Reconciling axiomatic quantum field theory with cutoff-dependent particle physics

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Abstract

The debate between Fraser and Wallace (2011) over the foundations of quantum field theory (QFT) has spawned increased focus on both the axiomatic and conventional formalisms. The debate has set the tone for future foundational analysis, and has forced philosophers to "pick a side". The two are seen as competing research programs, and the major divide between the two manifests in how each handles renormalization. In this paper I argue that the terms set by the Fraser-Wallace debate are misleading. AQFT and CQFT should be viewed as complementary formalisms that start from the same physical basis. Further, the focus on cutoffs as demarcating the two approaches is also highly misleading. Though their methods differ, both axiomatic and conventional QFT seek to use the same physical principles to explain the same domain of phenomena.

1 Introduction

Foundational invesitgation into quantum field theory (QFT) has emerged as a flourishing enterprise in philosophy of science, thanks largely to work done in axiomatic QFT (AQFT), particularly the C^*-algebraic approach enocoded by the Haag-Kastler axioms (Haag and Kastler 1964). Despite the methodological disconnect with 'conventional' approaches to QFT (CQFT), AQFT has been defended by Fraser (2009) as supplying a firmer foundation from which to conduct philosophical analyses. Though this is one of few explicit defenses of AQFT, the widespread use of algebraic methods in philosophical literature on QFT would lead one to believe that Fraser is merely making explicit the assumptions in her field. Recently, Wallace (2006; 2011) has questioned the focus on AQFT, arguing that CQFT is the better candidate for analysis. Since CQFT is the theory that has been emprically successful—the Standard Model of particle physics is built from CQFTs—and AQFT has yet to reproduce these results, Wallace argues that we should focus analysis on CQFT rather than AQFT. Fraser's (2011) reply has set up what is now known as the Fraser-Wallace debate over the foundations of QFT. The debate has set the tone for future foundational analysis, and seems to force philosophers to "pick a side"—you either work in AQFT or CQFT. The two are seen as competing research programs, and the major divide between the two manifests in how each handles renormalization. AQFT requires strict Poincaré covariance at arbitrarily small length scales, while the renormalization group (RG) methods in CQFT allow for a small-scale cutoff, below which QFTs needn't be well-defined.

In this paper I argue that the terms set by the Fraser-Wallace debate are misleading. One needn't view AQFT and CQFT as rival research programs; in fact, this view is detrimental to understanding the history and methodology of QFT. AQFT and CQFT should be viewed as complementary formalisms that start from the same physical basis. Further, the focus on cutoffs as demarcating the two approaches is also highly misleading: AQFT can accommodate cutoffs and RG methods, and CQFT does not explicitly require cutoffs. The focus on cutoffs as essential to CQFT could mistakenly be taken to mean that CQFT depends on cutoffs actually *being physical*, in the same way that cutoffs are physical in condensed matter physics (CMP). I will argue that this is not the case: cutoffs needn't be physical in any sense. Even if cutoffs are *physically significant*, that does not entail that the cutoffs are themselves physical. Specifically, RG methods provide no principled grounds for thinking that cutoffs are "real" in the sense of signifying a breakdown of field theories generally. Since Wallace (2011) set the terms of the debate, the bulk of the arguments in this paper will be in reference to that paper. I do not claim that Wallace holds all (or even most) of the views against which I argue; rather, I use his paper to clarify potential misconceptions that could arise from the debate. Renormalization is not central to the physical content of QFT, and the different ways of handling renormalization do not mark AQFT and CQFT as different research programs. We should instead view the formalisms as complementary: though their methods differ, both seek to use the same physical principles to explain the same domain of phenomena.

