PROBLEMS WITH MANUSCRIPT:  
  
In reviewing the figures of your paper, we note that the following  
changes would be needed in order for your figures to conform to the  
style of the Physical Review.  Please check all figures for the  
following problems and make appropriate changes in the text of the  
paper itself wherever needed for consistency.  
  
Figure(s) [9]  
              Please delete the "titles" at the tops of the figures, and  
         incorporate this information into the caption, if desired.

The figures have been modified to address this.

Figure(s) [1,9a]  
  
Physical Review Accelerators and Beams is an all electronic journal.  
Articles are published electronically using the figure and graphics  
files that authors provide.  Your manuscript has a large file size  
associated with the figures.  Our experience is that this type of  
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You should reduce the file size by using different graphics programs  
and/or bitmapping of images.  A reasonable target size for the pdf  
file for each separate figure file is 1 MB or less.

We are confused as all of the pdf files for the figures are already less than 1 MB (largest one is 524 KB).

Color code:

red: Referee’s comments

black: My response to referee’s comments, but I haven’t change the tex file correspondingly.

grey: I accepted referee’s suggestion and change it in the tex file already

green: I don’t agree with referee’s comment

blue: I need some help and suggestions from my co-authors.

----------------------------------------------------------------------  
Report of Referee A -- ZG10155/Zhao  
----------------------------------------------------------------------  
  
REFEREE A - ZG10155   
  
The authors of the manuscript "Emittance Preservation Through Matching   
the Witness Beam in Plasma Wakefield Acceleration" present analytical   
expressions, obtained by using the WKB solution of a single's particle   
motion, to evaluate the beam emittance in an arbitrary adiabatic   
plasma profile by considering a beam without acceleration but with a   
given energy spread. The results are used to evaluate the conditions   
to minimize the the emittance growth for unmatched beams.   
  
The results of the theory are compared with simulations of 3D QuickPIC   
code. The theory is also applied to the FACET II facility. The authors   
propose experiments based on their results to minimize the transverse   
emittance at the exit of the facility and they also investigate the   
emittance growth in an experiment of the same facility with   
self-ionized plasmas, when the plasma is formed in a lithium gas   
contained by a helium buffer gas.   
  
The paper is well written, and the results are, as far as I know, new   
and interesting, in particular the expression of the emittance   
evolution of a beam having a given energy spread, and the conditions   
of a minimal emittance growth for a fixed beam, even if they are valid   
in absence of acceleration. Therefore, I think it could be published   
in Physical Review Accelerators and Beams if the following points are   
clarified:   
  
Pag. 1, 6 lines before the end of the abstract: "the emittance growth   
can be still be minimized". There's a typo. 

Thanks for pointing it out. It has been corrected.

Pag. 1, column 1, 4 lines before the end: "where the witness beam is   
located, not only is there a longitudinal" -> I think "is there"   
should be "there is". 

This seems grammatically correct to us. It could be changed from …..”not only is there a ….” to ….”there is not only a …. .

Pag. 1, column 2, 6 lines before the end: "force is linear   
(proportional to r) , points". There's a space after the round bracket   
that should be removed. 

This has been corrected.

Pag. 1, column 2, the phrase "This ensures that the beam particles   
will not gain additional slice energy spread when undergoing   
acceleration". While it is clear how this is related to the fact that   
the longitudinal electric field does not depend on r, it is not clear   
why it is also related to a linear transverse force. Maybe the phrase   
should be reformulated (or clarified). 

We have modified the phrase to,

“…and does not depend on \xi = ct - z inside the bubble[3]. The fact that the accelerating field does not depend on r ensures that the beam particles will not gain additional slice energy spread when undergoing acceleration and betatron oscillations inside the bubble. Also, the fact that the transverse linear focusing force does not depend on \xi ensures that the beam particles at different longitudinal positions will oscillate at the same betatron frequency, if they have the same energy. If one of these properties is satisfied then the Panofsky Wenzel theorem guarantees that the other is as well ”

Pag. 2, column 1, line 15: the line starts with a comma. 

This has been corrected.

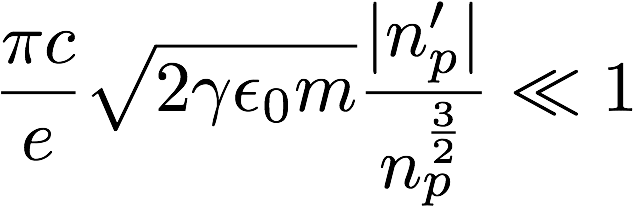
Pag. 2, column 1, line 28: "Therefore the beam parameters need to be   
changed to match the beam to the plasma". Maybe "optimized" is more   
appropriate than "changed". 

We have replaced ‘changed’ with ‘optimized’.

