1. Overall assessment

This paper applies a relatively new approach to QFT, developed by Costello and his school, to an important class of theories, namely the generalisations of electromagnetism whose fields are connections on (higher) toral gerbes. To do this requires the author to extend Costello's approach in new directions and gives intriguing insight into the phenomenon of abelian duality, which has close relationships with important (and currently fashionable) topics like the geometric Langlands correspondence. Pending some revisions (suggested below), I think this paper decidedly deserves to be published.

The overall structure and style of the paper is nice; I will not explain what I like in detail. Let me focus here, then, upon questions that arose as I read the paper and upon aspects that I think could be improved. The length of my report indicates the depth of my interest, rather than anything problematic with the paper.

2. Comments on content

(1) It would make the paper more useable and easier to follow if the key results of each section were articulated as lemmas, propositions, or theorems. As an example, consider the final part of section 3 (on page 15). A key claim is that there exists a map of factorisation algebras, and the text below sketches a description. It would be clearer to state a lemma "The curvature map F determines a map of factorisation algebra f from ..." where the author clearly articulates the source and target. The material below could be organised into a proof. (I will point out other examples.)

Organising the key results in such a conventional format would allow the author to make cross-references elsewhere in the text (also useful for those who'd like to cite this work in the future). It would also sharpen the text considerably, since the reader would know what to focus on.

Key objects would likewise benefit from being labeled by definitions. For example, it is strange that there is no explicitly labeled definition of generalised Maxwell theory; it is left rather implicit in the text of section 3.1.1. It should be possible for the reader, inspired by Theorem 1.1, to flip through the text and find a careful definition of 'degree k generalised Maxwell theory with gauge group T.' As a more extreme example of the lack of clear definitions, the notation Ω^p_{cl} is never explicitly defined. As these are key notions, it's crucial that the reader can quickly find them when flipping around the paper as she actively reads (e.g., when double-checking something brought up by a later section).

Finally, key formulas would benefit from being labeled by equation numbers. For example, the evaluation map just below definition 2.13 is used at multiple points in the text but it's not easy to find its definition.

Let me mention that I'm not suggesting a massive overhaul of the text. I think the author should be able to split out some of the text that's currently embedded in discursive paragraphs. Below I'll point out locations where a modest restructuring would improve clarity and utility, but I also encourage the author to make a list of what he thinks are the main objects and ideas of the text and then see if they are easy to reference and given clear characterisations.

(2) This paper uses homological algebra throughout and invokes derived geometry repeatedly. It is strange, then, that the author uses (or at least seems to use) an underived version of 'closed p-forms.' Let me explain what I mean. There is a sheaf of p-forms Ω^p and a map of sheaves $d : \Omega^p \to \Omega^{p+1}$. The traditional sheaf of closed p-forms is the kernel of d as computed in the category of sheaves. This sheaf has interesting cohomology, which can be computed using a convenient sheaf of cochain complexes

$$\Omega^p \xrightarrow{d} \Omega^{p+1} \xrightarrow{d} \cdots \xrightarrow{d} \Omega^n,$$

where n is the dimension of the relevant manifold. This cochain complex is particularly nice because it is simply a truncation of the de Rham complex, so one can lean upon her knowledge of it. For instance, in each degree, one has a soft sheaf; similarly, the Poincaré lemma tells us that on a ball this complex has nontrivial cohomology only at the beginning. It is this cochain complex of sheaves that seems (to me) most natural for relating to generalised Maxwell theories, since it naturally encodes the derived global sections of the 'naive' closed p-forms and hence should behave better with other derived constructions.

Note that if closed p-forms means the traditional notion, then one cannot invoke Costello's formalism, since 'fields' for Costello always seem to be sections of a vector bundle; closed p-forms are not. Similarly, one cannot invoke the theory of elliptic complexes. If, however, one uses the resolution, then one manifestly has an elliptic complex and can use Costello's formalism.

I attempted to follow the paper's prescription for a free theory using the derived p-forms, and it seems that one gets something similar to the abelian Yang-Mills complex of example 2.22, except that one has a truncated de Rham complex up to $n-p$-forms on the top and a truncated de Rham complex from p-forms to n-forms, with a diagonal map given by multiplication by R^2. (I'm assuming the pairing is the L^2 pairing.) If this is correct, then it seems the derived version of closed p-forms could be analyzed in much the style of rest of the paper (and presumably Costello's work).