2 Renormalization and the relationship between AQFT and CQFT

Wallace (2011) emphasizes the ineliminable dependence on cutoffs in CQFT, along with the success of RG methods for providing a physical motivation for cutoffs, as the wedge which drives AQFT and CQFT apart. For Wallace, AQFT cannot deal with physical cutoffs. Since RG methods have physically legitimized cutoffs, AQFT and CQFT have differing physical content and must therefore be considered a different research program (2011, Sec. 2). I disagree with this characterization on two fronts. First, AQFT has the resources to incorporate RG methods when needed. Though typical axioms make no metion of scaling behaviour, even the most rigid of axiomatic approaches—algebraic QFT as codified in the Haag-Kastler axioms—can incorporate something like RG flows.[1] Second, the calculational dependence on cutoffs in CQFT may not signal the physical existence of cutoffs.

So, are cutoffs really that problematic for AQFT? Many axiomatic approaches to QFT make no recourse to cutoffs, either explicitly or implicitly. An explicit forbidding of cutoffs would mean that one of the axioms/postulates of the theory claimed that the theory is empirically adequate at all spacetime length scales. Even if any axiomatization contained such an axiom (none do), it would be hard to imagine what sort of work it would do in derivations. Presumably, such a system could be modified to remove the guilty axiom, without spoiling any physically useful theorems. One should therefore not be concerned with an explicit ban on cutoffs in AQFT.

The more interesting case is when cutoffs are implicitly rejected by a particular theory.

[1] See Buchholz and Verch (1995) for an example of scaling algebras playing the role of RG flows.

There are two common assumptions in AQFT that are problematic for handling cutoffs: strongly continuous implementations of Lorentz invariance, and the association of algebras with arbitrarily small open bounded regions of spacetime. Though the latter is not common to all axiomatic QFTs (the Wightman axioms deal directly with quantum fields, rather than algebras), the dominant axiomatization in terms of C^* algebras—the Haag-Kastler axioms—define QFTs in terms of algebras of observables corresponding to open, bounded regions of spacetime.[2] It is implicit that for any open bounded spacetime region, *no matter how small*, one can define an algebra of observables satisfying the other axioms defining QFT. If cutoffs are physical, one might conclude that there should be a principled limit to the size of regions on which we can define algebras corresponding to observables in QFT. If the cutoff scale is physically relevant, and only CQFT predicts its existence, we might be tempted to conclude that the two are different, competing theories. However, there are several possibilities for reconciling AQFT and cutoffs, which I will outline below. These remedies are largely independent of one another, and organized in terms of increasing foundational disagreement with Wallace's view of cutoffs. The "quick fixes" proposed first lead to further conceptual worries, and I therefore endorse the option in Sec. 2.3, which is the biggest departure from taking cutoffs as physical in CQFT. Nevertheless, all the options sketched below are more-or-less viable. Section 2.4 outlines reasons for thinking that *both* AQFT and CQFT suffer the same conceptual challenges if cutoffs *really are physical.*

[2]Since algebraic QFT is prima facie the most problematic, I will deal primarily with algebraic QFT in this paper. The reader can take AQFT to stand for axiomatic QFT or algebraic QFT for the remainder of this paper. The reader should also note that constructive QFT is another important strand of rigorous QFT. Though it is conceptually distinct from AQFT, the two projects often overlap.

2.1 Possibilities for cutoffs in AQFT

Just because we need to associate an algebra with any arbitrary open bounded region of spacetime, we are not therefore compelled to make this algebra interesting. One way that cutoffs could be introduced into AQFT is to specify that regions smaller than some 4-volume Λ are to be uniformly assigned trivial algebras, i.e., algebras containing only multiples of the identity. Such assignments would be consistent with the demand that all open bounded regions of spacetime be assigned an algebra, but it would make the cutoff physically relevant, since no information about local parameters would be contained in regions smaller than Λ.

