Foundations of Quantum Field Theory

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Abstract

A survey of nonperturbative and potentially rigorous definitions of quantum field theory, and the questions we would like to study with them.

Outline

Quantum field theory and string theory have had a significant impact on mathematics, and mathematics is essential in studying quantum field theory and string theory, as we have seen in every talk at this conference.

This leads to a natural question, which many have raised:

How can we make mathematically precise definitions of quantum field theory and string theory ?

There is a long history of mathematically precise approaches to quantum field theory, including

- axiomatic quantum field theory
- constructive quantum field theory
- algebraic quantum field theory
- functional integration and approximating expansions
- vertex algebras and conformal field theory
- probabilistic approaches, such as Schramm-Loewner evolution
- chiral algebras and factorization algebras
- topological field theory and higher category theory

In this talk, we will

- Discuss what ‘mathematical precision’ would mean in this context.
- Discuss questions for which this is clearly valuable, independent of methodological or sociological considerations.
- Survey some of the approaches, and discuss next steps we could take towards answering these questions.

One should probably regard mathematics and theoretical physics before the mid-19th century as subfields within a common intellectual movement: Newton, Laplace, Lagrange, Gauss *et al* laid the foundations of both subjects more or less simultaneously.

This changed, largely during the mid to late 19th century, for many reasons. Probably the most important of these were simply the overall increase in accumulated knowledge and in the number of practitioners, and the diversity of the developments, so that no one person could keep up with all of them.

Several more specific reasons:

- Development of mathematical rigor
- Acceptance of nonconstructive methods in mathematics
- Development of specialized techniques to meet the needs of engineers, scientists, etc.

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Although it is an oversimplification, many have pointed out that by the mid-20th century, this gradual divergence between mathematics and theoretical physics had grown into a chasm.

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> Res Jost, as quoted in *PCT, Spin and Statistics, and All That*, Streater and Wightman, 1964.

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We are used to the idea that mathematicians and physicists can write papers about "the same thing" which will read quite differently; in particular the mathematics paper will read "definition-theorem-proof" while the physics paper will be "discursive," arguing from example and intuition to make bold and often correct conjectures. In our subfield, this has been a very successful mode of interaction.

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Although we should continue this successful approach, I believe the time is coming to also revive the more traditional "mathematical physics" approach of taking the most important results, and making rigorous versions of them.
A well known argument for this is that this is the traditional definition of "mathematical precision" – people prove rigorous theorems, and get them published in math journals.

While this is a valid argument, I will focus on different arguments in this talk.
Indeed, many mathematicians say that proving theorems is *not* the central goal of mathematics; rather the central goals are **understanding structure** and **communicating this understanding to others.**
See for example *On Proof and Progress in Mathematics*, W.P, Thurston, math/9404236, Bull. AMS.

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Clearly both groups have something to gain from this project, but as a physicist, I will mostly try here to convince physicists that foundations can be valuable for us.

Some standard and well developed foundational topics in the literature:

- axioms for QFT and reconstruction theorems
- perturbation theory around Gaussian integrals
- perturbative renormalization theory
- quantizing gauge invariant theories: BRST, BV, etc.

Rigorous work has been done on all of these topics. More to the point here, mathematical work tries to address some central aspects of the problem:

- Geometry of space of fields
- Infinite number of degrees of freedom

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There are many topics for which the physics discussion is adequate, or for which the relevant math has been fairly well incorporated: much of supersymmetry, solitons and instantons, anomalies and index theory, fall into this category. Classical and one loop physics have good mathematical foundations.

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1. Rigorous use and justification of physics methods

This is of course the traditional goal and one can list many possibilities. Even Feynman agreed that the CPT theorem was a worthy result.

In my opinion, the most promising next goal (in terms of payoff divided by effort) would be to rigorously justify localization computations in supersymmetric field theories:

- topological quantities in $D = 2, (2, 2)$ SCFTs
- prepotential in $D = 4, N = 2$ super-Yang-Mills
- Wilson loops in $D = 3, 4$ (Pestun, KWY, *et al*)

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2. Connecting the various approaches to nonperturbative QFT.

