1 SciPost Report 1 on Part I from Anonymous

This article describes and extends in a very useful way an approach to string perturbation theory and equations of motion by A. Tseytlin. There are a number of valuable insights in this paper and the article should certainly be published.

With this said, I would add that there are many places where more work in the exposition would make the article more valuable. I will make a few illustrative remarks, but there actually are many more places where I thought the article would benefit from a slightly more detailed explanation.

Starting on p. 6, the authors want to replace the operation of dividing by diffeomorphisms and Weyl transformations by an operation of gauge-fixing the Weyl group and dividing by diffeomorphisms. In order for this to make sense, the gauge-fixing of the Weyl group has to be diffeomorphism-invariant. I do not see that the authors quite say this, and I found the discussion of the redundant Weyl parameter $\bar{\omega}$ in the second column of p. 6 to be obscure and (therefore) not convincing. I suspect this was an attempt to avoid saying that the Weyl gauge fixing should be diffeomorphism-invariant, but I am not sure.

As a quick aside, the argument involving $\bar{\omega}$ was not intended as a way to circumvent the issue involving Diff-invariance. Its function was simply to clarify that our protocol is isomorphic to the usual on-shell gauge-fixing procedure, in which one starts with a partition function which is both Weyl and Diff invariant, and then both of them are fixed (and so we needed a dummy variable for the Weyl to act on). We agree that a more direct procedure (forthcoming in [91]) will probably be found more transparent and convincing by the string community, but here there is an issue of academic credit, as this work was done by Prahar Mitra, which is why we didn't wish to simply incorporate his argument directly into our draft.)

One obvious diffeomorphism-invariant gauge-fixing of the Weyl group is to ask that the worldsheet should have a metric of constant curvature with some specified volume. The authors actually use this gauge-fixing in much of the article (for the case of genus 0 where the constant curvature metric is a sphere of some radius). In that context, the authors' explanation in section III.C that the chosen volume (or radius, in the case of a sphere) does not matter up to a reparametrization of the field variables (i.e. the coupling constants of the 2 d model) makes sense and is correct.

However, a more complicated diffeomorphism-invariant gauge-fixing of the Weyl group is also possible, where over the moduli space M of Riemann surfaces of some given genus g, at each point in M one fixes in an arbitrary fashion a distinguished metric in its conformal class (one presumably would do this in a way that varies smoothly with the point in M). Then any metric g determines a conformal structure and hence a point in M and one would replace g by the distinguished metric in its conformal class. Do any of the arguments in the paper require such a more general gauge-fixing? I had trouble understanding this and I do not think this is clear in the way the article is written. For such a more general diffeomorphism invariant gauge-fixing I think there would be a difficulty, which is that the argument of section III.C that the choice of Weyl gauge fixing is irrelevant up to a redefinition of the field variables does not work for this more general kind of Weyl gauge-fixing.

The referee raises important concerns about two sections. In the previous draft, we briefly mentioned in a footnote that we are confining attention to Diff invariant gauge-fixings. But we have substantially revised the manuscript in parts III.A and III.C to clarify these issues more explicitly.

Regarding part III.A, we now give an explicit definition of what we mean by a Diff-invariant gauge-fixing, in a way that is not dissimilar to what the referee suggests. However, our approach now makes explicit that there are many other possible ways of doing this besides the constant curvature choice. As indicated by the new footnote 14, there are no substantive restrictions on which metric g we can select in each conformal class, as we are only interested in Diff equivalent classes of g, which can always (by definition) be described in a coordinate-invariant way.

However, this does not by itself address the concern with III.C, because, even once we have committed to describe everything in a Diff-invariant way, it is still necessary to explain why doing a nonuniform Weyl rescaling (that is not constant in space) is permissible given that the worldsheet couplings are required to be constant on the worldsheet. We have explained why this works in the text surrounding the new Eq (21) and (22).

It turns out that the way things work for a uniform Weyl rescaling is quite misleading for how things work in the general case. For constant ω, the field redef required is simply β itself. For non-constant ω, one has to do something completely different and re-express β in terms of the equations of motion, and then use the coefficient which multiplies *that* to identify the correct field redefinition. Quite nontrivially, this gives you a uniform field redefinition whose effects are the same as the naive "local RG" nonuniform redefinition. (This really depends on the string action giving the correct equations of motion, it isn't just trivially implied by the existence of a local RG flow.)