Pag. 2, column 1, line 33: "if the witness beam parameters are fixed".   
This phrase should be clarified. What do the authors mean by "fixed"?   
It seems that it is not possible to change the witness beam   
parameters, however, few lines below, they write to change the beam   
focal plane. This is an important point because one relevant result of   
the paper is the condition to inject a beam in order to minimize the   
emittance growth. It is not very clear why it is possible to change   
the focal plane but not to inject a matched beam. 

We have clarified this in the text. in reality the beam is fixed, for both Twiss parameters and its location. What we change is the location of the plasma. By moving the position of the plasma, we can equivalently think we are “changing” the witness beam’s focal plane position relative to the plasma. So although I say ‘change the beam focal plane position’, what we really do is the change the position of the plasma, and the witness beam’s focal plane position is ‘changed’ in the frame of plasma.

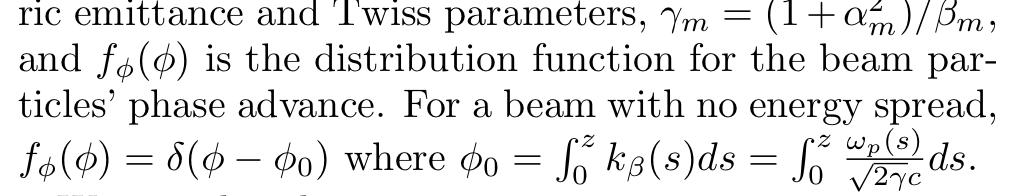
Pag.2, column 2, Eq. (2). It could be useful, if possible, also to   
write the condition for the plasma density profile n\_p(z).

The adiabatic condition in terms of n\_p(z) is:   


It contains a complicated coefficient, so I think it is not very elegant to write the adiabatic condition in terms of n\_p. If you think it is good to show this condition in terms of n\_p explicitly, I can add it into the paper.

Pag. 3, column 1, 4 lines before Eq. (7), when f\_phi(phi) is defined.   
It is written in Appendix A, but here it should be better explained   
the meaning of this function. Moreover, in the following phrase, the   
average gamma symbol is used, but it is explained after Eq. (9). Also,   
after Eq. (12), there is the explanation of the average phi, which is   
already used here. I think it's better to explain and clarify here all   
these symbols.

My modification is in the following (I don’t use the \bar anymore):



Pag. 3, column 2, before Eq. (9). Instead of "for arbitrary   
f\_phi(phi)" I would write "for arbitrary, but small, energy spread". 

We have modified as suggested.

Pag. 3, column 2, 4 lines after Eq. (9): the derivation of the phase   
advance as a function of gamma for small energy spread, phi(gamma),   
should be added. 

Ok, it is just a differentiation. I added the derivation in the appendix.

Pag. 3, column 2, 4 lines before the end: a comment of the   
approximation of neglecting the dependence of the Twiss parameters on   
energy spread should be added. Where it exactly comes from and what   
are the limits of such an approximation. Something is clarified in   
Appendix A, but here a phrase would facilitate a reader. 

This is a hard question. Need to discuss with Xinlu and Weiming. I think there are two reasons of doing it: 1. To make the math easier to work with. 2. Physical intuition tells us the main reason of emittance growth comes from the phase-mixing due to the energy spread, and the dependence of the Twiss parameters on energy spread should be a minor effect. But this is not rigorous… How should we explain this better?

Pag. 4, column 1, Fig. 2: is the 5% of energy spread also used for   
figures (c) and (d)? This should be written. 

Yes, the 5% of energy spread also used for figures (c) and (d). I added this in the paper as you suggested. I also added a sentence saying that the solid (dashed) black lines correspond to the same simulation result (analytical expression).

Pag. 4, column 1, last line before the expression of A for a uniform   
plasma: "we have alpha\_{mi} = 0 ..." -> "we have alpha\_m = alpha\_{mi}   
= 0, so gamma\_m = gamma\_{mi} = ..."

Ok

Pag. 5, column 1, Eq. (17): The authors should add a phrase to explain   
how these conditions are obtained and under which hypothesis. 

I added a citation so I think the problem is solved.

Pag. 5, column 1, after Eq. (18): the authors should give examples on   
how to change the beam's focal plane and comment why it could be   
possible to change the focal plane but not to match the beam. 

The way to change the beam’s focal plane is simply moving the position of the plasma relative to the beam’s focal plane, which doesn’t need to move at all.

‘Changing the beam’s focal plane’ is done in the frame of plasma. Actually we only need to change the position of the plasma. It’s possible to move the location of the plasma, so it is possible to ‘change the beam’s focal plane (in the frame of the plasma)’.