As is familiar and as we will review later, a single QFT has many definitions: in terms of correlation functions, partition functions, etc.; using different regulators or UV completions; etc. Most but not all definitions involve taking limits of some parameter $\epsilon \to 0$, for example ϵ might be a lattice spacing, or it might be $1/N$ for a family of truncations to N modes.

A very standard claim is that two definitions agree after taking the limit, and that the corrections at finite ϵ can be controlled in some way. Despite the ubiquity of such claims, few of them have been stated precisely.

In functional analysis, the standard way to formulate such a claim is to use a norm on function space, for example the L_p norm

$$||f||_p = \left(\int_M |f|^p \right)^{1/p} . \tag{1}$$

This then defines a family of distances between functions,

$$d_p(f,g) = ||f-g||_p. \tag{2}$$

Of course there are many more such definitions.

One then states approximation results in terms of the norm, for example the speed of convergence of a lattice approximation or a Fourier mode truncation depends on the norm. Which norm one uses depends on what one wants to do with the function being approximated, *e.g.* is it a final result, is it part of an iterative or perturbative expansion, etc.

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The natural way to formulate the relation between different nonperturbative formations of QFT is also in terms of norms and metrics on the space of QFTs. An example in which this language is well known is the exact (Wilsonian) renormalization group. The RG acts on a dimension Δ coupling as

$$-\mu \frac{\partial}{\partial \mu} g_\Delta = (D - \Delta) g_\Delta + \mathcal{O}(g^2). \tag{3}$$

One would like to make precise (and then prove) the claim that the RG is attracted to a critical surface. This can be done by defining a norm on the space of Wilsonian actions, say

$$|| \sum_i g^i O_i || = \sum_i ||g^i||^{(i)}, \tag{4}$$

and showing that distances decrease. This was done in Polchinski (1985) to prove perturbative renormalizability, and nonperturbatively in some cases by Brydges and collaborators.

A problem with this definition of the distance between theories, is that it assumes we have a functional integral formulation, and that we can compute with it. It is also useful to have definitions which depend only on the correlation functions, or on other universal presentations of the QFT.

In 1005.2779, and in work to appear with Bachas, Brunner and Rastelli, we are developing other distances for 2d CFT. For example, for theories defined using BPZ data (dimensions and o.p.e. coefficients of primary fields), one can define a distance in terms of differences between this data. One can then ask "how good" is the approximation of truncating to a finite number of primary fields, and if there is an improvement procedure which would converge on the correct CFT correlation functions.

Using a variety of distances, we could systematize the discussion of approximate QFTs.

3. Proving that QFTs with specific properties do **not** exist.

Even the most basic questions here lack convincing answers. An example:

Can we prove that there is no interacting bosonic field theory in $D > 4$?

A standard answer to this is

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A good reason to reconsider the belief that bosons cannot interact in $D > 4$, is that we now believe that there are interacting supersymmetric field theories in $D = 6$.

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A good reason to reconsider the belief that bosons cannot interact in $D > 4$, is that we now believe that there are interacting supersymmetric field theories in $D = 6$.

We might start from a $(1,0)$ supersymmetric theory in $D = 6$, say the theory on an M5-brane near a Horava-Witten boundary. On compactification, this looks like a super-Yang-Mills theory with matter, and the vacuum structure can be understood using this picture.

Already in $D = 6$, we can give vacuum expectation values to the matter scalars to break the gauge symmetry ("go on the Higgs branch"). After compactifying to $D = 5$, we can add fermion masses, to get a bosonic theory which looks in the IR like a nonlinear sigma model with target space a moduli space of E_8 instantons.

Of course, the UV limit of this theory is not purely bosonic, so the original question was ambiguous. But under the reasonable interpretation that any unitary UV completion would be acceptable, the answer is: no, there are such theories in $D = 5$.

What about $D \geq 6$?

