INTRODUCTION

1 . This book is a collection of some of my articles on non-perturbative aspects of quantum field theory. More than half of the papers reprinted here have a review character. Most of my review articles and many of my papers tend to be written under pressure of time and are therefore far from perfect. Thus, my ambition is, in general, limited to describing some ideas that I feel are beautiful or important, or both, and the main results that can be derived from them. It happens quite often that, in the course of the writing, I get sidetracked on a detour which, it later turns out, could have been avoided. Only rarely have I been able to do some rewriting or to pursue some scholarly ambitions going beyond the more obvious purposes of an article. The reader will notice that, for all these reasons – and, most certainly, for reasons rooted more deeply in my personality – most of the papers collected in this book have the character of sketches, even though they are sometimes rather elaborate and lengthy. I hope and believe that, in favourable cases, my sketches reproduce some of the main contours of what is being portrayed. However, the interested reader should take some time to rearrange the main lines of my drawing, or turn to the original sources, in order to understand more clearly what the main ideas and the main purpose of an article are.

2 . In order to preserve visual impressions and in order to try out new techniques or improve old ones, artists usually fill many sketch books with more or less rough sketches. What is being published here is in many ways analogous to an artist's sketch book. One should ask whether such imperfect sketches are of interest to the public. I am not sure what the right answer is. I suppose I leave the answer to the publisher – and to the reader.

Artists usually try to convert those sketches which they feel are really good and interesting into more or less perfect paintings. I sometimes try to do the same with some of my sketches. But often I do not succeed, either because I find it too difficult, time-consuming or painful to attempt to transform a scientific sketch into a scholarly work of science, or because other people can do it better, in a shorter time, than I could. Sometimes that can be a little awkward.

The reader may permit me one last, more general comment on the relationship between art and basic science. I think that artists and scientists engaged in basic research share closely related concerns, and that the motivation for their efforts and their successes originate from inter-related sources. They both contribute to the development of general human culture, albeit most often in very modest ways. But the cumulative effect is significant! The question of whether what they do is useful and applicable is and should remain secondary. At the root of their efforts, intuition, insights and visions of relatively little precision and of sometimes quite irrational nature play an important role. They are not only intellectually, but most often also emotionally involved in their work. They tend to be fairly ambitious, often rather egocentric people, and they are driven by forces other than those of becoming wealthy rapidly, or of having a comfortable and leisurely life. In the course of trying to implement an idea they tend to have the experience that their work develops its own dynamics, partially gets out of their control, forces them around corners or guides them through hidden doors to open the view to unexpected, new scenery – as if ideas had some pre-existence outside their minds, and the process of their incarnation followed rules which they had not set themselves.

Taken together and averaged over sufficiently long periods in time, the contributions of artists and scientists to the evolution of culture and civilization are clearly important. Reducing art to purely functional or decorative purposes would eventually suffocate art – as experience during certain periods in the history of art would, I believe, tend to confirm. Reducing science to merely "useful" or applied science would eventually cut it from its life roots. It would surely turn out to be a disservice also to applied science. (I hope society is not in the process of testing this prediction by experiment.)

3 . Many papers reprinted in this book represent the fruit of a collaborative effort in which my collaborators and friends have played fundamental roles. Where my review articles are based on such collaborations the original papers may well be more perfect than my reviews, and the interested reader is advised to consult the original papers.

Furthermore, some of my best work, often in collaboration with T. Spencer, does not concern quantum field theory, but areas like equilibrium statistical mechanics, disordered systems theory, dynamics of systems with many degrees of freedom and non-relativistic quantum theory. It is hardly represented in this volume. It tends to be rather more professional than most of the articles on the following pages.

Finally, I do not consider the articles reprinted in this book to represent anything like my "collected works" on non-perturbative quantum field theory – I still try to do work in that area, from time to time.

The reader should keep these remarks in mind! Also, my selection of papers is personal and does not cover some of the most important chapters in non-perturbative quantum field theory. I hope those chapters will appear in similar reprint collections by authors more competent to write them than I.

4 . I now turn to some brief comments on the different parts of this book and on some of the papers, and to some indications of relations between them.

Part I, "Phase Transitions and Continuous Symmetry Breaking", describes work carried out in collaboration with R. Israel, E.H. Lieb and, primarily, with B. Simon and T. Spencer. Our main aim was to understand phase transitions accompanied by the spontaneous breaking of continuous symmetries in systems like multicomponent scalar field theories in three dimensions and (classical) Heisenberg ferromagnets on three and higher dimensional lattices. Thinking of phenomena like chiral symmetry breaking in QCD, it becomes clear that a mathematically precise study of such phase transitions is important for quantum field theory. We wanted to understand mechanisms driving phase transitions with spontaneous symmetry breaking and the associated emergence of Goldstone bosons in a mathematically precise way. More or less known to us were heuristic analyses of the spontaneous breaking of continuous symmetries in models of quantum field theory and statistical mechanics (droplet picture, spherical model, $1/N$-expansion), some proofs of the Goldstone and the closely related Mermin-Wagner theorems [1,2], some forms of spin-wave theory and of Bose-Einstein condensation. After some futile attempts, trying, for example, to put the droplet picture on a firm basis, it eventually turned out that the right idea was to try to establish an analogue of the Källen-Lehmann *spectral representation* for the connected two-spin correlation function, with a temperature-dependent bound on the spectral measure that, in relativistic quantum field theory, would correspond to a bound derived from canonical commutation relations, with Planck's constant, $\hbar$, and temperature, T, playing analogous roles. Such a result would yield bounds on the connected two-spin correlation function which, when combined with a *sum rule* (e.g., $\vec{S}_x \cdot \vec{S}_x = 1$, in a classical Heisenberg ferromagnet), implies that the full two-spin correlation function, $< \vec{S}_x \cdot \vec{S}_y >_T$, exhibits *long range order* for sufficiently small values of the temperature T. Adaptations of the arguments of Mermin and Wagner [2] then turned out to prove the presence of a Goldstone boson in various more or less equivalent forms. It was found that the simplest proof of suitable bounds, so-called *infrared bounds*, on the connected two-spin correlation could be patterned on some results of Glimm and Jaffe [3] known by the name "π- and $\nabla\varphi$-bounds" in canonical quantum field theory.

