Response to referee's comments

We thank the referee for a careful reading of the manuscript, interesting questions and valuable suggestions. The referee has suggested a number of changes and advocated precise language regarding SFT which we appreciate. We shall implement most of the referee's suggestions. However, we feel that the referee has misunderstood our points regarding entanglement entropy and the edge-modes. We will attempt to clarify these in our response and in the paper.

In the following we respond to the comments made by the referee in their report.

1. Referee's remark can be separated in three parts.

   - **Comment:** I find the presentation of the "non-canonical kinetic term" confusing. The way it is introduced gives the impression that it is an ad hoc modification of the action without a physical basis (with the exception that one has some freedom off-shell). However, following the discussion in [1] (cited by the author), a more natural explanation could be given. Indeed, section 6 of [1] shows that, on top of the 1-loop determinant of the kinetic operator, there are two other possible contributions to the 1-loop vacuum amplitude: the fundamental 1-loop tadpole $\{\Psi\}_1$ (denoted in [1] as $S_{1,0}$ and the string field measure ρ (taken to be field-independent). I think that $S_{1,0} = 0$ for the light-cone SFT, but the measure remains: in this sense, taking the canonical flat measure in (4.14) is an assumption, and I would interpret the mismatch in the 1-loop vacuum amplitude as a failure of this assumption. Taking into account the path integral measure, the change of coordinates of the author can be interpreted as moving the contributions around to work with finite quantities (it is shown in [1] that ρ is divergent for the canonical kinetic term). The approach of the author to match the 1-loop amplitude is perfectly valid and pragmatic (and similar to what has been done in [1] to work with finite quantities). However, I would suggest adding more details about the path integral measure and changes of coordinates given the discussion in [1]. To my opinion, this would put the discussion on firmer ground.

     Response: The introduction of "non-canonical kinetic term" is based on the results of [1]. The key point is that there is a freedom in choosing the kinetic term, $S_{1,0}$ and the path integral measure to obtain the correct vacuum amplitude. There are many choices but we focus on the two simplest ones: (A) a flat integration measure with a non-canonical kinetic term and $S_{1,0} = 0$ or (B) a non-trivial integration measure with a canonical kinetic term and $S_{1,0} = 0$. We refer to these as choice (A) and choice (B) henceforth. We worked with the choice (A) in our paper.

     Following the referee's suggestion, we will add more details regarding the interplay between the kinetic term and the integration measure in light of the discussion in [1].

   - **Comment:** There is an additional point on this question. In the introduction, the author mentions that the entanglement entropy is an observable (page 2, §1). But, from section 4, it looks like it depends on off-shell data since the entropies (4.24) and (4.37) are different. In general, observables do not depend on off-shell data, so this point should also be clarified.

Response: We agree with the referee that physical observables do not depend on off-shell data. The entropy (4.24) is incorrect and different from (4.37) because it is obtained using a canonical kinetic term with the flat integration measure, i.e., neither choice (A) nor choice (B). The correct entropy is the one in (4.37), which is obtained using the choice (A). The situation is analogous to that of vacuum amplitude in closed SFT. A naïve path integral in SFT with the canonical kinetic term and flat integration measure gives a UV-divergent one-loop vacuum amplitude. It is only after taking into account the path integral measure that one obtains a UV-finite result.

- **Comment:** Finally, I would also suggest discussing how this modification of the kinetic term and/or measure could modify higher-order ($n \geq 0$)-point amplitudes. Indeed, if the propagator is modified, then one can expect the Feynman rules to be also modified, such that the interactions cannot be the standard ones. The exception I could see is if this comes completely from the measure, which is field independent: then, this may not contribute to ($n \geq 1$)-point connected Green functions.

  Reply: We agree with referee's observations. The kinetic term is modified via a field redefinition, which generally changes higher-point Green's functions. The key point is that the S-matrix elements are invariant. This follows from standard considerations regarding field redefinitions: The S-matrix elements are invariant under field redefinitions as long as $\langle 0|\phi_{\text{redefined}}|p\rangle \neq 0$, where $|p\rangle$ is a one-particle state and $|0\rangle$ is the vacuum state in the theory.

  While we believe that this is a standard point in quantum field theory, for the sake of clarity we will add a short comment regarding this at the end of section (4.2).

2. **Comment:** I have a problem with the way the level-matching condition is handled in the paper. I thought that this is an off-shell constraint which must be imposed on the string field. For this reason, I am puzzled by the fact that the author promotes it to an equation of motion derived from the action (4.18). Then, it looks to me that there will be more off-shell states in this case, and I am surprised that one gets the correct 1-loop vacuum amplitude. Can the author comment more on this point?

   Reply: We are afraid that we do not understand referee's criticism completely. We will try to respond to the best of our understanding. We welcome more comments from the referee if our explanation is not satisfactory.

