Referee Report

SCIPOST Submission 202001_00038v1

The quasilocal degrees of freedom of Yang-Mills theory

Overview

This paper builds upon previous work of the authors on a field-space geometric approach to gauge theories, aimed at an analysis of regional decompositions of field perturbations and a splitting of degrees of freedom into "gauge" and "physical". In this context this manuscript extends previous work in the context of Lorentzian manifolds and focuses mainly on the behaviour of relevant data upon gluing of subregions in "phase space", keeping the symplectic structure under consideration throughout.

The manuscript (as part of a program that the authors admittedly started elsewhere) succeeds in presenting an interesting an innovative point of view on a purely geometric approach to Yang–Mills theory on a (d+1) dimensional Lorentzian manifold, and present a consistent framework to deal with gluing of data associated to subregions in the phase space of the system.

Additionally, by means of a choice of a connection defining a notion of horizontality in field space, the authors propose to match horizontality with the notion of physicality of degrees of freedom and explore the consequences of this choice upon gluing subregions. This choice appears to be guided by the existence of a natural metric on the space of field configurations, and hence a unique connection that is metric compatible. This is called Singer-deWitt connection, and it is used throughout in the paper.

The splitting into physical and gauge degrees of freedom induced by the connection brings up a natural question when it comes to gluing: whether this decomposition of fields is preserved upon gluing. The authors show that this is generally the case and argue what are the limitations of this statement, by a careful analysis of the gluing procedure, which is sensitive of global topological data and singularities in the group action (stabilisers). This comes with a nontrivial interplay of physical and gauge fields, that the authors argue can be taken as an explanation of several claims in the literature regarding the emergence of physical modes from gauge theoretic considerations.

The authors propose an analysis of reducible configurations that is interesting per se, and provides a good handle on this issue. All in all, the perspective on the problem that the authors propose is (to my knowledge) innovative and highly detailed, in that it provides a concrete method to explore the gluing of subregions.

I do think that the paper contains new material relevant to the understanding of regional properties of field theories (i.e. on bounded regions), and it provides an intriguing perspective that, to the best of my knowledge, has not been tested before (save on the author's previous works).

In its own, this paper is self contained and clear, making it a capstone summary of this program that, in principle, I could suggest for publication in Scipost.

I do however feel the paper falls short on a number of issues, mostly related to preciseness of statements and (sometimes) mathematical clarity. I have detailed my concerns below. Summarising and highlighting some:

1. I would like to suggest the authors pay attention to the comments related to their Section 2, where I believe their mathematical setup is imprecise (but fixable). This is especially important when discussing generalisations of this formalism, a few comments on which should probably be given somewhere.

2. I strongly suggest that the authors curb the paper from as much jargon as possible, and that keep mathematical statements clearly separate from physical interpretation. This is a recurring weakness that I feel should be addressed. There are a number of instances of undefined terminology used to draw conclusions. Speculative interpretations can be perhaps condensed at the end of sections. In particular the redundancy of specifiers like "horizontal", "physical" and "radiative" or "vertical", "gauge", "Coulombic" does not help the reader.

3. The issue of smoothness of fields in Section 4 requires more attention. See my specific comments about it.

4. Lastly, a few more words on comparisons with other approaches, especially when there are contradictory conclusions, should be given.

Introduction

The introduction is weak. Mostly because the terminology is not appropriately defined and non-standard. It would probably help to place section (1.3) at the top (right before the current 1.1). I understand that the authors want to prioritise physical considerations, but they have chosen a fairly mathematical approach and the paper should follow this choice consistently. Physical and speculative interpretation of statements can be dedicated various subsections, that stem on the rigorous results the authors have and put them in context.

Section 1.1

probably needs to be fully rewritten.

1. The notion of Coulombic is unclear and should be specified (it is done later in the paper).

2. What is a generalised Helmoltz decomposition (of what?)?

3. There is a lot of nearly-esoteric wording, which mixes interpretation of results with definitions. This should be curbed. An example: "The radiative dof are freely specifiable within R. Conversely, the Coulombic dof prescribe the (nonlocal) coupling of the fields in R to the physics outside of R; they cannot be tuned or affected from within R; their existence is due to the gauge nature of YT and o the ensuing Gauss constraint."