There is no doubt that the concept of *Osterwalder-Schrader*, or *reflection positivity* – which had first been introduced and exploited in [4] – played an important role in our thinking and in many subsequent applications. In fact, our proofs of infrared bounds are based on the hypothesis of reflection positivity or, equivalently, of existence of a positive transfer matrix. (This represents a limitation of our methods which is also present in later generalizations. Reflection positivity is completely natural in the context of Euclidean (lattice) field theory, but far less natural in statistical mechanics.)

Our first results (by B. Simon, T. Spencer and myself) appeared in [5]. They were subsequently developed into a fairly comprehensive theory of phase transitions with continuous symmetry breaking, with contributions by Dyson, Israel, Lieb, Simon, Spencer and myself. This may become clear from the reading of the second and third papers of Part I, and references there.

Although the work described in Part I is conceptually not terribly original, it has the advantage of being quite elegant and technically transparent. Moreover, it turned out to have many important implications. Besides offering one ascent – there are some others – to a mathematically rigorous understanding of phase transitions, continuous symmetry breaking and its relation to the presence of massless excitations, the Goldstone bosons, in a class of models of physical interest, our work found applications in the study of the following problems:

(1) Existence of a critical point in some classical lattice spin systems [6].
(2) Bounds on field strength renormalization, with implications for the triviality problem, in four-dimensional quantum field theories, such as $\lambda\varphi_4^4$-theory and QED_4. This is discussed in Part IV, "Triviality of $\lambda\varphi_4^4$", and references given there. Some remarks are also contained in the first paper of Part I (bounds on two-point function of field strength in abelian lattice gauge theory, etc.).
(3) Study of the deconfining phase transition in lattice gauge theories at positive temperature and of chiral symmetry breaking in some strongly coupled lattice gauge theories by C. Borgs, M. Salmhofer and E. Seiler [7,8].

Although the work in [5] is mathematically complete, our arguments are simpler than many of the heuristic analyses of the same phenomena. One might have expected, therefore, that they will find their way into most new textbooks on phase transitions and critical phenomena, or on field-theoretic methods in statistical mechanics. Actually, that expectation does not appear to have been fulfilled, and, therefore, it makes sense to reprint some reviews of our work.

After the work summarized in the first two papers of Part I had been completed, Tom Spencer and I went on to analyze, using mathematically rigorous methods, the Berezinski-Kosterlitz-Thouless transition [9] in two-dimensional models such as the two-component Coulomb plasma (transition from the plasma phase with Debye screening to the dipolar phase without screening) [10] and the classical XY-model, the transition in the one-dimensional Ising model with $1/r^2$-interaction energy [11], and the deconfinement transition in the four-dimensional $U(1)$-lattice gauge theory [12]. This work is referred to in the third article of Part I, but see [13,14,15,16] for the original results. It is reviewed in another book [17] and has been elaborated upon, extended and simplified in numerous subsequent papers. Our results are the basis of the work described in the second paper of Part III ("Magnetic monopoles and charged states in four-dimensional abelian lattice gauge theories", with P.A. Marchetti).

On a more conceptual level, the work of Spencer and myself mentioned above led us to propose a rather precise form of the concept of "*asymptotic enhancement of symmetries*" which is discussed in some detail in the third paper of Part I. Clearly, this concept is of obvious importance in "non-perturbative quantum field theory". It also illuminates in a useful way the nature of the transition from the plasma phase to the massless Kosterlitz-Thouless phase in the two-dimensional, classical two-component Coulomb gas (enhancement of a spontaneously broken discrete symmetry $\mathbb{Z}$, to a spontaneously broken continuous symmetry, $\mathbb{R}$), of the deconfining transition in the four-dimensional, compact $U(1)$ and $\mathbb{Z}_n$ lattice gauge theories, and of many related phenomena. The best known (and least understood) example of symmetry enhancement is the enhancement of lattice symmetries to full Euclidean symmetries in the continuum limit of lattice field theories. Symmetry enhancement at very high energy scales has, of course, been discussed in connection with grand unified theories of elementary particles.

The third article of Part I also discusses the "spontaneous breaking" of local gauge invariance and sketches a gauge-invariant formulation of the Higgs mechanism and the role of field configurations with topological defects in gauge theories, based on joint work with G. Morchio and F. Strocchi.

5 . The first article of Part I contains a rather long section on the ***quantum theory of topological solitons*** which is the main topic of Part II, "Non-Perturbative Quantization of Topological Solitons". I was working on the problem of soliton quantization in two dimensional quantum field models with kinks, in 1974/1975, just before and during my work with B. Simon and T. Spencer on phase transitions. The last part of the first paper in Part I, the first paper in Part II, the third paper in Part III, and the first paper in Part VI provide somewhat complementary introductions to the theory of superselection sectors and topological charges in quantum field theory, in particular to the quantum theory of topological solitons. In this area I have made what I believe are among my more original contributions to non-perturbative quantum field theory to-date. In the course of my work on solitons, in 1975, I found examples of "*dual algebras*", or "*exchange algebras*", of fractional charges, of "parafermions", in the sense of Zamolodchikov (but without conformal invariance), and, a few years later, of the connection between topological solitons and topological (line) defects in Euclidean functional integrals. I do not think that any of this work – with the exception of the first paper in Part VI – became well-known. So, on hindsight, one should ask: what went wrong? Presumably, I knew too little about the physics of quantum fields to draw clearly explicit physical consequences from my analytical findings, and I knew too little mathematics to understand that there were some interesting, *general* mathematical structures to be uncovered. But it was also a question of style. My results were embedded in tedious, lengthy mathematical analyses which many people find somewhat inaccessible and, perhaps, somewhat beside the point. Some of the most original ideas remained unpublished, because I could not implement them in the form of mathematically rigorous constructive field theory.