Carefully discussing the difference between the traditional and derived notions may also clarify or simplify issues around the linear dual to closed p-forms, as discussed in section 3.2. I would have expected that something like the derived (i.e., Verdier) dual would be relevant.

(3) It would be useful (but not absolutely necessary) if the author included several more of diagrams like that in example 2.22, so that there is no confusion

about what cochain complexes are in play. Consider including, for instance, a cochain complex for the derived critical locus of a generalized Maxwell theory. It would then be possible to explicitly describe the maps of fields between the p-form theories and the generalized Maxwell theories.

Related to this suggestion is a slightly subtle point here that the author never remarks upon. A map of manifolds does not induce a map between their cotangent bundles, nor for their shifted cotangent bundles. The author writes down maps of fields that are compatible with the action functionals, and he implicitly asserts that there are then maps between the derived critical loci, as modelled by the polyvector fields. But that would mean there is an underlying map between the shifted cotangent bundles of these spaces of fields. Hence the author needs to explain why things work out in this special situation, possibly by explicitly exhibiting the map between these models of the derived critical loci.

3. Comments by section

(4) The introduction should be revised to include a statement of what abelian duality means in physics. It would help the reader know what the author is trying to capture mathematically; it would also clarify whether the paper actually relates to the physical work. As it stands, the first paragraph mentions a number of papers and objects quite swiftly, but these theories and results are never discussed again in light of the formalism used in the paper. It felt like a reference dump with little function in the paper.

In general, there should be further discussion somewhere in the paper of how its results compare to prior work. Does it imply some of the results mentioned in the first paragraph? If it does, how? If it doesn't, how is it verifying abelian duality, in a sense shared by the community? Moreover, it seems to me that factorisation algebras bear a strong resemblance to algebraic quantum field theory, and so the author needs to give a substantive comparison with the recent work of Becker-Benini-Schenkel-Szabo; remark 5.12 is quite brief. (Also, the author seems to imply that he can recover the commutation relations for torsion operators, but does not seem to show it. It would be interesting to see. If he does not wish to explain, he should amend his phrasing so as to avoid insinuations.)

(5) The statement of Theorem 1.1 should include all hypotheses explicitly. For instance, what is R? Is the manifold X fixed and how does the expectation value depend on X? In fact, what does the author mean by vacuum expectation value in this context? What does 'can produce' mean? As written, the theorem does not read like mathematics.

Given the opening sentence ("In the context of these ..."), it is not clear if this theorem is the author's or simply a succinct codification of others' work. If it's the former, that should be made abundantly clear and it should be explained how it relates to the preceding work just referenced. If it's the latter, then the author should make that clear as well. (Given what happens in section 5, it seems to be the author's result.)

One approach to revision might be to spend the first few paragraphs giving a brief survey of abelian duality as it was understood before, with a loose statement of what abelian duality means in light of the existing literature. (It's not clear which things are dual. Two theories? Just certain observables in the theories? Or just certain expectation values?) The author would then proceed to emphasising the framework in which he's working (namely, factorisation algebras) and explain his mathematically precise version of the loose statement.

Let me remark that it would be good to explain what a 'correspondence of factorisation algebras' is, since he means something precise and not just the colloquial meaning of 'correspond'. In particular, it might be best if the statement included an explicit diagram for the correspondence, so it might be worthwhile to introduce notationally some of the other key objects, such as the free p-form theory. I suggest something like

> The 'curvature' of a connection on a higher torus bundle is a p-form, and so there is a natural map $F : \Phi_p^{Max} \to \Omega^p$ of fields. The Hodge star determines an isomorphism between p-forms and $n-p$-forms, leading to a diagram
>
> $$\Phi_p^{Max} \xrightarrow{F} \Omega^p \overset{\cong}{\leftrightarrow} \Omega^{n-p} \xleftarrow{F} \Phi_{n-p}^{Max}.$$
>
> Taking functions on space (here, spaces of fields) flips the directions of the arrows. Hence we have a diagram of commutative dg algebras
>
> $$Obs^q_{Max,p} \xleftarrow{F^*} Obs^q_p \overset{\cong}{\leftrightarrow} Obs^q_{n-p} \xrightarrow{F^*} Obs^q_{Max,n-p},$$
>
> also known as a correspondence of algebras.