4. This is not a mathematical/rigorous statement and it is frankly unclear even after a very careful reading of the manuscript. Interpretation should be probably left for the conclusions (which are much better written). In general, the authors should aim for dry, self contained and rigorous statements, and only provide an interpretation. Ideally, I would like to know what it means for a "dof" (which I interpret as a point in the physical phase space) to be "freely specifiable",

and what it means to "prescribe the nonlocal coupling of the fields in R to the physics outside of R".

5. The remaining section 1.1 has further problems: it involves undefined objects, making the first read almost impossible to understand, and peppered with a number of interpretational comments which at this stage are neither understandable nor possible to argue with.

6. Last paragraph is also borderline esoteric. "complete ignorance of the field configuration outside of R is assumed" does not make perfect sense to me.

Section 1.2

1. Comment: why limitation to Cauchy surfaces?

2. Gluing of information on subregions taps into sophisticated methods such as Factorisation Algebras, Perturbative algebraic quantum field theory, etc. The authors should consider commenting on relevant literature here.

3. When mentioning the contradictions between the authors' work and other efforts in the literature, a more informative comparison between, say, gluing procedures, should be presented. If the contradiction is in mathematical statements, this can in principle be proven. If it is an issue of physical interpretation, then a comparison of frameworks (which I understand to be different) should be made.

4. The comment on charges $Q[\chi]$ representing a "signature of the physical nature of the reducibility parameter" is unclear.

Section 2

I have one main complaint here, which is that - as it is written - the mathematics seems incorrect/imprecise to me. The authors consider infinitesimal gauge parameters $\xi \in \mathrm{Lie}(\mathcal{G}) = C^\infty(\Sigma, \mathrm{Lie}(G))$, and then introduce a Lie algebra action on the space of connections $\mathcal{A}$. This is encoded in $\xi^\sharp \in T\mathcal{A}$, (the image of said action map).

Later, the authors turn to a more involved situation in which ξ is declared to be "field dependent". However, such a field dependent gauge parameter should live in $C^\infty(\mathcal{A}, \mathrm{Lie}(\mathcal{G}))$ otherwise d cannot be applied to it. So - to be clear - with how things are currently defined, formula (5) is incorrect. If $\xi^\sharp$ were a generic vector field on $T\mathcal{A}$ one could certainly take the Lie derivative of ϖ, but in order to express this in terms of an element $\xi \in \mathrm{Lie}(\mathcal{G})$, this would require the lie algebra action to be surjective - which in general is not the case.

What the authors *mean* is probably that one should consider a $\mathrm{Lie}(\mathcal{G})$ bundle over $\mathcal{A}$ (let us call it LG) which is trivial (in this optimal scenario), together with a generalisation of a Lie algebra action map $\rho\colon \mathsf{LG} \to T\mathcal{A}$ with the appropriate generalisation of the good properties that a Lie algebra action should have (a Lie algebra morphism with the space of vector fields). Then, the field-dependent object $\xi^\sharp$ would be the composition of a section of LG (which is not a vector field) with ρ.

Now, since the authors do not talk about this, it is unclear to me whether the formulas presented in this paper go through automatically or not. I am personally inclined to think that they should, but I would like to see a few more details, as there may be a number of subtleties. Since the authors are going for a mathematical approach to this, I strongly suggest to fix this issue. The advantage would be also that of connecting with other sophisticated languages that discuss field theory along the same lines.

I would like to point out in this respect that, while for gauge theories such as Yang–Mills a more naive approach is expected to work because symmetries are given by Lie algebra actions, in the much more involved case of General Relativity the picture is more complicated. The symmetries of induced metrics on a Cauchy surface are not straightforwardly given by 4-diffeomorphism, as the canonical structure involves field dependent Poisson brackets (see e.g. [1]). I do not know how the picture of principal bundle can be applied to this case, but a structure like the one suggested above can, so I suggest the authors to look into this.

Moreover, on the point of principal bundles, it seems to me that the authors do not really need a principal bundle structure (even more so as they never really consider the base of this bundle), but rather a foliation in field space. I suggest the authors to take a look at [2, 4, 3].

Section 2.2

1. When introducing the Electric field, it is not entirely clear to me if it is seen as an independent field configuration and to what extent. What about temporal derivatives of A? Are they considered independent of A on Σ? Irrespectively of temporal gauge, does it mean that E is simply *denoted* as $\dot{A}$? In general E should be unambiguosly defined somewhere. On Σ the time derivative of fields does not really make sense.