I was working on problems concerning superselection sectors and topological charges in quantum field theory during three different periods, from the end of 1974 till the middle of 1977, in 1979, and, on and off, from 1984 till the present. In recent years, my efforts were supported or superceded by those of P.A. Marchetti, G. Felder, G. Keller, F. Gabbiani and T. Kerler.

One goal during the first period was to study two-dimensional models exhibiting topological solitons as examples for the deep axiomatic results of Doplicher, Haag and Roberts [18] concerning superselection rules and quantum statistics; to better understand the connection between *vacuum degeneracy* and *symmetry breaking*, on one hand, and the existence of *soliton sectors* with non-trivial topological charge in such models, on the other hand; and to put the semi-classical analysis of quantum solitons then carried out by many distinguished physicists, see [19] and refs. given there, on a non-perturbative basis. All this led, in a natural way, to a primitive notion of "exchange algebra" (see e.g. [20], formula (71); see also §6 of [20]) which I found before I learnt about the work of Kadanoff and Ceva [21], and Wegner [22].

Exchange algebras are algebras generated by unobservable, charged fields, ψ_α, (or by bounded functions of such fields) with quadratic relations, e.g. of the form

$$\psi_\alpha(\vec{x},t)\,\psi_\beta\,(\vec{y},s) \;=\; R(\pm)^{\gamma\delta}_{\alpha\beta}\,\psi_\gamma\,(\vec{y},s)\,\psi_\delta\,(\vec{x},t)\;,$$

for $(t-s)^2 - (\vec{x}-\vec{y})^2 < O$ and $\vec{x} \gtrless \vec{y}$. Here $(\vec{x},t)$ and $(\vec{y},s)$ are points in two-dimensional Minkowski space; the order relations $\vec{x} > \vec{y}$ or $\vec{x} < \vec{y}$ are then Poincaré-invariant, for space-like separated points. The matrices $R(\pm)$ turn out to be Yang-Baxter matrices.

Such unobservable fields play an important role in analyzing the statistics and scattering of charged particles.

The ansatz just described has been criticized for valid reasons, but is perfectly correct and general for field algebras with *abelian* fractional statistics. The models studied in the seventies gave rise to such field algebras – examples that one now regards as somewhat trivial. Nevertheless, there was something a little new in the idea that, in two space-time dimensions, charged *fields* need have neither Bose nor Fermi statistics, but could have what is now called *fractional statistics* interpolating between Bose and Fermi statistics.

Exchange algebras have a precursor – the algebra of Weyl operators, $W(a,b)$, satisfying the quadratic Weyl relations

$$W(a,b)\,W(a',b') \;=\; e^{i(a\cdot b' - a'\cdot b)}\,W(a',b')\,W(a,b)\;,$$

with

$$W(a,b) \;=\; exp\,i\,[a\cdot p + b\cdot q]\,,$$

where q and p satisfy Heisenberg's canonical commutation relations. The Weyl relations for systems with infinitely many degrees of freedom and their representation theory (developed by Gårding and Wightman, and by Segal) are at the basis of canonical quantum field theory. The free, massless field in two space-time dimensions is an example of a canonical field theory. This field theory describes left and right-moving waves created from the vacuum by what has become known as *vertex operators*, e.g.,

$$V_\lambda\,(\vec{x},t) \;=\; N\,exp\,i\,\lambda\,[\pi(\Theta^{\pm}_{\vec{x}},t) \,+\, \phi(\vec{x},t)]\,,$$

where $\Theta^{\pm}_{\vec{x}}(\vec{y}) \;=\; 1$, for $\vec{y} \gtrless \vec{x}$, $=\; 0$, otherwise, and where N denotes normal ordering. Bounded versions of such vertex operators were first studied by Streater and Wilde [23] as examples for axiomatic results of Doplicher, Haag and Roberts. The analysis of Streater and Wilde was inspired by the seminal work of Skyrme on bosonisation in two dimensions [23]. Their results were extended in [20] (see also the second paper of Part II) to interacting field theories, including the sine-Gordon model.

In 1979 and in the eighties, then in collaboration with P.A. Marchetti, I became interested in the analysis of quantized solitons within the framework of Euclidean functional integrals. It became clear in 1979 that the existence of topological solitons in some quantum field model is intimately connected with the existence of *topologically stable line defects* in Euclidean field configurations, such as domain boundaries (contours) in the $\lambda\varphi^4$-theory in two dimensions, or vortices in three-dimensional Higgs models. This was first discussed in the third paper reprinted in Part III. Soliton Green functions can be constructed by minimally coupling the Euclidean matter fields of the model to an external gauge field whose curvature has singularities in points of Euclidean space-time. In every field configuration there must be open line defects connecting those space-time points. This method has been further developed in the work of Schroer, Swieca and collaborators [25], and in the work of Marchetti and myself; see the second and third article of Part II, and the second and last article of Part III. The masses of solitons turned out to be given in terms of corresponding *defect free energies.* Such a connection had been envisaged by Glimm and Jaffe for the two-dimensional Ising model and was first made precise for the kinks of the $\lambda\varphi_2^4$-model in [24], in 1977. This is discussed in detail in the first article of Part II.

Thanks to M. Struwe, the analysis of soliton Green functions, expressed in terms of Euclidean functional integrals, with the help of semi-classical methods has recently led to a decent piece of mathematics reprinted as the last paper of Part II.