Though this solution is available, it is admittedly somewhat ad hoc. Even worse, it violates one of the crucial Haag-Kastler axioms: that of weak additivity. The axiom of weak additivity states that, for *every* closed, bounded region $\mathcal{O}$ of Minkowski spacetime $\mathcal{M}$, the C^* norm closure of the algebras $\mathfrak{A}(\mathcal{O} + \alpha)$ for $\alpha \in \mathbb{R}^4$ is just the quasilocal algebra for the whole spacetime, $\mathfrak{A}(\mathcal{M})$.[3] There are two reasons why this is a problem for introducing cutoffs in the way described above. First, we run into the problem that the quasilocal algebra corresponding to the whole of $\mathcal{M}$ can be constructed from *any* algebra corresponding to *any* closed, bounded region $\mathcal{O}$. The norm closure of extensions of a trivial algebra will not produce any interesting algebra as a result, so regions smaller than the cutoff Λ will violate weak additivity. Second, extensions of an arbitrary region $\mathcal{O}$ by some $\alpha < \Lambda$ should not be physical if Minkowski spacetime breaks down at scales below Λ. In the spirit of the first ad hoc axiom modification, weak additivity could be modified to exclude regions $\mathcal{O}_{small} < \Lambda$, and arbitrary extensions $\alpha_{small} < \Lambda$. However,

[3]See Ruetsche (2011), especially chapters 4 and 5 for an introduction to algebraic QFT. For a more comprehensive review of algebraic QFT, see Halvorson and Müger (2007).

there seems to be no principled reason for choosing a specific value of Λ, and one may question the naturalness of such axioms. This makes the solution of simple axiom modification less tempting, and forces us to admit that AQFT—at least in its current guise—is in conflict with approaches to QFT that take cutoffs as physically meaningful, since the basic axioms are currently in direct conflict with the introduction of cutoffs. If we admit that there is currently no room in the formalism of AQFT for cutoffs, are we doomed to take AQFT as (incorrectly) positing its own validity at all energy scales?

2.2 No cutoffs? No problem

If QFT methods are only applicable up to some cutoff energy, and we expect QFT to incorporate this fact, we are saying that a good theory should signal its own demise. The formal necessity of cutoffs in the formalism of CQFT has lead to the idea that our best theories will continue to be an increasing hierarchy of effective field theories. Each field theory requires cutoffs to be implemented at a certain energy scale, and this signals the field theory's domain of applicability. If supplanted by a successor field theory, one expects that the new theory's low energy regime reduces to the old theory, and further that the new theory will itself have a higher energy cutoff. Following this approach, the conventional formalism of field theories would allow us to climb higher and higher up the ladder of energy scales, but we would never reach the top. We would require a theory of a fundamentally different formal type in order to end the ladder of cutoffs. This is presumably the view that Wallace holds, as he claims that if we replace one field theory with another applicable at higher energies, "that field theory in turn will need some kind of short-distance cutoff" (2011, p. 118).

As great as it may be to have a framework in which theories limit their own domain of applicability, this is certainly not a necessary condition that any good formalism need satisfy. Even if AQFT does not contain cutoffs explicitly, this does not make it at odds with CQFT. Many theories that have been useful in the past do not signal their ultimate demise; on the contrary, most are mathematically well-defined well beyond their domain of applicability. For example, classical theories of fluid dynamics treat fluids as classical continua, and these continua are uniform to arbitrary precision. Classical continuum fluid dynamics is a useful theory, and compatible with classical point mechanics, even though classical point mechanics leads one to believe that the continuum is only an approximation—at some point fluid dynamics must break down. There is nothing within the formalism of fluid mechanics that signals its eventual breakdown; rather, the physical systems we model using classical fluid dynamics, as well as the complementary formalism of classical point particles, give us a physical motivation for the eventual breakdown of the formalism. Deeper theories, such as quantum mechanics, also provide grounds for believing in the limited applicability of both of the complementary classical formalisms. Similarly, we can view AQFT as a complementary picture to the formalism of CQFT. Both formalisms rely on the same general physical principles, though they are implemented in different ways. Though the AQFT formalism does not demarcate its domain of applicability in the form of explicit cutoffs, the necessity of some form of cutoff in CQFT provides reason to believe that the AQFT formalism is only approximately mapping the actual physics. Further, whatever extratheoretical grounds we have for taking cutoffs to be physical—typically in the guise of speculative physics beyond the Standard Model—can inform the scale at which we lose faith in the predictions of *both* the AQFT and CQFT formalisms. When one does not view AQFT and CQFT as rival research programs, the two can work together to provide a deeper physical understanding of high energy physics, and the role of cutoffs is made clearer.