(6) Section 2.2 is intriguing, and it should be treated with some care. There are a number of places where I found a definition or construction difficult to parse.

In definition 2.1, note that "algebraic group" sometimes implies a group object in algebraic varieties (and hence a finite-type scheme), which is not what the author means. Perhaps the author means "abelian group objects in schemes"? It should be made clear that these are very big spaces too, typically arising as mapping stacks. Definition 2.1 does not indicate how the smooth structure of the manifold X is related to the space of fields. In physics the fields are typically smooth sections of a fibre bundle over X; that data appears nowhere in this definition. Moreover, do we think of Dens as being a discrete abelian group or more like an affine space? This choice affects what kind of Lagrangian densities can arise.

In the following paragraph, it is not clear why there should exist compactly supported sections, or what that means. Presumably the author means that the identity of the group is the basepoint. The term 'naïve' struck me as pejorative; 'standard' might be more reasonable.

(7) The paragraph after remark 2.3 seems to play a crucial role in defining such theories. (It is confusing to use Φ again but in a different sense than definition 2.1.) It should be promoted to a lemma along the lines

> There exists a functor from the category $Sh(Open(X) \times Zar, sAb)$ of sheaves of simplicial abelian groups on the site $Open(X) \times Zar$ to the category $Sh(Open(X), sAbSch)$ of simplicial abelian group schemes on the site $Open(X)$. Given such a sheaf F, it assigns the sheaf whose value on the open $U \subset X$ is ...

The description of the construction is terse and hence hard to follow. Read strictly, it seems that for each U, one picks exactly one scheme $\mathscr{X}$. (Please don't use a symbol that looks like the manifold X; also how would one decide which scheme to use?) This cannot be what the author means. Next, the author says to take the colimit of Φ over a diagram given by the set (?) of maps of simplicial sheaves, but this colimit is supposed to be computed in the category of simplicial schemes, rather than in simplicial abelian group schemes. (Note that it is not explained why the mapping spaces have the structure of simplicial schemes.) This concerns me, as I do not see why this colimit should have an abelian group structure. (The coproduct *as a set* of two abelian groups is not naturally an abelian group.) Note that the term 'homotopy colimit' is used, indicating that the author does not mean 'sheaves' or categories in the standard sense, but in the sense of $(1,\infty)$-categories; the categorical settings should be explicit.

The author should make clear what he really means by this construction (and make sure it is meaningful). An explicit formula would help, as would an extensive explanation of the formula.

It would also be nice to have an easy example immediately following, so that the reader can verify her understanding. Please justify the computation of homotopy colimits!

(8) In example 2.5 the formula for Φ does not satisfy descent until it is stackified, I believe, due to the sum over P. This is a classic example for motivating stacks in the first place: one should construct a sheaf of groupoids when thinking about bundles over a space.

There should also be references for this example, justifying why this description is Yang-Mills theory, why the stated object defines a stack, etc.

(9) In example 2.6 I found the notation $id\log$ a bit distracting, since id is sometimes used for identity. Perhaps adjust the spacings and font?

Note that in the second line defining the smooth Deligne complex, the notation $\cong$ is misleading since it is a quasi-isomorphism and not an isomorphism of sheaves of cochain complexes.

It would be useful to introduce the notation Φ_p here and give an explicit description of the relevant cochain complex, as Φ is used in the preceding examples but nowhere here. (Φ_p only really appears in section 3.) It might be better, even to use Φ_p^{Max} and reserve Φ_p for Ω^p and Φ_p^{cl} for closed p-forms.

(10) Section 2.3 begins on a puzzling footing as it fails to define carefully some of the key objects. A key sentence begins "we define ... where the first line is only heuristic ...," which is extremely awkward since it is not then a definition. It is also strange to use $\cong$ in a definition. It would be clearer to write something like "We use the notation $T^*[-1]\Phi$ for the sheaf of simplicial abelian group schemes $T_0^*\Phi[-1] \times \Phi$, where $T_0^*\Phi$ means the cotangent complex of Φ at the identity element 0."