Through studying the work of Kadanoff and Ceva [21] and of Jimbo, Miwa and Sato [26], it had become clear in 1979 that dual (or exchange) algebras for order and disorder fields were in direct correspondence with ***non-trivial monodromy*** *properties* of their Euclidean Green functions. This was also noticed by Schroer, Swieca and collaborators. These findings (largely unpublished) turned out to have been a useful preparation when two-dimensional conformal field theory was reborn in 1984 [27]. The work of Belavin, Polyakov and Zamolodchikov [27] and of Knizhnik and Zamolodchikov [28] on conformal field theory and of Jones [29] on knot theory provided the motivation and justification to finally systematically develop the idea of exchange algebras and their relation with non-trivial monodromy properties of Euclidean Green functions and with the representation theory of the ***braid groups.***[1]

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6 . Apart from the examples of exchange algebras that had appeared in the quantum theory of topological solitons, as discussed above, exchange algebras in the context of two-dimensional conformal field theory first appeared in the work of Gervais and Neveu on the quantization of Liouville theory [30]. Their work did not receive the attention it deserved. Further important work was carried out later, independently, by Tsuchiya and Kanie [31], Rehren [32] and myself [33] (a summary of my results of that period is contained in the first paper reprinted in Part VI).

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[1] In retrospect, some of the developments around this theme, between 1987 and 1990, can perhaps best be characterized by recalling the following quotation from Blaise Cendrar's "L'or": 'Rêverie. Calme. Repos. / C'est la paix. / Non. Non. Non. Non. Non. Non. Non. Non. Non. / C'est l'or. / Le rush.' (I believe that a few people did find some gold – but some found less precious metals.)

My own efforts were also motivated by an analysis, due to Marchetti and myself, of anyons in Chern-Simons-Higgs models, reprinted as the last paper of Part III, that we had started in 1986, under the influence of talks by Y.S. Wu and G. Semenoff on related matters which we had attended, and motivated by a desire to understand some aspects of the fractional quantum Hall effect.

While the work of Gervais and Neveu, Tsuchiya and Kanie, and Rehren specifically addressed two-dimensional conformal field theories, the work of Marchetti and myself on anyons and my study of exchange algebras in two-dimensional theories suggested that these algebras represented a concept reaching beyond the realm of conformal invariance, that similar mathematical structures would reappear in three-dimensional gauge theories, and that the algebraic methods of Doplicher, Haag and Roberts might be adequate for a general, model-independent analysis. These ideas are developed systematically in [34] and, for three-dimensional theories, in [35], and in several more recent papers. (The general structure in three space-time dimensions is now understood, while the most general version of field algebras in two-dimensional theories is still unknown.) The role played by exchange algebras in two-dimensional conformal field theory, generated by *chiral vertex operators*, was investigated in detail by Rehren and Schroer [36]; Moore and Seiberg [37]; Felder, Keller and myself [38]; Babelon [39]; King and myself [40]; and many other people. All these developments are reviewed in the first six papers reprinted in Part VI, where many references to the original literature can be found.

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7 . Fractional statistics was not only found in the analysis of field statistics in two and three dimensional quantum field theory, but turned up in a study of *particle statistics* in quantum mechanics in $2 + 1$ space-time dimensions, due to Leinaas and Myrheim [41], in 1977. They considered point-like particles carrying electric charge and magnetic flux and noticed that, as a consequence of the Aharonov-Bohm effect, such particles exhibit fractional statistics under exchange of positions. This work went unnoticed, and the ideas of Leinaas and Myrheim were rediscovered, a few years later, by Goldin, Menikoff and Sharp [42] in their study of current algebra in non-relativistic physics, and by Wilczek [43]. It deserves to be mentioned that Goldin, Menikoff and Sharp made the connection between fractional statistics in two dimensions and representations of the braid groups, whence the currently used expression of "*braid statistics*". This was popularized in [44] which was the paper from which I originally learnt about the braid groups. In contrast to other people working on braid statistics, they also conceived the possibility of ***non-abelian*** braid statistics, a form of quantum statistics whose mathematical aspects have been understood only recently; see [34,35] and the sixth paper reprinted in Part VI. Unfortunately, their work did not receive much attention either. The situation only changed when Wilczek's work [43] appeared and its relevance for the theory of the fractional quantum Hall effect [45] (Laughlin vortices in incompressible quantum Hall fluids with filling factor $\frac{1}{3}$, $\frac{1}{5}$ are fractionally charged excitations with fractional statistics), and, perhaps, for the theory of layered high T_c superconductors [46] was

noticed. All this is described in a reprint collection introduced and edited by Wilczek [47].

The results in [43] through [46] triggered the interest of P.A. Marchetti and myself in braid statistics, in the context of $(2+1)$-dimensional Chern-Simons gauge theories. Our early results are summarized in the last paper of Part III and are also mentioned in the second paper of Part VI. One of the puzzles we attempted to understand was how the exchange algebras of charged fields of the type of Mandelstam strings in three-dimensional Chern-Simons gauge theories would lead to the braid statistics of particles, in the sense of [41,42,43], in the asymptotic (scattering) states of such theories. A sketch of our insights appears in the last paper of Part III, a more precise theory was developed in [48]; but there is room for better understanding of these matters in theories with non-abelian braid statistics.