2.3 Physical significance versus being physical

Are cutoffs really that central? The arguments in the previous section assume that the cutoffs required to generate predictions in CQFT are physical, in the sense that they signal a breakdown of QFT. The fact that perturbative calculations within a particular model diverge when the integrals are unbounded does not entail that field theoretic methodology loses physical significance near these bounds. Undoubtedly we have extratheoretical reasons for supposing that the QFTs making up the Standard Model are not accurate to arbitrary energies—at some point gravity will surely play an important role, to say nothing for possible unknown physics at higher energy scales—but this needn't signify a breakdown of QFTs *in general* beyond a cutoff. Nor is this notion built in to the conceptual apparatus of RG methods, as Wallace claims.[4] It remains entirely possible that a QFT built with more terms in its Lagrangian could describe all relevant physics and be well-defined at all energy scales. In fact, the renormalization group procedure presupposes a theory given in terms of a Lagrangian or Hamiltonian with an arbitrary number of terms. These terms are shown to go to zero in the low energy limit (Wilson and Kogut 1974). We know—using the RG methods to determine the flow of coupling constants—that for non-Abelian gauge theories, interactions become weaker at higher energy scales. Total asymptotic freedom would be one way to eliminate cutoffs at

[4] "Wilsons explanation of the renormalisation procedure relies upon *the failure of the QFT to which it is applied* at very short distances. It is then intriguing to ask how to put on a firm conceptual footing a theory which relies for its mathematical consistency on its own eventual failure". (Wallace 2006, 34, emphasis added) Again, this passage can be read in a way that agrees with the arguments of this section. I am attempting to argue against a naive reading, which takes the failure of *one* QFT (i.e., a single form of interaction, encoded in a particular Lagrangian) to signal the failure of QFT methods in general.

high energies. A successor QFT, such as a grand unified theory or supersymmetry, could therefore unite the strong and electroweak coupling constants, while remaining well-defined to arbitrarily high energies.[5] All that RG methods rely on conceptually is the ability to average out behaviour at high energy scales, and this is compatible with many options for high-energy behaviour. First, our theories could be low-energy approximations that break down at higher energy scales. This could be due to a fundamental granularity or discreteness in the more fundamental theory, or due to the absence of terms in the Lagrangian modelling high energy dynamics. Second, we could have a well-defined high energy dynamics that is unimportant at the energy scales with which we are concerned. In any case, RG methods provide no principled grounds for thinking that cutoffs are "real" in the sense of signifying a breakdown of field theories generally. Unlike the breakdown of classical fluid mechanics—for which we have a more fundamental successor theory (quantum mechanics) providing grounds to reject the continuum as merely an approximation—there is as of yet no (empirically successful) fundamental successor theory for which QFT can be considered a continuum approximation.

One of the major reasons for thinking that cutoffs in QFT mark a regime beyond which the methods of QFT can no longer be applied is the success of RG methods originating from CMP (Wallace 2011, Sec. 1). RG methods were initially developed to investigate long range correlations in materials approaching a phase transition. Long range interactions are those most relevant to global transitions of a material, and so RG

[5] Whether a theory can be made well defined for arbitrarily high energies is a distinct issue from the accuracy of that theory's predictions at high energies. It may turn out that Standard Model QFTs can be extended in a consistent way, but that the high energy predictions turn out to be false. This is the case that is argued in Section 2.2 regarding AQFT.