This interpretation is, unfortunately, undercut by the final paragraph of page 7, where the author reveals that $T_0\Phi$ is not the tangent complex of Φ, but a different cochain complex involving "tangent vectors with compact support" (presumably in X, which depends on an explicit differential-geometric model of the tangent complex).

Overall I would suggest the reorder the discussion to make clear what he actually wants to work with and to make obvious how and when he is abusing notation.

(11) The author never discusses what $\mathscr{O}(\Phi(U))$ means, except for the case that $\Phi(U)$ arises from a cochain complex of vector spaces. He ought to explain what he means when Φ is a smooth Deligne complex. Presumably it is a simplicial commutative algebra arising from a homotopy limit; it would be worthwhile to examine explicitly for U an open ball. (The comment on page 13 of 'aren't so easy to describe directly' can sound like a cop-out.)

I am also curious how he takes into account certain topological issues. According to remark 2.8, even the linear case involves using continuous linear duals. Hence he must not be working with the usual structure sheaf of algebraic geometry in the smooth Deligne case.

Let me note that here is another place where there is a notable difference between taking functions in the standard and derived sense. An abelian variety, such as an elliptic curve, as only constant global functions, but the derived global sections of the structure sheaf are interesting. Which does the author mean?

(12) It might be helpful to give more detailed references to the definitions of '(pre)factorisation algebra' etc on page 8, as I had trouble tracking them all down in [10].

(13) Regarding definition 2.14 I am unclear why $\mathscr{O}(\Phi)$ admits a filtration by polynomial degree. What fact about abelian group schemes is being used implicitly here? Is there a canonical choice for an abelian group scheme that is not an affine space?

(14) Section 2.5 implies that classical observables are not 'well-behaved' but does not explain what behaviour of Obs^{cl} is attractive or repulsive to him.

More importantly, it is misleading to say that his construction is the 'free' commutative dg algebra, since he modified the symmetric algebra to involve the completed projective tensor product. That is, his construction is not functorial (so far as I understand). This is especially notable in definition 2.19.

Section 2.5 ends before raising and addressing a natural question: how do the smeared observables compare to the unsmeared versions? This issue seems to resurface later.

(15) Section 3.1.1 should be revised to make it easy to cross-reference (e.g., make an identifiable definition of generalised Maxwell theory) and to see hypotheses (e.g., note that a Riemannian metric is chosen).

Definition 3.1 should be edited to indicate how a homotopy colimit appears. (Is this where the stuff near remark 2.3.4 is relevant.) It should be explained why this map is a generalisation of curvature in the conventional sense: unpack the case $p = 2$ explicitly.

I also found remark 3.2 a bit cryptic, particularly the use of the algebraic de Rham complex. Give a more careful formula.

The top of page 13 could be organised as a lemma: "The classical observables form a factorisation algebra given by ..." The author should explain *how* he knows $T_0\Phi_p$, i.e., how he computes a tangent complex in this setting. It would be nice to clarify the comment about the Poincaré lemma (mention the exact triangle of sheaves).

(16) In section 3.1.2 it would be worth remarking that it is a special feature of the theories discussed here that there is a map from Φ to $H^0(\Phi)$. There is no canonical map of complexes $C \to H^0 C$ if C has nontrivial terms in positive degrees.

(17) In section 3.2 it would be worthwhile to begin by sketching the quick physics of these theories. The fields are vector spaces (viewing closed p-forms as a subspace), and the action is a positive-definite quadratic form. Hence the critical locus is just an isolated point, namely the zero p-form, for both bases. (That is, just compute the Euler-Lagrange equation.) This makes clear why functions on the critical loci is just the constants. Then it is easy to follow the manipulations with factorisation algebras. (State that result as a lemma.)

It would be useful to note that this action is not well-defined on noncompact manifolds, but the factorisation construction is.

It would helpful for the author to show why and how smearing affects the observables of the closed p-form theory, perhaps by explicit example (e.g., on noncontractible open).