8 . Since 1986, the exploration of braid statistics has been one of my main scientific interests. With the appearance of the work of Reshetikhin et al. [49] it became plausible that *quantum groups* (more generally, "quasi-Hopf algebras") had a role to play as *generalized symmetries* of quantum field theories in two and three spacetime dimensions, whose representation theory would reproduce the super-selection rules and sectors and the structure constants of the exchange algebras – the braid statistics – of these field theories. In spite of early insights and partial results in this direction (see especially [50], and the fifth paper of Part VI, and refs. given there) this turned out to be a tricky and therefore fascinating problem. The best results in this area known to me – examples where the idea of quantum groups as generalized, global symmetries of low-dimensional quantum field theory, can actually be proven, mathematically, to work – are due to T. Kerler [51], in whose work I have had the luck to be somewhat involved, to G. Mack and V. Schomerus [52], and to G. Felder and C. Wieczerkowski [52], and refs. given there. (The work of Kerler and our joint efforts are briefly sketched in the sixth paper of Part VI.) On hindsight, it is clear that we should have followed the lead of S. Doplicher and J.E. Roberts and formulated braid statistics and the problem of constructing Hopf algebras as generalized symmetries whose representation theory reproduces the super-selection structure and braid statistics of some quantum field theory using the language of *abstract tensor categories*. See [53] for their fundamental results. Braided tensor categories offer a point of view unifying the theory of super-selection sectors with braid statistics in low-dimensional quantum field theory, the theory of quasi-triangular (quasi-) Hopf algebras [54], discrete versions of current algebras and WZNW models, and the theory of those invariants of knots and links, and of invariants for 3-manifolds [55], that generalize the Jones polynomial. (The connection between the braid statistics and operator product expansion (fusion) of chiral vertex operators in conformal field theory and the theory of invariants for links in 3-manifolds and other related objects (ribbon graphs), is described in the fifth paper of Part VI. We learnt from those efforts that all one needed for the construction of such invariants were certain combinatorial data, six-index symbols related to "braiding" and "fusing", which

rational conformal field theories – but quantum groups, too – provide. But the full structure of conformal field theory is irrelevant. In mathematical jargon, any braided monoidal category with unit and conjugates, subobjects and direct sums – we call this a "*quantum category*" – provides suitable combinatorial data for the construction of link and 3-manifold invariants. This can be extracted from [56]. In that paper we also suggested how quantum groups might appear as symmetries of conformal field theories. Our ideas were similar to independent ideas of Moore and Reshetikhin.)

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9 . One might be worried that, over all that abstract mathematics, one would forget the physics. Clearly, two-dimensional conformal field theory has brought about a revolution in our understanding of the theory of critical phenomena in two dimensions. These developments are well-known. In 1989/90, it turned out that chiral current algebra (especially $U(1)$ and $SU(2)$ current algebra) and the closely related topological Chern-Simons gauge theories of Witten [55] were perfectly natural tools in the analysis of the *fractional quantum Hall effect* – almost as if they had been invented for that purpose. This observation can be extracted from a paper of Halperin [57] on chiral edge currents in integer quantum Hall systems and was made, independently, by several people, including Wen, whose work came first, Stone, Kerler and myself and, most probably, other people as well. The beginnings of our work in this area which is still continuing are described in the last paper reprinted in this volume.

In the form of chiral electric and spin currents flowing around the boundaries of incompressible quantum Hall fluids, nature manifests itself as an ***anomalous***, $(1+1)$-*dimensional, chiral gauge theory*; it cancels the gauge anomaly by a $(2+1)$-dimensional, topological Chern-Simons term – as it should [58].

In other two-dimensional, incompressible quantum fluids, nature appears to have realized non-abelian braid statistics (related to the one that can be reconstructed from the representation theory of the quantum group $U_q(s\ell_2), q = exp\, (i\pi/n+2), n = 2,3,\dots$) [59]. But experimental realizations of such fluids and observable consequences of non-abelian braid statistics, related to a non-abelian form of the Aharonov-Bohm effect, are not really explored yet.

It looks very promising to try to apply similar ideas and methods to a variety of other quantum mechanical many-body systems, in particular to superfluids, to chiral spin liquids, to systems in quantum optics, etc.. Many groups of people, including some of my collaborators and I, are presently pursuing such applications. Concepts and methods from *gauge theory*, including the theory of anomalies, confinement and the deconfinement transition, magnetic monopoles, the Anderson-Higgs-Kibble mechanism, chiral symmetry breaking, etc., and from *current algebra* are likely to play a fundamental role in condensed matter physics. One is starting to understand why and how abelian and non-abelian, external and dynamical gauge fields can appear in non-relativistic, quantum mechanical many-body theory. We are, I think, in the process of extending the Landau theory of thermodynamic phases and of phase transitions to a *gauge theory of thermodynamic phases and of phase transitions.* I tend to think that this is an exciting development.

All this motivates the reprinting of the last paper, with T. Kerler, in Part VI which concerns the quantum Hall effect and of the last three papers in Part III.

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10 . I still have hopes, perhaps romantic ones, that string theory, or something inspired by it, will come back to life again. I believe it is interesting to attempt to formulate string theory in an "invariant" way, quite like it is useful to formulate geometry in a coordinate-independent way. One might, for example, start with a family, $\mathcal{F}$, of hyperfinite type III_1 von Neumann algebras – to be a little technical – indexed by intervals of the circle with non-empty complement (or of the super-circle). It may pay to formulate the starting point using the language of sheaves. One then proceeds to study a certain class of "good" states on that family of von Neumann algebras: good states satisfy *Haag duality.* Moreover, if one makes more precise what is meant by "good" it is likely that one can reconstruct from a good state, ω, on $\mathcal{F}$, using the *Tomita-Takesaki theory of modular automorphisms,* a projective unitary representation of the group of Möbius transformations leaving the unit circle invariant. A "good" state is, in particular, one for which the generator, L_0, of rotations of the circle is bounded from below. One then studies the Möbius-covariant representations of $\mathcal{F}$ and shows that they can be generated by a semigroup of localized *endomorphisms of an arbitrary element of $\mathcal{F}$. This structure determines a braided monoidal C^*-category with unit (the representation determined by ω is the unit) conjugates, ... ; briefly, a quantum category. From a combination of such tensor categories (left and right movers) one would attempt to reconstruct (symmetries of) *physical space-time.* String amplitudes would correspond to arrows (intertwiners) of the tensor category. There is a beginning of understanding how chiral (current) algebras and Riemann surfaces enter this picture. (See e.g. the second but last paper of Part VI, and ref. [51].)