methods average out the unimportant short range behaviour near a critical point. The apparatus of non-relativistic QFT (i.e., functional integrals using Galilean invariant Lagrangians) is used in CMP as an *approximation* to the discrete atomic (or ionic) physical makeup of bulk systems. Given the the CMP field theories are explicitly constructed as approximations to a known underlying lattice model, we know that the field theoretic methods must break down within CMP. RG flow equations are derived by separating field variables φ into low- and high-momentum components $\varphi = \varphi_{low} + \varphi_{high}$ (where the cutoff from low to high is chosen arbitrarily) and averaging over the high momentum modes. The resulting Lagrangian $\mathcal{L}'(\varphi_{low})$ is then manipulated to fall into the same form as the original Lagrangian $\mathcal{L}(\varphi)$. This process is repeated and generates discrete recursive relations between the rescaled coupling parameters in the $(n+1)$th Lagrangian in terms of the nth one. In the limit where the rescalings are continuous, these become differential equations determining the flow of coupling constants under RG. As the flows are taken to zero frequency—equivalent to the infinite spatial limit—only those parameters relevant to phase transitions will remain in the renormalized Lagrangian. One of the most qualitatively interesting features of successively averaging out short distance (and therefore high energy) degrees of freedom is that, no matter how complicated the initial field dynamics are (encoded as a Lagrangian), only the renormalizable terms will contribute to the low energy dynamics of the theory. This implies that a very broad class of higher energy Lagrangians can "reduce" to the relevant dynamics at lower energy scales.

The success of RG methods in CMP lead to their quick application in QFTs (Wilson 1983)[6], since the relevant formalism is shared between the two disciplines. If we choose

[6]Wilson even forms the QFT/statistical mechanics analogy explicitly, though the source analog in that

to endow the RG methods with similar physical significance in QFT, then we can interpret the high energy cutoffs required as marking the domain at which we expect new physics to occur. The problem is that, because RG flows tell us that our low-energy (effective) QFTs are largely insensitive to the dynamical details at higher energies, they provide little insight our guidance into the high energy physics. Though the path to the successor theory isnt apparent given our current QFTs, the up side is that our best QFTs are protected from the details of our ignorance of high energy dynamics.

Where Wallace might be read to err is in the jump from believing that cutoffs have physical relevance in QFTs to believing that cutoffs *are physical*:

> "This, in essence, is how modern particle physics deals with the renormalization problem: it is taken to presage an ultimate failure of quantum field theory at some short lengthscale, and once the bare existence of that failure is appreciated, the whole of renormalization theory becomes unproblematic, and indeed predictively powerful in its own right" (Wallace 2011, p. 119).[7]

The difference is subtle. Cutoffs can be *physically relevant* in that they signal the breakdown of the *particular* theory or model beyond a certain energy scale, but whether cutoffs themselves *are physical* depends on the precise nature of the breakdown. If the

case is a classical Ising model (Wilson and Kogut 1974). Fraser (2016) has provided an in-depth analysis of the elements of the analogies between QFT and the Ising model, as well as the process of describing RG flow.

[7]Or at least this is a jump he is sometimes guilty of. In other places he is more careful to elaborate on this view, and it appears that he at least appreciates the fact that field theoretic methods may not break down at all (Wallace 2006, pp. 43-4). As mentioned in the introduction, this paper is not a critique of Wallace's view explicitly, but of the misleading way of framing AQFT and CQFT as rivals based on their differing treatments of the arbitrarily small; for this reason I aim to clarify the mistakes in a "naive" reading of Wallace.

breakdown can be remedied by adding new terms in the Lagrangian—effectively changing the particular theory, but retaining the field theoretic framework—then the cutoffs signal new physics, but are not themselves physical. If the breakdown is due to the inapplicability of field theoretic methodology beyond that scale, then the cutoffs are themselves physical.[8] Even if one takes the cutoffs to have physical significance, cutoffs needn't *be physical* in this stronger sense.

