1 SciPost Report 2 on Part I from Matt Headrick

This paper takes up the long-dormant mantle of Tseytlin's nonlinear sigma model approach to string theory. Many technical advances are made, and the whole theory is put on a somewhat more secure foundation. In addition, the theory is explained in a more transparent way than in Tseytlin's many papers on the subject, which unfortunately suffered from leaps in logic, hidden assumptions, etc. Applications of the theory, in particular to black hole entropy, come in a second paper, which I am not reviewing here. This paper is long and highly technical, and addresses many subtle and confusing issues. While I think I understand the gist, and did not find any suspicious or outright false claims, I cannot claim to have checked each derivation carefully. Nonetheless, based on what I do understand, I believe the paper easily clears the bar for publication in SciPost. The results are of great importance for our understanding of string theory, and, with some exceptions detailed below, the presentation is generally clear.

Requested Chnages

Before publication, I would like the authors to address the presentational issues listed below. Some of these are minor or cosmetic, while others are more substantive. In the cases where I suggest a fix, based on my understanding, the authors don't have to follow my suggestion; but in all cases they need to address the issue. From p. 18 onward, where my list ends, the authors may want to follow the spirit of the suggestions and try to identify and clean up any further presentational infelicities.

1. p. 2 R column, a few lines below (T1), "super(string) theory", why is "string" in parentheses?

   This should be "(super)string theory".

2. p. 2 R column, near the bottom, first bullet: What does "the limit where $\log \epsilon^{-1}$ is small" mean? ϵ is dimensionful, so I don't think you mean the limit $\epsilon \to 1$. I think you just mean "at finite ϵ ", i.e. not taking the limit of the next bullet.

   Yes, this should be "finite".

3. p. 3 R column, near bottom: "The sigma model approach is most successful only when the characteristic length of the background spacetime is much less than the string scale". Don't you mean "greater"?

   Yes.

4. p. 34 R column, just below (3): "Unfortunately, this method does not give the correct entropy unless perhaps (following Dabholkar [82]) we allow tachyons to condense on the orbifold." Perhaps the authors did not intend

it this way, but to my reading this is a weirdly derogatory and dismissive throw-away comment, toward what many of us believe is an interesting and well-grounded line of research. Why "perhaps"? Why would we not allow tachyons to condense? Obviously this is not the place for a full discussion of these issues, which presumably comes in paper II. I would suggest just deleting this sentence (and maybe citing Dabholkar in the previous one).

It was not our intention to derogate this approach, we were simply trying to avoid giving the impression that we had actually checked it in some way. We have replaced "perhaps" with language which suggests the approach is quite promising (as we do in fact believe).

5. p. 5 L column, top of page: "For products over n... " This really confused me. I think you don't mean products "over n ", you mean products over the vertex operators at fixed n. The notation strongly suggests a product over n, making equations like (22), (30), etc needlessly hard to understand. I realize you don't want to include yet another index, but some change of notation would be helpful. Maybe put the n over (rather than under) the Π, since it is a product "up to n "?

   We agree that the notation $\prod^{n}$ is less misleading and will use that. We also agree the term "over n" should be replaced with "of n factors".

6. p. 7 L column, bottom of page: "i.e. is proportional to some E_A " Shouldn't that be E_a ?

   Yes, thanks for spotting that.

7. p. 9 R column: Eq (31) is impossible to understand. What does the colon mean? What is on the LHS of the equation? Please rewrite using standard notation.

   We have rewritten this equation, and the surrounding paragraphs, to explain the situation more clearly. The key point was that the new Eq. (34) is valid only near the external poles, but we are now expressing this limitation in the text rather than by shoehorning it into Eq. (34).

8. p. 12 R column: I didn't understand in what sense the S-matrix emerges in the limit that the effective action becomes non-local. Usual QFTs have a local action and an S-matrix. Related to this, my understanding was that the worldsheet cutoff ϵ is related to the size of the string: in the limit the cutoff is small, the string gets large and the effective action becomes non-local in the target space. However, here it seems to be related instead to the distance over which the string can propagate. What is the relation between these things?

   This section needs to be read in light of the preceding section (IV.B) which is illustrated by Figure 2. The key point is that if we regard the worldsheet as having a gas of insertions, the worldsheet theory can still be regarded as a CFT everywhere else. It is therefore possible to conformally transform the worldsheet so that any internal propagator looks like a cylinder. The maximum length of such a propagator is of order $\log \epsilon^{-1}$. In the Schwinger picture of particle propagation, this determines (via the heat kernel) the (approximate) longest possible distance that the particle can propagate in target space. The formulae for this propagation distance is given in the middle bullet point of the 12 R column.

9. p. 13 R column: Eq(40) is missing a minus sign in the exponent.

   Thanks, we have fixed this. [Now Eq. (43).]

10. p. 14 L column: The measure factor in parentheses is confusing, with the n subscript. Maybe just write $d^{2n}z$?

    We agree.

11. p. 15 caption to fig 5(i): "the hyperbolic volume of the regulated gauge orbit is noncompact" I think you mean "is infinite".

    Yes.

12. p. 17 L column: The notation $ij \dots z$ is confusing, given the other role of z here. I would recommend instead $i_1 \dots i_n$ (particularly since the number n of them is fixed).

    While we appreciate the suggestion, we believe that double indices will lead to an excessively messy equation. As z is clearly an index here, we don't believe there is much danger of confusion.

13. p. 17 R column: On the LHS of (60), I believe that I_0^{eff} should be I_χ.

    Yes, this should be $I_{(\chi)}$ if we keep the notation of Eq. (59).

14. p. 17: Eq (61) follows directly from (57) and (58). I didn't understand what was supposed to be gained by the detour through (59) and (60).

    Upon further reflection we agree, and have rewritten the section in the suggested manner.