Of course, this project has some loopholes. Assuming they can eventually be filled, it would provide a general way of thinking about string theory that does not presuppose knowing the target space-time of the theory. But one can, perhaps, not hope to extract much quantitative information from such an abstract approach.

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11 . The first two papers of Part III, "Gauge Theories, ... ", are concerned with the infrared problem and the construction of *charged states in four-dimensional quantum electrodynamics* (QED_4). These are problems which have intrigued me since the time of my graduate studies. The first paper, with G. Morchio and F. Strocchi, contains a general analysis with roots in work of Faddeev and Kulish, Zwanziger, Strocchi and coworkers and others, and in my Ph.D. thesis. Our results were later extended by Morchio and Strocchi and by Buchholz; see [60] for references.

In the second paper of Part III, with P.A. Marchetti, it is indicated how charged states can be constructed in four-dimensional abelian lattice gauge theories. Details of our construction and its application to spinor QED_4 on the lattice appear in a joint contribution to [60].

A comment on the rudimentary state of non-perturbative QED_4 is that we still do not have a non-perturbative understanding of radiation phenomena and resonances in systems of non-relativistic matter, such as atoms and molecules, coupled to the quantized radiation field, even in the presence of an ultraviolet cutoff. It might be a good idea to return to these problems which, after all, stood at the beginning of quantum theory.

Part III also contains a paper, the fourth one, reviewing ideas about *quark confinement* and the *roughening transition* in lattice gauge theory. It derives a random surface ("string") representation of lattice gauge theory based on joint work with B. Durhuus. These are problems that have been forgotten somewhat, in recent years, not because they are solved, but because they have turned out to be too hard or time-consuming for us physicists, who need the perception of steady progress and of changing fashions – spring collections of new problems and autumn collections of new results.

The roughening transition is, incidentally, another example of the phenomenon of symmetry enhancement (lattice symmetries enhanced to continuum symmetries) discussed in point 4, above.

The main justification for the inclusion of the last three papers of Part III is, perhaps, that they discuss concepts and ideas that may find new and interesting applications in condensed matter physics, as already remarked upon in point 9.

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12 . Part IV, "Triviality of $\lambda\varphi_4^4$", reprints a paper on the *triviality of* $\lambda\varphi_d^4$*-theory* in $d \underset{(=)}{>} 4$ space-time dimensions. The results established in this paper are similar to those proven, somewhat priorly, by Michael Aizenman [61]. My methods grew out of joint work with D. Brydges and T. Spencer on *Symanzik's polymer gas representation* of $\lambda\varphi^4$-theory which, with encouragement by J. Glimm and E. Nelson, we had adapted to lattice spin systems and field theories. We had shown that it can be used to prove, in a systematic way, many old and new correlation inequalities. The new inequalities in Aizenman's and in my paper made precise the intuition that two simple or (self) repelling random walks on $\mathbb{Z}^d$ (not starting at the same point), miss each other with probability one, in the scaling limit, for $d > 4$ (and for $d = 4$, in the case of simple random walks). The form in which this intuition is cast is an inequality on the connected four-point Euclidean Green function somewhat motivated by earlier conjectures of A. Sokal.

But what is the connection between two random walks on $\mathbb{Z}^d$ and the connected four-point function of $\lambda\varphi_d^4$-lattice field theory? With the help of a partially resummed version of Symanzik's polymer gas representation of (lattice) $\lambda\varphi_d^4$-theory one can express the connected four-point function as a sum over weights indexed by two random walks connecting the arguments of the four-point function and then estimate (using a correlation inequality) its absolute value in terms of intersection probabilities of the two walks. Another correlation inequality then bounds these intersection probabilities by tree diagrams with *coupling-constant-independent* coefficients.

It is shown in my paper that *one and two-component* $\lambda\varphi_d^4$*-theory, constructed as a continuum limit of ferromagnetic reflection-positive lattice* $\lambda\varphi_d^4$*-theory*, is necessarily trivial (Gaussian) in $d > 4$ dimensions, and that in $d = 4$ dimensions, and under the same hypotheses, the only *scale-invariant* $\lambda\varphi_4^4$*-theory* (ultraviolet fixed point theory) is the trivial (Gaussian) one.

One moral of these results is that, for renormalizable or non-renormalizable field theories, (renormalized) perturbation theory is slippery, as had been emphasized by K. Wilson and K. Symanzik, and that one needs more powerful methods among which Wilson's renormalization group plays the leading role, to come up with reliable predictions about their non-perturbative aspects.

In point 4, above, I have been careful in stating that B. Simon, T. Spencer and I have proven the existence of phase transitions and symmetry breaking for $\lambda\varphi^4$-theory in *three* space-time dimensions. Earlier, Glimm, Jaffe and Spencer had proven such a result for $\lambda\varphi_2^4$-theory with *discrete* symmetry [62]. What about $\lambda\varphi_d^4$-theory in $d \geqq 4$ dimensions? In 1982, A. Sokal and I have proven partial results in the direction of showing that in $d \underset{(=)}{>} 4$ dimensions the symmetries of $\lambda\varphi_d^4$ theory are either unbroken, or the one-point function ("magnetization') diverges in the continuum limit (if one imposes renormalization conditions keeping the two-point function well-defined).

All these developments, including a construction of $\lambda\varphi_2^4$ and $\lambda\varphi_3^4$ theory in the symmetric phase based on correlation inequalities and due to D. Brydges, A. Sokal and myself, besides many other topics in the theory of critical phenomena, are reviewed in much detail in a forthcoming book by R. Fernandez, A. Sokal and myself [63], where the reader will find lots of results and more references to the original literature than he may care for. I therefore do not wish to go into further details about Part IV, but see also the fourth article of Part VI. Nevertheless, one final comment should be added. The triviality results of Aizenman and myself have been applied to proving upper bounds on Higgs masses in the standard model. Actually, our results do *not* directly apply to exactly this problem; only a reasonable but *unproven* extrapolation thereof does. There would be a few interesting things to say about these matters which are not settled in a final way, but that would take us too far.