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4. Rough characterization of the space of QFTs.
I have given several talks on this topic, and will be brief here. A priori, it is clear that any definition of the "space of QFTs" requires choosing some precise definition of QFT. One then needs some ability to show that certain QFTs do not exist. The set of QFTs which do exist is then, in general, too complicated to ever find any complete description.
On the other hand, one can hope to get bounds and approximate descriptions. Some goals:

- Define a "central charge" in each D satisfying a c-theorem.
- Is the set of CFT_D's with $c \leq c_{max}$ and $\Delta_1 \geq \Delta_{min}$ precompact?
- Can the set all be described by flows from Gaussian or other known UV fixed points? Is there a bound on the number of fields needed to do this?

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Before surveying some of the existing approaches, let us raise the first question which arises in any approach, namely:

What data do we use to characterize the QFT?

The standard options from physics are

- Correlation functions of local fields
- S-matrix
- Linear operators on Hilbert space

From this point of view, the problem is to state axioms and construct models, beginning along the lines suggested by physics, but perhaps using new concepts and tools. For example, the translation between correlation functions, a measure on field space, and the operator interpretation, is surprisingly simple if the starting point satisfies the right axioms.

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Mathematics could also suggest other possibilities. While these will surely (?) have some physical or intuitive interpretation, it might be difficult to get results that way; rather one needs nontrivial mathematical facts and techniques to work with these possibilities.

We already see some of this in rational CFT. For example modular invariance is a strong constraint, which seems to draw its strength from analytic and even number theoretic facts, which do not have much to do with standard physics intuition.

Another fascinating example is the probability theory used in SLE.

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Axiomatic QFT addresses the questions "what is a QFT?" and "what is the relation between different presentations of a QFT." The standard answers to the first question include

- Wightman axioms (operator formulation of Minkowski QFT)
- Osterwalder-Schrader axioms (correlation functions in Euclidean QFT)
- Haag-Kastler axioms (operator algebras independent of representation)

These were proven equivalent and this justifies the usual focus on correlation functions in Euclidean QFT. There has been some work on generalizing this to curved space-time (*e.g.* Jaffe and Ritter).

We now have various other presentations, such as BPZ data or partition functions for a CFT, and it would be useful to prove equivalence theorems for these presentations as well.

There are two classes of QFT for which we can write down an explicit operator algebra. One is free bosons, fermions and abelian gauge fields in D dimensions, which realize the "CCR" (canonical commutation relation or Heisenberg algebra) and "CAR" algebras. Take space to be a manifold M, and time to be $\mathbb{R}$, with a product metric, then the free bosonic field is

$$\phi(x,t) = \sum_N a_N \psi_N(x) e^{-i\omega_N t} + a_N^\dagger \psi_N^*(x) e^{i\omega_N t} \tag{5}$$

in terms of the CCR and eigenfunctions of the (conformal) Laplacian,

$$[a_M^\dagger, a_N] = \omega_M \delta_{M,N}; \qquad \Delta_M \psi_N = \omega_N^2 \psi_N. \tag{6}$$

For $D > 2$, the free theories are not fully understood, in a general curved background or with general boundary conditions. How does one sew together general manifolds? The partition function $(\det \Delta_M)^k$ is modular invariant on T^D, how does this work? Does it have other interesting structure?

The other class is the solvable conformal field theories in $D = 2$ – minimal models, WZW models, orbifolds, Gepner models, etc. Most of these are rational CFTs and there is a highly developed mathematical formalism of vertex operator algebras (Frenkel-Lepowsky-Meurman, Borcherds, Huang, many others ...).

One can regard this as a deformation of the algebra of functions on the target space:

$$f(X(z_1))g(X(z_2)) \to e^{G(z_1,z_2)\frac{\partial^2}{\partial X(z_1)\partial X(z_2)}}(fg) \tag{7}$$

In the best understood cases (WZW models and minimal models), one can qualitatively understand the theory as obtained by a "RG flow" from a free field theory. Take the $SU(2)_k$ WZW model, it has $c = 3 - 6/(k+2)$ and the flow eliminates states from the free field Hilbert space (by null vectors and integrability conditions). A good deal can be understood by comparison to the large volume limit.