The ‘matched’ Twiss parameters at the plasma entrance are completely determined by the energy of the beam, the value of the plasma density and its derivative at z=0, which has nothing to do with the beam profile (the value of beta at the beam waist). If the beam profile is given, there is no reason that the beam can be matched to the plasma entrance unless there is a happy coincidence.

Pag. 5, column 2, line 11: "assumptions(adiabatic". There should be a   
space after "assumptions".

Sure  
  
Pag. 5, column 2, lines 13, 15, 21 and 22: Figs. 4 should come after   
Figs. 3. 

I agree. I changed the order of these two figures.

Pag. 6, column 2, 13 lines before the end: it's not clear if Fig. 6 is   
obtained without acceleration and how important acceleration is on the   
experiment that can be performed at FACET II. Moreover, it's not clear   
if the nominal parameters of FACET II are close or far from the   
conditions they have determined. Is the proposed experiment something   
particular? Is it possible to optimize the nominal FACET II beam, or   
is it already optimized?

Fig. 6 is obtained with acceleration. I should have pointed it out explicitly.

Also in the previous simulation, I also added a sentence that says that we turn off the longitudinal push so the beam is not accelerated.

The acceleration itself is not very important to the emittance growth. What’s important is the energy chirp of the witness beam caused by the imperfect beam loading when we consider the acceleration, and the emittance will grow a lot more than the no acceleration cases because of that.

As for the parameters of FACET II, I just got these parameters from the people at SLAC. I don’t really know whether they can still be optimized or not.  
  
Pag. 8, Fig. 8: it would be interesting to see what happens of Fig. 8   
(a) if the witness beam's focal plane location is different from 3.39   
cm. How sensible is the emittance growth to this condition with more   
realistic parameters (including acceleration and asymmetry)?

Actually, this is done in Fig 6 (a)(b). I used 8 different values for the witness beam’s focal plane location. In Fig 6 (a)(b), the simulations used more realistic parameters: The beams have acceleration, the driver is asymmetrical. But the plasma is still preformed.

My logic is to find the optimal focal plane position: 3.39cm first, then use it to study the field-ionized plasma with Helium gas on both sides (Fig. 8).

Pag. 8, column 1, 8 lines before the end: it's true that with an   
initial emittance of 20 um, this remains almost constant, but one   
could argue that this emittance is, in any case, larger than those of   
Fig. 8 (a). What is then the advantage? The authors should add a   
comment on this.

Both referees are concerned about this part. Need to discuss with Weiming. How should I deal with it?  
  
Pag. 9, column 1, line 30: "We find that this can potentially lead to   
the witness beam’s emittance can growing by". The phrase seems to   
contain a typo.

Thanks for pointing it out. I deleted the second ‘can’.  
  
Pag. 9, column 2, line 3: "above integral we assume the". I would add:   
"above integral with an energy spread in the beam, we assume the".

Sure. I added ‘with an energy spread in the beam’ like you suggested.

----------------------------------------------------------------------  
Report of Referee B -- ZG10155/Zhao  
----------------------------------------------------------------------  
  
REFEREE B - ZG10155   
  
This paper presents a theoretical analysis concerning the emittance   
evolution of a beam in a plasma wakefield accelerator (PWFA) when   
density ramps at the entrance and exit of the plasma target are taken   
into account. The authors derive an analytical expression (assuming   
there is no acceleration and the plasma profile is adiabatic) for the   
beam emittance growth when the beam has an energy spread and it is not initially matched. Furthermore, given a beam with fixed initial   
parameters and a fixed plasma density ramp, the authors determine to   
optimal position of the beam's focal plane in vacuum such that the   
emittance growth is minimized when the beam enters the plasma.   
Analytical results are validated by means of numerical modeling   
performed with the code QuickPIC. Finally, the authors consider   
emittance degradation from beam-induced ionization of residual gas   
(Helium buffer) when the beam enters the plasma. The study is of   
interest for FACET II.   
  
My first impression was that this paper is a mild extension of the   
works presented in Refs. 4-7. However, the determination of the   
optimal position of the beam's focal plane in vacuum discussed in Sec.   
III is of some interest and, to my knowledge, it was not treated in   
previous literature. Hence, this paper could be considered for   
publication in PRAB. However, before publication can be granted, the   
comments and criticisms discussed below should be addressed and the manuscript amended accordingly.   
  
In Sec. III the authors say that the profile used in Fig 4 was   
obtained by cutting the “non-adiabatic” tail at the entrance and exit.   
How exactly was this done? The adiabaticity condition Eq. (2) [or Eq.   
(5)] does not identify a precise point (the condition is expressed by   
“<< 1”, and not a “< 1”). How sensitive are the results to the exact   
location of the cut? Making the cut at, let's say, +/-1 cm from the   
chosen position does it change the results significantly? Was the non   
adiabatic part of the profile considered in the QuickPIC simulation   
(Fig. 3). Please clarify.