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13 . I come now to some brief comments on Part V, "Random Geometry".

The first paper, "Regge Calculus and Discretized Gravitational Functional Integrals", has not been published before, and, maybe it should have remained that way. It was written in the winter of 1980/81. I have made only very minor revisions since then. It is a piece of science fiction about the lattice approximation to

Euclidean gravity [64]. Of course, we do not really understand what "Euclidean gravity" means.

My preprint encountered some interest, and quite a few people asked for copies. Now it suffices to buy this book to find out what is in this article. It reviews, in a somewhat crude way, some results in combinatorial topology and geometry which are due to other people and states some conjectures which were then proven by some bright colleagues, in particular by Cheeger, Müller and Schrader and by Feinberg, Friedberg, Lee and Ren [65]. I suppose it poses some good problems, some of which are still open (e.g. the counting of the number of isomorphism classes of triangulations of, for example, the 3-sphere, or the 4-sphere, with N simplices, as N becomes large. Does this number grow like $const.^N$, for a sphere?).

Furthermore, I made some attempts to reconstruct a quantum-mechanical space of states with positive-definite scalar product from discretized gravitational functional integrals. The main idea is to assign to essentially combinatorial data associated with a manifold with boundary a vector in a Hilbert space with a scalar product that can be expressed in terms of discretized gravitational functional integrals. This is somewhat related to the "wave function of the universe", à la Hartle and Hawking, and to ideas in topological quantum field theory, in the sense of Witten [55]. But my construction was plagued with certain combinatorial ambiguities that could not be explained away, except by appealing to a "deus ex machina": universality. (My problems were most likely a consequence of not having isolated quite the right concepts. Perhaps, ideas and results due to M. Gromov would clarify the situation.)

In the same article, I also suggested that the incorporation of spinor fields in my formalism imposes strong constraints on space (imaginary) time topology if one insists on a form of Osterwalder-Schrader (reflection) positivity. (These constraints say *more* than that the second Stiefel-Whitney class must vanish.) Maybe this is worth looking into in more depth.

Nowadays, my paper is, at best, of some historical interest, apart from the circumstance that it draws attention to some problems that are still open and that I find somewhat interesting, and to some useful literature.

I have never become a professional in matters of quantum gravity, and this explains why my article and other dreams in this area (concerning Hawking radiation) remained unpublished. Furthermore, I did not pursue mathematical problems related to Regge calculus either. This was done by more competent people. I think that Regge calculus has not been a very useful tool in gravity theory, so far. However, ideas reminiscent of it (in particular, the work of Ponzano and Regge and of Hasslacher and Perry [64]) have recently proven, in the hands of Turaev and Viro [66], to lead to fascinating mathematics. Crudely speaking, what they have found is the following. Associate with each edge (1-cell) in a triangulation of a three-dimensional topological manifold (compact and without boundary) an irreducible object of a "rational" braided quantum category (e.g., an irreducible representation of $U_q(s\ell_2)$, with q a root of unity, or of a chiral current algebra, such as $\widehat{su}(2)_k$). Associate with the objects on the six edges of every tetrahedron a corresponding six-index symbol. Take their product over all tetrahedra in the triangulation, multiply by some phase factor, and then, for each edge, sum over all possible irreducible objects of the rational tensor category ("rational" means there are only finitely many irreducible objects, so the sums converge). Turaev and Viro show [66] that this produces a 3-manifold invariant related to (the modulus square of) Witten's invariants [55]. This is certainly a very beautiful result.

My attempts to understand Regge calculus turned out to have been a useful preparation for the study of *random surface theory.* My interest in random surface theory was triggered by the work of Tom Spencer and myself on the roughening transition in the SOS model, by the confinement problem in lattice gauge theory and by Polyakov's paper on string theory [67]. Moreover, motivated by remarks of Giorgio Parisi, M. Aizenman and I started to study plaquette percolation, a problem which in the competent hands of M. Aizenman, J. T. Chayes, L. Chayes and L. Russo turned out to have a number of fascinating features. The results of our efforts appear in [68]. The methods developed in this paper turned out to be quite useful and powerful in disordered-systems theory.

Subsequently, Durhuus, Jónsson and I became interested in the theory of self-avoiding and of planar (genus 0) lattice random surfaces. Our main motivation came from lattice gauge and string theory. For the simplest models of planar random surfaces, we were able to show, modulo a bound on a critical exponent only "verified" numerically, that the "mean-field" theory of Drouffe and Parisi becomes exact in dimension $d \geqq 2$.[2] It was felt (and argued) that this "collapse to branched polymers (or sea weed)" was related to the tachyon problem of bosonic string theory – which is likely to be correct. These developments are briefly reviewed in the second paper of Part V, "The Statistical Mechanics of Surfaces".

This paper – which was written in the spring of 1984 – is not only a review paper. It introduced some new ideas on, and some new models of random surfaces, most importantly the triangulated random surface models – which I viewed as decent, discrete approximations to Polyakov's functional integral formulation of string theory – and the *matrix-model formulation of triangulated random surface models.* I also suggested interpreting models of this type with zero-dimensional target space as models of two-dimensional gravity. The triangulated random surface models were then analyzed by Ambjørn, Durhuus, Orland and myself, and by François David, and later by numerous other people. Giovanni Felder and I spent some time studying the matrix model formulation of triangulated random surface models. But we did not find results that would have appeared to extend, in an interesting way, those obtained by Brézin, Itzykson, Parisi and Zuber, and by Bessis, Itzykson and Zuber [69], who had had different applications in mind.