I'm confused about the claim that d^{-1} preserves the support of a form. That is a characterising property of differential operators, whereas this operator is, in essence, integration. Consider S^1 with the usual metric. The Hodge decomposition picks out the constant 1-forms (e.g., $c\,d\theta$, with c a scalar) as the harmonic forms and picks out the L^2 orthogonal complement as identified with the exact forms. A d^{-1} operator thus projects away the 'constant component' of a 1-form, which is given by the integral of the form over S^1. Hence, a 1-form with proper support and nonzero mass will be projected to an exact form with support everywhere.

More generally, those final paragraphs about the map of factorisation algebras deserve to be turned into a lemma with a careful proof.

(18) Section 4.1 should be reworked so that the important points are emphasised. Indeed, the main idea of the section is buried in the middle. It should be expressed as a lemma or proposition, with all hypotheses gathered in one place. (Note how they are spread out over several paragraphs at the bottom of page 16.) Something like this might be helpful:

> **Lemma 1.** *Consider a free theory on a closed (?) manifold X whose underlying elliptic complex has trivial cohomology. Then the global smeared quantum observables is quasi-isomorphic to $\mathbb{R}$ in degree zero.*
>
> Key for our work below is that the following notion is well-posed.
>
> **Definition 1.** *Under the hypotheses of the preceding lemma, the* expectation value *of a cocycle f in $Obs^{sm}(U)$ is the cohomology class of its image in $H^* Obs^{sm}(X) \cong \mathbb{R}$ under the structure map.*

It would be good to explain why this definition deserves the name 'expectation value' because a probabilist (or even physicist) might not recognise much similarity with the usual notion. Presumably there's some way to compare with section 2.1 and its treatment of Gaussian measures.

This section also contains some misleading statements. Let me highlight remark 4.4, however. As noted earlier, the closed p-form theory is not elliptic if the standard notion of closed p-forms is in use. (The sheaf of closed p-forms is not given by the sheaf of smooth sections of a vector bundle.) I also find it difficult to ascertain whether the theory of closed p-forms possesses no massless modes, since it is not clear how to apply some of the definitions in this case (as noted earlier). At the very least, the author needs to unpack the remark in much more depth.

(19) In section 5 I wish the author spent a moment explaining why he includes the adjective 'Fourier' before duality. It becomes apparent (if one is looking for an explanation) in the formulas near proposition 5.4, but it would be nice to

say more explicitly "this proposition justifies the use of the terminology 'Fourier dual,' as we show that $\tilde{O}$ agrees with a Fourier transform of O viewed as a function on the vector space of p-forms."

Note that the formulation of proposition 5.4 is unfortunate, as $\tilde{O}(a)$ does not equal the finite-dimensional integral, which involves a choice of k. It would be better to distinguish more carefully, e.g., by "Let $\tilde{O}_k$ denote the global observable arising from finite-dimensional Gaussian integrals:

$$\tilde{O}_k(\tilde{a}) = \frac{1}{Z_k}\int_{F^k} \cdots$$

where $\tilde{a}$ is an $n-p$-form. Then as k goes to infinity, the sequence of observables $\{\tilde{O}_k\}$ converges to the smeared observable $\tilde{O}$ defined by Feynman diagrams." The author might call back to the notation introduced in proposition 4.6, for the sake of clarity.

I also assume that the author means that for each fixed $\tilde{a}$, the sequence of values converges, since he never specifies convergence inside the algebra of observables.

(20) Section 5.2 should begin with a more careful discussion of the role of integral periods, so that the paper is more self-contained. (This issue is not brought up earlier but seems highly relevant to the behavior of the factorisation algebras.) I'm also unclear on what the integral over $F^k\Omega^p_{cl,\mathbb{Z}}$ means: How does the filtration by spectrum interact with the lattice? Does integration involve summing over the lattice?

This theorem does not make an explicit statement about Maxwell theories, but only about the theory of closed p-forms. Do we know that there are expectation values (in the factorisation sense) for the Maxwell theories? Do they agree with those for the closed p-forms? There should be a clearly stated lemma that is recalled here. The remark only notes that the observables have images in the factorisation algebra for Maxwell theories, not that the expectation values agree.