We thank the referee for bringing up this point as we did not understand the issue as clearly, when we submitted our previous draft. Even though the previous argument in III.C was technically valid as written (and remains in the draft surrounding (18) and (19)), we believe our additional exposition explains what is going on much more clearly. We have also rewritten the summary paragraph in light of our new understanding.

(It is true that when it comes to doing concrete calculations in sections subsequent to III, we mostly just use the uniform sphere choice. But we thought that in section III, it was conceptually important to explain why the off-shell formalism should be independent of this choice.)

(Though this is possibly irrelevant if it is true that the authors never need the more complicated type of Weyl gauge-fixing described in the last paragraph, it is possibly worth comparing to other approaches to string perturbation theory.

In conventional "on-shell" approaches to computing the perturbative string S-matrix, when computing S-matrix elements involving massive states - or any states whose masses are renormalized - it is actually necessary to go slightly off-shell to deal with mass renormalization. The procedure for doing so has been explained in most detail by A. Sen. Sen explained that one has to make a Weyl gauge-fixing of the more general form indicated in the last paragraph [something less than a complete Weyl gauge-fixing is enough: one only needs to fix the Weyl gauge near the positions of vertex operator insertions]. Then to prove that results do not depend on the choice of the Weyl gauge-fixing one uses the BRST machinery. The BRST machinery is not used in the paper under review here, and as I've noted, their RG argument to explain that the choice of Weyl gauge-fixing does not matter does not appear to apply to general diffeomorphism invariant gauge-fixing procedures. I am a little skeptical that the tools they are using would be adequate to deal with the more complicated Weyl gauge-fixings because a general change in Weyl gauge-fixing could not be compensated by an RG flow, and because they do not use the BRST formalism that was important in other approaches. But as I have indicated, it is not clear to me if this is relevant.)

Thanks for the explanation. We agree the approach involving BRST is valid although we do not use it here. See the previous reply for why what we do is valid.

I did not find section IV.D entirely convincing, because holomorphy in the parameter ϵ of the hard disc cutoff wasn't clear to me, and I had no intuition about what happens when $\log(1/\epsilon)$ is imaginary and large. The same remark applies to some later parts of the paper that refer back to this discussion.

Regarding the analyticity with respect to log epsilon, we have argued on physical grounds that changing epsilon is equivalent to changing the length of worldsheet tubes, and these tubes were shown to have the necessary analyticity properties in Witten [15]. We suggest looking at this paper to get more intuition regarding this limit.

In footnote 36, we explicitly state our assumption that "the RG flow is analytic, as it is in perturbation theory". This is true because, at any finite order in n of conformal perturbation theory, the beta function flows are polynomials in the coupling constants, and hence the RG flow of the coupling constants can be perturbatively integrated along the flow in terms of powers and logs; hence the coupling constants should be an analytic function of epsilon, at least at finite order. (Here we are allowing for the possibility of branching sheets when the RG is plotted in terms of epsilon, i.e. there is no requirement that the function go to itself when the phase of epsilon is rotated through 2π.)

I am embarrassed to say that I did not understand where the e^{-2T_0/ϵ^2} comes from in eqn. (105) [now (107)].

This is simply the partition function of a theory whose only term is a 2d cosmological constant. We have explained this more clearly in the revised manuscript, and also fixed some typos.

While the normalization factors are purely conventional, there was a consistency issue between different equations, that we have now corrected (hopefully in a way that matches standard conventions). There is now a $1/4\pi$ in the new (106) to cancel the unit sphere volume, and the tachyon partition function is now e^{-T_0/ϵ^2} (without the 2 in the exponent) which changes the numerical coefficient of (109).

Jumping to p. 31, I asume $\phi_{\mu\nu}$ is meant to be specifically a graviton mode rather than a more generic string mode, but this isn't stated very clearly. I cannot see where the authors define $\widetilde{\Phi} = \Phi - \frac{1}{4}\log\det g$, though various expressions involving this quantity are written.

We agree it is valuable to write this explicitly. In fact, as certain aspects of this section were unclear, we have completely rewritten this section, starting just before the new (150) [the requested equation]. In the new section, we refer to the quantity above as $\hat{\Phi}$, since its equivalence to $\tilde{\Phi}$ is true only in a certain gauge choice (implied by transverse gauge), for reasons discussed in the new text.

In eqns. (149)-(152), I think these formulas would be clearer if one writes what is being kept fixed in these variations, for example $\delta\widetilde{\Phi}\Big|_{\phi_{\mu\nu}}$, etc., assuming this is what is intended.