Matrix models of random surfaces were then reintroduced and studied by V. Kazakov and A. Migdal, and by F. David, who eventually went considerably beyond what we had found.

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[2] Our result is an example of a "*theorem*" only proven to be *true with high probability.* Our arguments are rigorous but require an inequality, whose truth has been demonstrated numerically with a certain (presumably high) probability, but *not* with certainty.

My own interest in this subject had a payoff in mathematics. I told W. Thurston and R. Penner about the work of the Saclay group on matrix integration theory and that it had something to do with summing over Feynman diagrams and, in particular, triangulations of surfaces. R. Penner apparently found these remarks helpful in his work on the virtual Euler characteristic of moduli space [70].

Through their work, V. Kazakov and I. Kostov kept alife the physicists' interest in matrix models of two-dimensional gravity. In 1989/90, this led to beautiful work on non-perturbative two-dimensional gravity with important contributions by Brézin and Kazakov, Gross and Migdal, Douglas and Shenker, and many others. These developments are reviewed in [63], where many references to the original articles can be found (there are too many to list all of them here).

While I have some diffculties in understanding the relevance of the present results on two-dimensional gravity to physics, primarily because of the famous $d = 1$ barrier, they again triggered activities whose main payoff is in pure mathematics, at least so far: Witten has proposed a formulation of two-dimensional gravity in terms of intersection theory on the moduli space of curves and conjectured that his formulation was connected to matrix models [71]. This conjecture has recently been proven in beautiful work of Kontsewich [72]. These developments are likely to leave some traces in the mathematics of the moduli space of curves.

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14 . It may be worthwhile to ask what purpose a reprint collection like this may serve, with all the many gaps it leaves open. My hope is that it illustrates a few basic ideas and concepts in the subject of non-perturbative quantum field theory – of course the perspective is personal – but, primarily, that it might arouse the reader's interest in this important and still active branch of theoretical physics and guide him to *some* of the relevant literature (*not* to all of it, of course). The bibliographies of this introduction and of several of the papers reprinted in this volume should be helpful in finding one's way to some good literature on axiomatic quantum field theory, constructive quantum field theory, conformal field theory, gauge theory, quantum solitons, equilibrium statistical mechanics, renormalization group methods, and combinatorial-geometrical methods. The second and third papers of Part I, the first and second papers of Part II, the fourth paper of Part VI and the last two papers of Part VI may be particularly useful in this respect (besides the literature quoted at the end of this introduction).

Of course, many important topics in non-perturbative quantum field theory are not surveyed at all, and not adequately referred to. I believe that the four most important areas of application for field-theoretic methods are: gauge theories of elementary particles, the theory of critical phenomena in statistical mechanics, (many-body theory in) condensed matter physics, and astrophysics and cosmology. It is regrettable that none of these areas is discussed in sufficient detail, although glimpses are provided. Some topics are not touched upon at all. For example, one does not find anything about the fascinating area of exactly solved models in

$(1+1)$-dimensional quantum field theory (I have never worked in it). But, of course, this is not meant to be a textbook!

Although many of the papers reprinted in this volume are not very technical, they describe results which, by and large, have grown out of technical work, work carried out in the spirit and with the methods of mathematical physics. Mathematical physicists tend to be confronted with a discrepancy between results they would like to establish, because they would be physically relevant, and results that are accessible if one insists on mathematical precision. The reader is likely to encounter this discrepancy, but this is, perhaps, quite educational.

To be somewhat immodest, I think the reader may also find out that working in the spirit and with the methods of mathematical physics does not prevent one from sometimes being a little ahead of the crowd, idea and conceptwise. Some of the papers in Parts V and VI may illustrate this point.

Physics, including theoretical physics, and, in particular, quantum field theory have had considerably more brilliant periods than the present one. Many areas in theoretical physics which, in former times, were exclusive to people engaged in fundamental research are now in the hands of mathematicians, applied physicists or engineers. One may think of much of classical physics, quantum mechanics, nuclear physics, parts of condensed matter physics, optics, etc. Often our colleagues in other departments are more successful in improving our understanding of these areas of physics and making them applicable. The present situation appears to diminish the glamour of the job of a theoretical physicist proper and induces some people to conclude that support of fundamental research in physics should be reduced.

I am not pessimistic! I believe that physics, including classical physics, will remain a vital source of genuinely deep and important problems in basic research for a long time. We may not live through revolutions comparable to the ones in the first quarter of our century (relativity theory and quantum mechanics), or during the late sixties and seventies (gauge theories of fundamental interactions in particle physics, renormalization group approach to critical phenomena), every few years. But we can do important work even in the absence of such revolutions! And we may keep in mind that further revolutions will be necessary before we understand how events in space-time and the structure and dynamics of space-time itself and of interactions mediated by gauge fields emerge from a more fundamental quantum theory of nature whose basic formulation does not anticipate a model of space-time and of gauge fields, but will be able, at least in principle, to predict it. There is always reason to think that behind some steep mountains new horizons will open up.

In conclusion I wish to apologize to all those people whose important work is underrepresented, or even grossly underrepresented, in this book, even though it may be concerned with non-perturbative quantum field theory. Once again: this volume is neither a textbook, nor does it have scholarly intentions comparable to those of a textbook! It only reprints some of my (review) papers on quantum field theory. I hope it will be somewhat useful.

I do not think that anybody will read a long book like this from cover to cover. The idea would be that, by turning the pages of one or another section or paper, some readers will develop a little enthusiasm for the beauties of non-perturbative quantum field theory and some of the important problems left open.

I wish to thank all those colleagues, whose influence on me, guidance, inspiration, or collaboration with me has made this book possible! There are too many of them to thank them individually. Colleagues, who have been directly involved in work described in this volume, and to whom I am particularly indebted, are:
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