2. Once again the expression $\mathbb{E}(\xi)$ does not make sense if $\xi \in C^\infty(\Sigma, \mathrm{Lie}(G))$, which is the only definition given for ξ (see caption of Figure 2).

3. Defining $\Phi := T\mathcal{A}$ seems a bit redundant. I understand the authors later wanted to avoid writing $TT\mathcal{A}$, or identify it with some version of a phase space, but this is unnecessary notation, in my opinion. Moreover, the identification of pairs (E, A) with $T\mathcal{A}$ as opposed to $T^*\mathcal{A}$ seems rather arbitary to me. This is also not what arises naturally from the covariant phase space formulation of Yang–Mills theory, where the form $\int_\Sigma \delta A \delta E$ is the canonical symplectic form on $T^*\mathcal{A}$ (modulo the usual identification of E with the appropriate components of the field strength).

4. Again I do not think that the authors really need Φ to be a bundle anywhere (it certainly is a bundle on $\mathcal{A}$, but that's not what they mean). Probably a way of saying this is that the connection associated to a foliation of $\mathcal{A}$ lifts to a connection for the lift of the foliation to $T\mathcal{A}$.

5. What is the meaning of *fiducial* referred to a subregion?

Further questions:

1. Is ϖ assumed to be a local form on $\mathcal{A}$ or can it be any form (even nonlocal)?

2. What exactly is the advantage of considering field-dependent gauge transformations?

Section 3

1. The 1-form ϑ is called (pre)symplectic density. This is confusing in my opinion, as it is the *potential* of a symplectic form. It is not clear if (pre) refers to the possibility of $\mathsf{d}\vartheta$ being degenerate or to the fact that it is a potential. The name for ϑ subsequently changes twice by the end of the chapter.

2. When introducing the Kinetic supermetric it seems that this restricts the approach to theories that indeed depend on external data (such as a Riemannian metric). I believe this is a very important point that should be discussed. Do the authors expect to have a similar super metric in topological theories such Chern–Simons, or is it a unique feature of non-topological theories. This very observation might have some bearing on the results in this paper.

3. It would be useful to have a more systematic definition of what $\mathbb{G}$ is supposed to be, for other theories as well. What happens in first-order formulations?

4. Footnote 7 on page 15 seems important and could be promoted out of a footnote.

5. Of the claim following formula (37) should be given a sketch of a proof.

6. In formula (60), what is a time derivative in this setting? I am under the impression that to define a symplectic structure on Φ one needs to discard all time derivatives but one, which becomes E (plus corrections).

7. In Section 3.6.1 and 3.6.2. a characterisation/interpretation of the fields effectively comprised in the horizontal/vertical parts of the symplectic form is carried out in terms of quantities that are "known to be" physical. Is this not a circular statement? Physical fields have been defined to be horizontal wrt the (chosen!) connection. This connection has been chosen this way because it gave the "correct" notion of physical w.r.t. some standard notion. When measuring, surely there are observable quantities, some of which might be standard, but I believe this statement of physicality should be made "relative" to whatever empiric notion of physicality is decided to be. In other words, may it be that the relation holds at the level of moduli spaces of connections on $T\mathcal{A}$, the SdW being just one example (perhaps more canonical than others)?

8. In Section 3.7 the role of field dependent gauge parameters becomes prominent. I suggest the authors to adapt the results of this section to the necessary modifications of the general setting as mentioned above.

9. The third paragraph between equations (72) and (73) is unclear to me.

Section 4

Here I have a general complaint about the decomposition of smooth tensors that the authors consider from the outset. The introduction of theta functions clearly breaks down the smoothness of tensors, and I do not believe the overall space to be the direct products of smooth tensors decomposed as presented. This might have negligible consequences for the applications the authors consider, but a few more words would be useful. Typically, working with distributions is tricky because they do not behave well under pullback and restriction.

I also had some trouble understanding the assumption of global smoothness of glued quantities. If I understand correctly there is a statement of uniqueness, provided existence, but existence per se is not very well discussed. This is further confused by comments like point 2. in page 29 when the authors claim that the quantity H "is now guaranteed to be smooth at S". Later smoothness is replaced with continuity (see Proof 4.1.2). Clarifying more this point would improve the discussion, I believe.