How different are orbifolds, Gepner models etc. from the large volume limit? Because the central charge is the same, there has to be some one-to-one matching between the states, and those of the large volume limit.

A conjecture: in the large volume limit, one has momentum states, winding states and oscillator states:

$$\mathrm{Z} = \left(\mathrm{Tr}\ \mathrm{e}^{\mathrm{i}\tau\Delta} + \sum_{\mathrm{l}} \mathrm{e}^{\mathrm{i}\tau\mathrm{l}^2}\right) \times \frac{1}{|\eta(\tau)|^{2\mathrm{n}}}. \tag{8}$$

While these dimensions will all get α' corrections, the structure of the Hilbert space 'remains the same' even deep in the stringy regime.

Rational conformal field theories are a very special case. For example, $(2,2)$ sigma models with Calabi-Yau target spaces come in families, parameterized by complex structure and "stringy Kähler structure." These are all SCFTs, but the subset which is rational is measure zero and probably not even dense.

As with all CFTs, the non-rational theories must satisfy the conformal bootstrap equations of BPZ (a generalized associativity condition). However there is no known way to get these down to a finite number of equations.

Another way to define these theories is to start at a rational point (say a Gepner point or orbifold) and add the marginal operators to the action which deform the complex and "stringy Kähler structure. It is believed that correlation functions are analytic in these couplings and that perturbation theory with respect to them is convergent. However this perturbation theory still needs to be renormalized, and has never been developed beyond low orders. Proving that it has finite radius of convergence would be a major advance.

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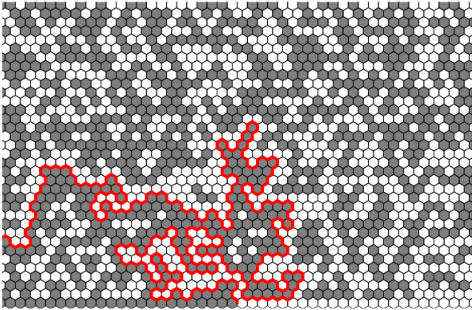
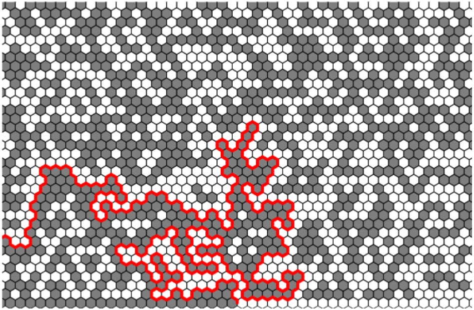
In recent years, two Fields medals have been given for rigorous results in statistical field theory, to Wendelin Werner in 2006 for work on SLE, and to Stanislav Smirnov in 2010 for proving that various lattice models have conformal limits.

Schramm-Loewner evolution (SLE) is a rule generating a ‘random curve’ in $\mathrm{D} = 2$, in terms of a Brownian motion. It describes many curves arising in 2d CFT, for example the boundary of a percolation cluster here.



In recent years, two Fields medals have been given for rigorous results in statistical field theory, to Wendelin Werner in 2006 for work on SLE, and to Stanislav Smirnov in 2010 for proving that various lattice models have conformal limits.

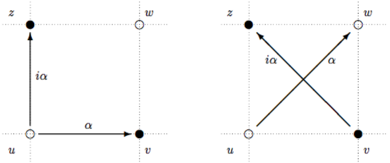
Schramm-Loewner evolution (SLE) is a rule generating a ‘random curve’ in $\mathrm{D} = 2$, in terms of a Brownian motion. It describes many curves arising in 2d CFT, for example the boundary of a percolation cluster here.



An example of Smirnov's work is his proof that the two-dimensional square lattice Ising model, in the limit of lattice spacing taken to zero, becomes the conformal Ising fixed point. The basic variable here is a free fermion and it was long known that a pair of free fermion operators correspond to a cut across which the Ising spin changes sign. But having analytic control over the limit, as opposed to taking the limit of exact results, seems to be new.