One possible reason for thinking that cutoffs are physical is based off of reading too much into the analogy with CMP. We know that field theoretic methods are approximations in bulk matter systems—the atomic theory implies that macroscopic matter is composed of discrete components. The analogy between QFT and CMP is based on the use of the same field theoretic formalism in both disciplines, not on a well-grounded physical similarity.[9] Cutoffs are physical in CMP field theory because field theoretic methods have been introduced as an approximation. Given that discrete quantum mechanics of 10^{23} particles is intractable, we sacrifice (a surprisingly small amount of) precision in order to apply the more soluble methods developed in QFT. But the fact that cutoffs signal the breakdown of field approximations in CMP does not imply that the same is true in QFT. The reasons we treat cutoffs as physical in CMP are absent in QFT; there is no empirically successful theory that claims QFT breaks down due to an underlying discreteness of physics near cutoff scales. Speculative physics may posit some underlying structure for which quantum fields are merely an approximation,

[8] Presumably, the failure of field theoretic methodology in general would require some physical granularity at high energies. This is what I mean by the cutoff being physical and is in direct analogy with the case of non-relativistic QFT in CMP.

[9] Fraser (2016) and Fraser and Koberinski (2016) provide two concrete examples of fruitful formal analogies between QFT and CMP. In the former case, it is the RG flow that is formally analogous, while the latter deals with the formal similarities between spontaneous symmetry breaking within the two theories.

but until any of these theories make successful empirical predictions their significance for interpreting QFTs must be limited.

2.4 Why physical cutoffs are also a problem for CQFT

Even though, as I have argued, there is currently no physically motivated reason for supposing cutoffs to be physical, it may be the case that we find such a reason in the future. Perhaps we will need radically different methods from those of field theory to describe physics beyond the Standard Model. There is no shortage of candidates that claim to radically alter our picture of the world—from 11-dimensional string theory to discrete spacetime to the emergent spacetime of loop quantum gravity. Though experimental support for any of these speculative theories would mean that the axioms of any AQFT must be at best only approximations, this does not mean that CQFT would escape unscathed. Any observed violation of Lorentz invariance would signal bad news for both AQFT and CQFT, and the extent to which we choose to reject or salvage the former, we should do the same for the latter.

Though its importance is not encoded in a set of axioms, Poincaré invariance is of central importance to the physical content of CQFT. In constructing QFTs, one starts by writing down a classical Lagrangian to encode the physical content of the theory. The two major constraints on the form of candidate Lagrangians are renormalizability (dealt with above) and Poincaré invariance. Since the Lagrangian is a scalar, it must remain strictly invariant under the action of the Poincaré group on its component fields. All of the fundamental forces—as described by the Standard Model—are encoded in Lagrangians obeying strict Poincaré invariance. If anything qualifies as physically relevant to CQFT, the Lagrangian certainly does; it is the starting point for building a

QFT, and determines the types of fields, their masses, and the particulars of their interactions. A violation of Poincaré invariance at a more fundamental level—be it in a particular physical process or in the structure of some new spacetime picture—undercuts to the same extent the physical significance of *any and all* theories that depend on Poincaré invariance for their formulation. Thus, despite the lack of rigid and precise axioms demanding Poincaré invariance, the physical content of CQFT stands or falls with AQFT.[10]

Once again, the major difference between AQFT and CQFT lies in the formalism. Though the *physical* content of CQFT is built upon Poincaré invariance[11], the formalism is indifferent to the constraints placed upon the Lagrangian. The success of field theoretic methods in CMP is evidence of the flexibility of the formalism; in CMP the Galilean group is taken as the appropriate symmetry group, given the low energies dealt with. In contrast, the formalisms of various AQFTs are constructed around the axioms. Any theorems that rely on exact Poincaré invariance will only hold in the real world if nature is Poincaré invariant.[12] The greater precision of the formalism in AQFT makes it more rigid in this regard.

If violations of Poincaré invariance are problematic for all variants of QFT, should investigators into the foundations of QFT fret if such violations are experimentally

[10]CQFT *methods* could still be useful, but the theoretical framework of CQFT—as encoded in the Standard Model—depends on Poincaré invariance.

