Report on Article by Ahmadain and Wall

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 $\overline{\omega}$ 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.

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 2d 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.

(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.)

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.

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

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

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

Is eqn. (153) obvious?

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.

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

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.

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