Further comments on Section 4

1. In Section 4.2 induced supermetrics are introduced. This raises again the point of theories that do not depend on external geometric data (e.g. topological field theories). Is this procedure unavailable in that scenario?

2. Again, formula 116 and its context should be generalised to cases that are not Lie algebra actions, for induced symmetries on boundaries and boundaries of boundaries are not necessarily of this type.

3. Formula (126). Under this asymmetric gauge transformation, how is W gauge invariant? I am probably missing something, but a few words can be spent to clarify this.

4. Last paragraph of 4.3.1 is unclear. What is the statement behind this interpretation in terms of "magnetic monopoles"?

5. I find the second part of Section 4.4 a little bit unclear. The authors talk a lot about "knowledge", "having access" to regions and "affecting the physics" beyond a region. I would like if this statements could be made more terse and precise. The only point where this made sense to me was when discussing Green's functions. If this is what going on, I strongly suggest the authors consider making a dictionary between a precise statement and its interpretation in these terms. The concept of "summarising physics" is standard in effective approaches to field theory, but this use of the term seems to be slightly different. Or is this the effective result of integrating over field configurations on "inaccessible regions"? Is the "knowledge of f" more of a specification of a boundary condition or an effective action type of contribution?

6. I suggest the promotion of footnote 50 to an inline comment.

7. In formula (141) does the restriction mean pullback along the inclusion ι_S?

8. The last paragraph of Section 4.6 contains a lot of jargon, or undefined terminology that makes it obscure to a reader that is not accustomed. This is along the lines of previous comments I made. I suggest a clarification/rewriting.

9. Footnote 54: the first sentence is unclear, what does it mean to "glue at a specific value of the coordinate"?

10. In section 4.7.2. the authors claim that "topological mode[s] arise automatically and [..] solely from the interplay between gauge and topology". This is as opposed to what? The literature showcases several examples of treatment of cohomologies and (more generally) zero modes, all of which play a role in the gluing process (in fact many consider extensions of the phase spaces of topologically trivial regions, to accomodate for nontrivially glued manifolds).

11. The last paragraph in page 43 of 4.7.2 is similarly unclear and jargon-y.

Appendix D

Why is this an appendix? It appears to me that the results here are not published elsewhere and they seem relevant to the general discussion.

GENERAL QUESTIONS

1. Isn't the result on the rebalancing of energy distribution upon gluing a simple consequence of inducing inner products on a non-orthogonal splitting? What does it mean geometrically?
2. Ambiguities in the gluing reconstruction and zero-modes are addressed both in the language of Costello–Gwilliam and Cattaneo–Mnëv–Reshetikhin, a comparison with which might be useful.
3. How is it sensible to attach physical meaning to definitions that depend on an arbitrary choice such as the choice of a horizontal distribution. I understand that the SdW connection is "natural" to some degree, but I guess an important question the authors should address is whether anything is preserved under a change of connection. In other words, is there a meta-symmetry of the structure they are considering that makes (some of) the statements more invariant? Or is there a way to know what physical setup corresponds to a given abstract choice of horizontal subspace?

NOTATION

1. I suggest to change Λ^k to the more standard Ω^k, when denoting forms.
2. "Perturbative" is really not a happy terminology, as it is usually referred to perturbations in $\hbar$. I suggest that what the authors call "perturbations" be less ambiguously called "fluctuations", and that the setting be called something else than "perturbative". This goes together with referring to tangent vectors in field space as "perturbations". Once again, fluctuation is a less charged term, if a term is really needed beyond "tangent vector".
3. "Gauge fixing" is another charged term that should be avoided. I would suggest the authors to avoid this. Especially the combination "perturbative gauge fixing" that appears in section 1.3 (and elsewhere).

References

[1] Blohmann C., Fernandez M. C. B., Weinstein A., *Groupoid symmetry and constraints in general relativity*, arXiv:1003.2857 [math.DG]

[2] Blohmann C., Weinstein A., *Hamiltonian Lie algebroids*, arXiv:1811.11109 [math.SG].

[3] Koike N., *Foliations on a Riemannian Manifold and Ehresmann Connections*, Indiana University Mathematics Journal Vol. 40, No. 1 (Spring, 1991), pp. 277-292

[4] Lazzarini S., Masson T., *Connections on Lie algebroids and on derivation-based noncommutative geometry*, arXiv:1003.6106 [math.DG]