The role of the open set U also confuses me. In the statement of theorem 5.6, is it a hypothesis that U is contractible and hence diffeomorphic to $\mathbb{R}^n$? If not, what conditions apply to U? If so, then I find the proof confusing, as it involves a filtration that is not amenable to the manipulations used. (Smooth functions on $\mathbb{R}^n$ admit a continuous spectrum for a Laplacian, and the use of Hodge decompositions requires more care, since there is not a Hilbert space here.)

It would be nice to include, as part of the hypotheses or as a sharply stated lemma, exactly how much freedom there is in choosing a dual observable and whether it can be done uniformly. (Note the second bullet point at the top of page 24.)

(21) The notion of 'incident observable' deserves to be a stand-alone definition. It is not clear to me, however, how sensitive these observables are to the choice of torus and coupling constant. Do these actually see anything interesting about

the Maxwell theories? What observables are being missed? Does abelian duality only mean, for physicists, a comment about this class of observables? (A potentially small class, depending on the topology of the manifold.)

4. Typos, infelicities, etc

Systemic:

* The author switches between English and American spellings for the same work, particularly 'factori(s/z)ation.' It's distracting.

* The author seems to use the inequality sign $\leq$ for inclusion of vector spaces. This is not wholly conventional, and it should be remarked upon. It might be convenient to gather together notations someplace in the text.

* There are several places where 'Fourier dual' is used and where 'Fourier duality' would be more appropriate (e.g., in the abstract).

* Given the references to derived algebraic geometry, it is surprising that no citations are given for Toën, Vezzosi, Lurie, or a myriad of forebears and followers in this topic.

Introduction:

* A citation should appear for Batalin and Vilkovisky's work.

Section 2.1:

* In the line about $\mathscr{O}(\Gamma_{dS})$, the symbol $\sim$ is not defined. I presume it means 'quasi-isomorphic'; this notation strikes me as unconventional.

Section 2.2:

* In remark 2.7 'bEIng used here'

Section 2.3:

* In remark 2.8 does 'completed' mean the use of the completed projective tensor product or taking formal power series?

* On the bottom of page 7 the description of dS looks off to me. One is computing the Lie derivative of the whole density $\mathscr{L}$ at ϕ, not of ϕ itself.

* "by modifying the internal differential on ITS functions" (namely the shifted cotangent bundle)

* There's a missing period after 'usual Čech maps'

* Remark 2.11 is misleading. Colimits in the homotopy category do not agree (typically) with homotopy colimits, which is the construction used in [10], so far as I can tell.

Section 2.4:

* In defining the BV operator, it should made clear which degree (polynomial or cohomological) is being used.

* Definition 2.16, or at least the remark afterward, might deserve a more specific reference, as it seems to be a result.

Section 2.5:

* What does “completing the square (up to a constant factor)” mean? It took me a while to realise that the author was trying to deal with first-order and constant terms in a differential operator.

* In definition 2.18 it would be worthwhile to indicate that the $\vee$ indicates continuous linear dual (cf. remark 2.8) and hence is distributional in nature. Also, is the invariant pairing more data that defines a field theory or something auxiliary?

* Example 2.22.1 involves a choice of compact Riemannian manifold, so as to have the Hodge star. Presumably Δ means the Laplace operator for that metric. Examples 2.22.2,2 would benefit from stating the Lagrangian density and giving citations.

Section 3.1:

* On the bottom of page 13, what is the ‘usual sense’ of ‘gauge invariant degree zero observables’? Whose sense?

Section 3.2:

* What does “the classical BV operator is just a scalar” mean?

* What is a ‘subtheory’? Presumably the author simply means the fields are a subspace of the fields of the free p-form theory, and that the action is the restriction of the free action.

* The broken-out line following ‘we have local smeared quantum observables’ should have something like $Obs^q_{\Omega^p} = \cdots$, much as the classical observables in the preceding stand-alone line.

* There is a missing period after ‘complement to coexact forms’ on page 15.

Section 4.1:

* In the first paragraph, the invocation of Hodge theory makes me think that X is assumed closed. If so, make that explicit.

* It might be worth noting that $\mathscr{M} = \mathscr{H}_0$ in the notation just introduced.

* Remark 4.2 seems irrelevant to the paper. Moreover, it is incorrect: I looked at Costello’s definition and for him, a nonpropagating field is always a section of a vector bundle, whereas the massless modes are not given by sections of a vector bundle (if they are nontrivial).