Discrete holomorphic functions:

$$\mathrm{F}(\mathrm{z}+\mathrm{ia})-\mathrm{F}(\mathrm{z}+\mathrm{a})=\mathrm{i}(\mathrm{F}(\mathrm{z}+(1+\mathrm{i})\mathrm{a})-\mathrm{F}(\mathrm{z})) \tag{9}$$



A good deal of mathematical work starts with the Euclidean functional integral. There is no essential difficulty in rigorously defining a Gaussian functional integral, in setting up perturbation theory, and in developing the BRST and BV formulations (see for example Kevin Costello's work).
A major difficulty, indeed many mathematicians would say the main reason that QFT is still "not rigorous," is that standard perturbation theory only provides an asymptotic expansion. There is a good reason for this, namely exact QFT results are not analytic in a finite neighborhood of zero coupling.

Fixing this problem has been the goal of a great deal of work. The most developed approaches are the subject of "constructive QFT" (Glimm/Jaffe/Spencer, Ecole Polytechnique group, Brydges et al, many others).

One solution to the problem is the "cluster expansion" (Glimm, Jaffe and Spencer, 1973). This can be explained by the example of ϕ^4 theory with action

$$S = \int_{\mathbb{R}^D} \frac{1}{2}(\partial\phi)^2 + \lambda\phi^4. \tag{10}$$

Note that even in $D = 0$, the λ expansion is asymptotic,

$$\int d\phi\, e^{-S} = \sum_{n\geq 0} \frac{(-\lambda)^n}{n!} \int d\phi\, e^{-\phi^2/2} \phi^{4n} \tag{11}$$

$$= \sum_{n\geq 0} \frac{(-\lambda)^n}{n!} 2^{2n-\frac{1}{2}}\, \Gamma(2n + \frac{1}{2}). \tag{12}$$

On the other hand, for $\lambda > 0$, the $\lambda\phi^4$ term in the action should make the integral *more* convergent than a Gaussian. If we could make use of this fact, we should get a convergent expansion.

In $\mathrm{D} = 0$, this is easy to do, by decomposing the integration region:

$$\int \mathrm{d}\phi\, \mathrm{e}^{-\mathrm{S}} = \left(\int_{-\infty}^{-\mathrm{C}} + \int_{-\mathrm{C}}^{\mathrm{C}} + \int_{\mathrm{C}}^{\infty} \right) \mathrm{d}\phi\, \mathrm{e}^{-\mathrm{S}} \tag{13}$$

for some $\mathrm{C} >> 1$. After expanding in powers of λ, the middle term grows no more quickly than $\mathrm{C}^{4\mathrm{n}}/\mathrm{n}!$, which is convergent. Thus this series can be summed and the limit $\mathrm{C} \to \infty$ taken. The other terms vanish rapidly in the limit, as exp $-\lambda \mathrm{C}^4$.

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Thus we make a *double expansion*, first splitting position space into a union of small cells X (say hypercubes), then decomposing the integration region for each cell:

$$\int d\phi \, e^{-S} = \prod_{X} \left(\int_{-\infty}^{-C} + \int_{-C}^{C} + \int_{C}^{\infty} \right) d\phi_X \, e^{-S} \tag{14}$$

Cells with $|\phi| > C$ get suppressed by the interaction, and the resulting series is convergent.

Unfortunately, the position space decomposition does not fit very well with gauge symmetry or supersymmetry, and the resulting expansion is a combinatorial nightmare. Still, these techniques were applied to rigorously define the general $D = 2$ Landau-Ginzburg model, and the ϕ^4 theory in $D = 3$.