(How exactly was this done?)

By ‘by cutting the “non-adiabatic” tail at the entrance and exit’, I simply mean only use the middle 70cm part of the plasma density profile in the QuickPIC simulation. I numerically calculate the adiabatic factor (The left hand side of (2)) and make sure it is much less that 1 in during this 70cm plasma.

(How sensitive are the results to the exact location of the cut? Making the cut at, let's say, +/-1 cm from the chosen position does it change the results significantly?)

If the ‘results’ means the agreement between the analytical expression and the simulation results, I believe the results will not be sensitive the exact location of the cut as long as the remaining plasma density profile satisfies the adiabatic condition, although I admit that I did not try other cuts for the original plasma density profile.

My point here is just to choose a specific plasma density profile that is close to the profile in the proposed FACET II experiment, and use it to verify the validity of the analytical expression. I have to cut the non-adiabatic part because the theory assumes an adiabatic plasma profile.

(Was the non adiabatic part of the profile considered in the QuickPIC simulation (Fig. 3)?)

In Fig. 3 I did not consider the non adiabatic part, because I want everything in the simulation to satisfy the assumptions in the theory part.

However, in the later part of this section(Fig. 6), I used the whole plasma density profile (with the non adiabatic part). So although I did not consider the non adiabatic part in Fig. 3, I considered in the later part of the paper.

Equation (20) is not justified nor derived anywhere in the text. 

I added the derivation in the appendix

Why are the Twiss parameters in Eq. (19) [Sec. IV] different from the   
ones given at the end of Sec. III (the density profile and witness   
beam parameters seem the same in both sections)? In general more   
clarity on the way parameters are chosen would be greatly appreciated. 

I add some descriptions to make it as clear as possible: For Eq. (19) I added a citation in which the same method was used to find the initial matched Twiss parameters. At the end of Sec. III I added ‘…using the density value and its derivative at z = 0. They are different for two reasons: 1. They are the initial matched Twiss parameters for different plasma density profile (Fig 3(a) and Fig 3(b)). 2. They are obtained using different methods.

The issue discussed in Sec. V was already discussed in Bruhwiler et   
al., PoP 10, 2022 (2003). It is surprising that the authors did not   
acknowledge this work. Increasing the initial beam emittance (from 3   
mm mrad to 20 mm mrad) to suppress the emittance growth is not really an elegant solution since now, even though the emittance growth is suppressed, the quality of the bunch is intrinsically bad from the   
start. 

I am really sorry that I did not cite this paper. When I wrote my paper, I just wanted to show some of my simulation results for FACET-II. I had no idea that some similar work had been done before. This is my first time to submit a paper and I don’t have a lot of experience. I will read this paper carefully and cite it properly.

I admit that increasing the initial emittance of the beam is not an elegant solution to this problem, or not even a solution: It is just a way to get around the problem of large emittance growth. I simply wanted to show it in the paper because this is the result I got from the simulation.

Both referees are concerned about this ‘increase the initial emittance to 20um’ part. Need to discuss with Weiming. How should I deal with it?

The list of references in this paper is very FACET/UCLA-centric, this   
degree of self-referentiality is a bit disconcerting. At a very   
minimum, the authors should add “Bruhwiler et al., PoP 10, 2022   
(2003)” to the list of references. The problem of emittance   
preservation/degradation when a beam is injected or extracted from a   
plasma stage has been analyzed by several groups and this effort, I   
think, should be acknowledged. A list of possible relevant references   
should include Mehrling et al., PRSTAB 15, 111303 (2012); I. Dornmair   
et al., PRSTAB 18, 041302 (2015); P. Antici et al., J. Appl. Phys.   
112, 044902 (2012); M. Migliorati et al., Phys. Rev. ST Accel. Beams   
16, 011302 (2013). In addition, the authors should/could acknowledge   
related work in the contest of conventional accelerators (e.g., check   
the reference list in Ref. 7). 

I am really sorry for not citing these papers properly. I guess the main reason is that when I did this research, I mainly discussed with the people in my group and read their papers, and I did not even know the existence of Migliorati’s paper. I will read all the papers you mentioned carefully, and then do a thorough research of what other groups did before. I apologize for not acknowledging these related work and I promise I will fix it.

The language and text should be revised removing typos (e.g., there   
are two typos in the abstract) and improving the form of the English   
language (e.g., the title does not read particularly well) is strongly   
encouraged.

I apologize for my careless. I will read my paper carefully and remove all the typos. I will also check my English and make sure they read well.

I only found one typo that was pointed out by referee A. Is the other typo the missing ‘the’ in front of ‘wake’ in line 11?