   While the level-matching condition is generally imposed as an off-shell constraint, there is, in principle, no obstruction to promote it to an equation of motion in the light-cone SFT. Moreover, from the worldsheet perspective, it is clear that off-shell states with $L_0^- \neq 0$ contribute non-trivially to the zero-point torus amplitude. We fail to see why the referee finds it problematic to promote, in a consistent way, the level-matching to an equation of motion.

Moreover, if we understand correctly, the referee is puzzled regarding the equality of the vacuum amplitude from actions (4.18) and (4.9). We believe that there is nothing mysterious here. The extra off-shell states in (4.18) come with a trivial kinetic term (i.e., the identity operator) and the vacuum amplitudes computed from (4.9) and (4.18) are the same.

3. In accordance with referee's wishes we have added more discussion about modular invariance and edge-modes at the end of section 4.4. Referee's comment can be divided into three parts to which we will respond now.

   - **Comment:** I am puzzled about the lack of modular invariance of the final result (4.37). I am not sure how edge-modes are supposed to solve the problem since they arise only in gauge-invariant theories, whereas in the current paper the theory is completely gauge fixed. Hence, I would suggest to add more explanations on that point or to leave it out.

     Response: It is true that we are working in a completely gauge-fixed theory but the edge-modes could still contribute and solve the problem. The edge-modes arise in theories with gauge invariance. One can, of course, fix the gauge completely and compute entanglement entropy, but this misses the contribution from the edge-modes. The example of Abelian gauge theory is helpful here. In the light-cone gauge, the theory has only two degrees of freedom, and the entanglement entropy is twice that of a massless scalar field, but this is not the full contribution [2]. To account for the edge-modes, one needs to work with a gauge-invariant formulation and carefully study how the edge-modes become dynamical at the entangling surface. In a similar vein, SFT has a gauge symmetry and our computation in the light-cone gauge captures only a part of the entanglement entropy. To properly account for the edge-modes, one needs to start with a gauge-invariant formulation and extend the analysis of [3] to the case of closed SFT.

   - **Comment:** The author mentions that "it is related to the fact that we are considering an off-shell background". I am not sure to understand this sentence and how the background plays a role: I could imagine that modular invariance is broken if the background is not a CFT, but it does not look to be the case here. Could the author explain in more detail what they means?

     Response: In the introduction we have explained that, to compute the entanglement entropy via the replica method, we need to compute the partition function of the theory on a branched cover of the underlying manifold. In the light-cone SFT the underlying manifold is $\mathbb{R}^{26} \times \mathcal{X}$, where $\mathcal{X}$ parameterizes oscillator directions. The replica method instructs us to compute the partition function on $\mathbb{R}^{26}_n \times \mathcal{X}$, where $\mathbb{R}^{26}_n$ is an n-fold branched cover of the flat space. From the world sheet perspective this is analogous to considering a branched-cover target space, which is not a CFT. Hence this explains the modular non-invariance.

   - **Comment:** I would like to point out the paper [4], where the authors discuss the modular properties of Renyi entropies and entanglement entropy in 2d CFTs. There, it is stated that the entanglement entropy fails to be modular invariant by an additive constant, see (2.34), but a simple modification given in (2.35) leads to a modular invariant entropy. Maybe this paper could be helpful to interpret the results obtained, even if the lack of modular invariance in the current paper is more severe than in [4]. More generally, this point looks crucial to me and I would suggest that the author adds some discussion.

Response:

We thank the referee for pointing out the paper by Lokhande and Mukhi. As appreciated by the referee, the lack of modular invariance in our result is severe. Their results, while interesting in their own right, do not seem immediately relevant to us. We will cite this paper and emphasize that it is not clear to us how one can apply their results to our situation.

4. Referee's remark can be divided in two parts.

   - **Comment:** I find the definition of what is computed exactly brushed over too quickly in section 2. Indeed, only the point-particle QFT is described, whereas the generalization to string field theory looks non-trivial to me. Indeed, string theory is non-local and a simple partition of a Cauchy surface (as done in section 2) raises difficult questions in a theory of gravity, but even more in a theory of strings. Leaving aside the question of gravity, a string can lie completely in one of the two regions, but it can also have parts of it in each region. The author is following [5], where only the center-of-mass is considered to determine in which region the string lies. This is the simplest thing to do and a perfectly valid starting point to investigate possible definitions of entanglement entropy in SFT. However, this assumption should be more explicit in the introduction, and ideally in the abstract. There is some discussion in section 5, but not on all the points mentioned before. Moreover, in view of the importance of the question, this is a bit late in the paper. I would also suggest adding some discussion from [5] on the difficulty of defining the entanglement entropy for strings.

     Response: We agree with the referee regarding issues with the definitions of Cauchy surfaces and subregions in string theory. These are well-known issues in the field and unfortunately our work does not provide additional understanding of these. We simply use the definition proposed by [5] and compute the entanglement entropy in closed SFT. We will add a paragraph in the introduction, reviewing the definition and the discussion in [5].