[11]Depending on how one views Poincaré invariance, this may seem odd. The specific transformation properties of scalars, vectors, and tensors under the Poincaré group are undoubtedly formal properties of the particular field representations. However, the physical symmetries represented in this way have a physical basis (e.g., rotation invariance implies that the physical system can be modelled the same way when rotated).

[12]Though it isn't always possible, proofs of the form "If Minkowski spacetime then x" are strengthened and made more robust by also showing "If *approximately* Minkowski spacetime then *approximately* x. Given that our best current theories lead us to believe that spacetime is only locally Minkowski, these are the results for which we can have a high degree of confidence in their robustness.

confirmed? No; the experimental success of QFT implies that the world is at least *approximately* Poincaré invariant, and any evidence revealing the limits of that approximation has no bearing on the theory itself. We have good reason to believe that the QFTs in the Standard Model are not the final story: General Relativity implies that strong gravitational effects distort spacetime, and that our spacetime is only ever Minkowski in small patches where gravity is negligible. Though this approximation seems to hold for experiments at the LHC, if we want a theory that gets spacetime symmetries *exactly* correct, QFTs relying on Poincaré invariance will not do the trick. Rather than abandoning foundations of QFT for being approximate at best, investigation should proceed given that QFTs are highly successful within the energy domain currently testable. To this extent, we are justified in viewing the world as approximately described by QFTs, and should content ourselves with investigating an incomplete (though highly accurate) picture of nature. Whether we are dealing with a formalism that encodes Poincaré invariance into its axiomatic framework, or a formalism in which Poincaré invariance has been used indirectly to construct empirically successful theories, we should not take violations of Poincaré invariance as signalling the failure of either approach. Any robust results obtained within either formalism will still hold approximately, and should be equally subject to foundational analysis.

3 Conclusions

I have tried to show that cutoffs do not provide physical grounds for separating AQFT and CQFT as rival research programs. First, RG methods can be incorporated into AQFT without major issue, and cutoffs can be introduced as well—though explicit cutoffs provide a more pressing conceptual revision to AQFT. Second, we needn't take AQFT to be an exact description of the world. In the same way that classical fluid dynamics is compatible with classical point mechanics, AQFT defined to arbitrary precision can be compatible with a CQFT that requires cutoffs. The apropriate lesson is that we should take AQFT to be approximately true in sufficiently low energy domains. Finally, even if cutoffs are of physical significance, they don't require a breakdown of continuum methods in general. This idea stems from pushing an analogy with CMP, which appears to be unjustified.

Though the Fraser-Wallace debate has spawned increased investigations into the foundations of QFT, it has set the boundaries of the debate in such a way as to create a false dichotomy: one is forced to choose whether to immerse oneself in the AQFT or CQFT formalisms. When we discard the false dichotomy and recognize AQFT as complementary to CQFT, we open the door to the synthesis of axiomatic methods with Lagrangian QFT. In this way the general features of QFTs can be investigated rigorously in AQFT, and we can be confident that—insofar as the axioms of AQFT capture the physical assumptions of CQFT—the results carry over to CQFT.

Though it is true that there do not yet exist AQFT models that incorporate interactions in four-dimensional spacetime, the successes of AQFT have been compatible with CQFT. Free field theories and ϕ_2^4 interaction theories constructed in AQFT give predictions in agreement with comparable CQFTs. Insofar as AQFT is a successful formalism, its results should be thought of as complementary to those of CQFT: one uses the same physical principles to construct differing formalisms.

In essence, I advocate for a position similar to Wallace's earlier view (though note that in this passage he refers only to specific results of AQFT, such as the spin-statistics theorem):

> the foundational results which have emerged from AQFT have been of considerable importance in understanding QFT and in general they apply also to Lagrangian QFTs. This paper should be read as complementary to, rather than in competition with, these results (2006, p. 35).

The particular choice of formalism will depend on the scope of the foundational investigation. If the goal is to prove general results applicable to any relativistic QFT, then AQFT is the appropriate formalism; if the goal is to determine the consequences of specific physical interactions, then CQFT should be used.

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