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The $\mathrm{D} = 2$ multiscalar theory with a polynomial potential is a broad class of models. Furthermore the main difficulty in defining a $\mathrm{D} = 2$ supersymmetric gauged linear sigma model is to define the bosonic sector. The only divergences and renormalization arise in defining the interaction. These can be regulated by introducing a cutoff propagator $\mathrm{G}_\epsilon(\mathrm{x}, \mathrm{y})$ and defining a scale dependent normal ordering operation,

$$:\mathrm{V}(\phi(\mathrm{x})):_\epsilon \equiv \mathrm{V}(\phi(\mathrm{x})) - \mathrm{G}_\epsilon(\mathrm{x}, \mathrm{x})\mathrm{V}''(\phi(\mathrm{x})) + \ldots \tag{15}$$

While the limit $\epsilon \to 0$ formally exists, the problem with this is that, considered as a function of ϕ, the renormalized potential can become unbounded below. But, since $\mathrm{G}_\epsilon(\mathrm{x}, \mathrm{x}) \sim \log \epsilon$, this is only a problem on a tiny part of field space, which can be controlled using the kinetic term.

The cluster expansion can be applied with a wide variety of regulators. In more recent work (Brydges et al, Rivasseau et al, Feldman et al, Gallavotti et al), it is often combined with a "multiscale expansion," in which momentum integrals are decomposed into slices,

$$\int d^D p = \sum_I \int d^D p\, f(\lambda^I p). \tag{16}$$

This has been used to construct (for example) the $N >> 1$ $D = 3$ fixed point of the Gross-Neveu and $O(N)$ models, and to define a nonperturbative exact RG.

The cluster expansion can also be done on the lattice. Gauge theory is more easily defined on the lattice, and Balaban (1987) is often cited as the state of the art in this direction.

Supersymmetric theories can also be defined on the lattice. The best approach (as reviewed by Catterall, Kaplan and Unsal 0903.4881) is to twist the theory so that the fermions become lattice differential forms, and one nilpotent supercharge becomes a scalar.
This allows defining the A-twisted D=2 $(2,2)$ SCFT, and the $\mathrm{D}=4, \mathrm{N}=4$ SYM twisted so that the scalars transform as $4+1+1$ of $\mathrm{SO}(4)'$.

With AJ Tolland, we have been looking at justifying instanton computations in the $\mathrm{D}=2$ gauged linear sigma model, in other words making the computations of Witten 1993 and Morrison-Plesser 1994 rigorous. It turns out that a BPS instanton is a discrete holomorphic map satisfying $\mathrm{P}(\mathrm{Z}(\mathrm{z}))=0$ (modulo gauge equivalence). But since products of discrete holomorphic functions are not discrete holomorphic, these are hard equations to solve.

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The problem that perturbation theory is asymptotic is more general than any particular approach to perturbation theory (*e.g.*, Feynman diagrams). For example, recent work on $\mathrm{D} = 4, \mathrm{N} = 4$ gauge theory (BCFW, BCJ, Arkani-Hamed *et al*, Hodges, many others) has dramatically simplified the expansion – but presumably it is still asymptotic. How can we make sense of this theory?

In broader terms, the root cause of the asymptotic nature of perturbation theory is the *large field problem*: the interaction is a singular perturbation, in the sense that it qualitatively changes the large field behavior of the action.

Perhaps the most direct way to fix this is to consider theories without a large field region: fermions, or bosons with compact target spaces. It is well known that fermions are much simpler to define nonperturbatively (even interacting fermion theories). In quantum mechanics, considering compact target spaces completely eliminates IR problems. In QFT, perturbing around such a theory is believed to give a convergent expansion.

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The cluster expansion fixes the problem by decomposing configuration space into regions, a large field region in which $\exists x : |\phi(x)| \geq C$ and a complementary small field region, treat the integral differently, and add the results.

There is some analogy with the usual development of perturbative renormalization. There, we must also decompose configuration space into regions, a UV region in which $\exists p > \Lambda : |\phi(p)| \neq 0$ and the rest. One can think of the RG or a multiscale expansion as adding up integrals over these regions.

Perhaps by using a more geometric way of dividing up configuration space, in which changes of these decompositions correspond to precise algebraic operations (analogous to the BV formulation of string field theory), the cluster expansion would look simple and natural.

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This stuff [basics of Quantum Field Theory] is not impossible to learn; after all we teach it to physics graduate students in a year.

Edward Witten, from the 1997 IAS year on QFT.