling might be avoided.

The problem of species doublers is presumably a general problem for any claim of taking an ultraviolet cut-off seriously, and especially for us.