   - **Comment:** At the end of section 5.1 page 16, it is stated that "The ambiguities in defining spacetime and subregions are the stringy manifestation of the subtleties familiar in diffeomorphism theories." I don't agree with this because I see these two points as different sources of subtleties. The same problem can be found for the open SFT (as justly described by the author in other parts of the paper) even if there is no gravity.

The stringy problem arises from the extension of the string, not from gravity, on which it is an additional complication.

Response: We thank the referee for pointing this out. We will modify the sentence to reflect that the ambiguities we discuss are due to the extended nature of strings. Such ambiguities, in the case of closed strings, go beyond the ambiguities associated with the diffeomorphism invariance.

5. **Comment:** The author says that the background is off-shell, i.e. it does not solve the classical equation of motion. This implies that there is a tree-level 1-point vertex $g_s^{-1}\{\Psi\}_0$ in the action [6, 7]. This could contribute to the entanglement entropy at order $O(g_s^{-2})$. Why is this not taken into account to compute Renyi's entropy?

   Response: We thank the referee for bringing the reference [7] to our attention, which we will cite. We have already mentioned in the introduction that our computation does not capture the important $\mathcal{O}\left(g_s^{-2}\right)$ contribution.

   The $\mathcal{O}\left(g_s^{-2}\right)$ contribution is a challenging open question. Our failure to compute this is due to technical reasons. While the references pointed out by the referee discuss the general strategy for dealing with non-conformal backgrounds, we fail to see how those methods can be used to compute the $\mathcal{O}\left(g_s^{-2}\right)$ contribution in the light-cone SFT.

6. **Comment:** It would be useful to explain in more detail why the light-cone SFT is used instead of the covariant SFT. Gauge invariance and edge-modes should not be an issue when considering the gauge fixed action (in Siegel gauge). Footnote 7 seems to indicate that the problem is related to the level matching L_0^- and the b_0^- conditions, but it is not clear to me why.

   Response: Gauge invariance and the edge-modes have no bearing on our choice of formalism. In fact one can use either the covariant or the light-cone formalism. The author is more adept with the light-cone formulation, which had recently been used to study entanglement entropy in open strings. Remarks regarding L_0^- and b_0^- merely state that the level-matching conditions are imposed on off-shell fields in the covariant formalism.

7. Comment: p. 3, footnote 2: I would have expected the power of the coupling constant to be $g_s^{-\chi(\Sigma_{0,0})} = \frac{1}{g_s^2}$ for the tree-level vacuum amplitude, instead of $\frac{1}{g_s}$

   Response: We thank the referee for pointing this out. This is a typo and will be corrected now.

8. **Comment:** p. 10, footnote 7: This footnote could be improved and expanded. Indeed, from the formulation it is not clear how the situation for the covariant string is different from the light-cone SFT since the level-matching condition $L_0^- = 0$ is present in both cases, as in (4.12). Moreover, I would not mention gauge invariance in that place to justify these conditions since they are also present for the gauge fixed covariant SFT and for the light-cone

SFT (at least, the L_0^- condition for the latter). Considering the kinetic term, the inner-product and the interactions are sufficient (or, from another angle, the geometry of the moduli space decorated with local coordinates).

Response: We believe that the referee has misinterpreted this remark (See response to comment #6). In the covariant formulation, one has to impose the constraint $L_0^- = 0$ on off-shell fields as well as the gauge parameters (see discussion around eq. 3.2 in [6]). It is not clear to us if these conditions can be relaxed off-shell in the covariant formalism. In the light-cone SFT, it is clear that the level-matching condition can be relaxed off-shell.

We will expand and reword the footnote 7 to clarify these points.

9. **Comment:** p. 19: It is stated that the full action of closed SFT has an infinite number of terms. However, this is true only for the covariant form of SFT. The light-cone closed SFT considered in this text is only cubic as proved in [8]. More recently, multiloop amplitudes for light-cone SFT have been studied in [9–14], where a dimensional regularization procedure has been proposed to handle the possible divergences.

   Response: We thank the referee for pointing out these references. We will make our remark more precise and cite the relevant references.

10. **Comment:** sec. 5.2: I am slightly confused about this section. While I understand that the goal is to discuss non-locality in SFT, it looks slightly at odds with the rest of the paper where the non-locality arises from the path integral measure, not from the interactions. Maybe a better toy model would involve a free scalar with a non-trivial x-dependent measure. Or, at least, the author can explain why considering non-local interactions offers a good analogy to the situation discussed in the paper.

    Response: In the literature it has been shown that, due to non-local interactions, some commutators in SFT fail to vanish at spacelike separations [15]. The purpose of this section is precisely to discuss how such non-locality is related to the non-locality in the kinetic term in our paper. In particular, we show that by a field redefinition, we can go from a theory with a local kinetic term but non-local interactions to a theory with a non-local kinetic term but local interactions. After the field redefinitions, the commutators still fail to vanish at spacelike separations, so the two notions of non-locality are related via a field redefinition.

    In the beginning of section 5.2, we will add sentences to clearly state the goal of this section in the context of the paper.

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