We agree this notation is clearer. While the section has been rewritten, we have followed this suggestion whenever we write down partial derivatives wrt the target space fields.

In footnote 93 , should Φ be $\widetilde{\Phi}$?

In fact if we hold the metric fixed, these variations are equivalent. But we agree that the use of $\tilde{\Phi}$ would more clearly connect to the statements in the main text, so we have made the change (in what is now footnote 96).

Is eqn. (153) obvious?

Eq. (153) [now (163)] was derived in section VI.F. We will add a cross-reference.

I think the c-theorem is significant enough that eqn. (160) and the following discussion deserved a more thorough explanation. First of all, one could elaborate on (160):

$$\frac{\mathrm{d}I_0}{\mathrm{d}t} = \sum_i \frac{\partial \phi^i}{\partial t}\frac{\partial I_0}{\partial \phi^i} = -\sum_{ij} \kappa_{ij}\beta^i\beta^j$$

Here t is renormalization group time. As the authors indicate, this would prove monotonicity of I_0 under RG flow if κ_{ij} were positive definite. In fact, for the on-shell modes, κ_{ij} is positive-definite except for the single mode $\widetilde{\Phi}$. Therefore, we want to eliminate $\widetilde{\Phi}$, which we can hope to do by extremizing I_0 as a function of $\widetilde{\Phi}$, keeping the other ϕ^i fields fixed. (The extremum does not exist because $\widetilde{\Phi}$ would flow to infinity; we will correct for that in a moment.) Assuming the extremum exists and we always evaluate I with $\widetilde{\Phi}$ at the extremum, when we evolve in t, the ϕ^i will all change and in particular $\widetilde{\Phi}$ will change. Hence $\mathrm{d}\widetilde{\Phi}/\mathrm{d}t$ is no longer given by a beta function and hence the contribution to $\mathrm{d}I_0/\mathrm{d}t$ involving the change in $\widetilde{\Phi}$ when t changes is modfied from what I wrote above. But as we are always evaluating I_0 at a value of $\widetilde{\Phi}$ such that $\partial_{\widetilde{\Phi}} I_0 = 0$, this contribution is actually replaced by 0 . So we would get monotonicity - if we could extremize I_0 as a function of $\widetilde{\Phi}$. But as I noted, the extremum does not exist. We deal with this by using the existence of the function V defined in eqns. (162-3) that depends on $\widetilde{\Phi}$ but not on the other ϕ^i. The ratio I/V does have a unique extremum (actually a maximum) as a function of $\widetilde{\Phi}$ with the other fields fixed (as an aside, this fact is widely used in the mathematical theory of the Ricci flow - for example, see hep-th/0510239 by Woolgar et. al. for a nice review, where a simple proof is given that a unique maximum exists, in the case of a compact target space; I think the proof extends nicely to the noncompact case). And now the argument can be made correctly for I/V : it is decreasing under RG flow. In this paragraph I have simply filled in some gaps in the argument on p. 32.

We thank the referee for these comments, and we have rewritten the latter half of VI.D significantly in light of the proposed suggestions. This includes moving some material that was previously in VII.B to this section, in order to give a more cohesive account. Discussion of Woolgar et al papers have been included.

In point (2) on p. 34, I find it unnerving to be told about an "interpretation" of eqn. (170), which seems to acknowledge that eqn. (170) wasn't clearly defined at the outset. If possible, one would prefer to define (170) properly when it is first written.

All we meant here is simply the use of the definition of the stress-tensor, in terms of differentiating with respect to the background metric. But we have rephrased this, and added a new equation (181) to explain the maneuver.

I realize though that a proper explanation of this derivation is being deferred to the second paper.

The citation is to a 3rd paper which is still in progress, not to part II.

Speaking of that, is the last sentence in the first column of p. 34 really a reference to the second paper rather than part of a summary of this paper?

This is a mistake coming from improperly dividing the discussion from parts I and II. We will remove the sentence from part I.

The comments I have written are illustrative of where I think the authors could try to give the reader a liltle more help, but they are not exhaustive. My overall assessment is that this is a very interesting paper that would be even more valuable if the authors would provide a little more detail in the explanations.

We have made a few other changes to clarity issues as we have noticed them, as well as responses to the other referee. But more extensive rewriting is probably off the table at the moment.

As I have indicated at the outset, I do recommend publication. Otherwise I would not have gone to the trouble of making these comments.