Report of the LCLS Injector Laser Heater Physics Review Committee

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1. Introduction

The operation of the x-ray free electron laser (FEL) at the LCLS requires that the brightness of the electron beams entering the LCLS undulator be very high. The high brightness is obtained by high peak current, small (transverse) emittance, and small energy spread. The LCLS injector, consisting of an rf photocathode gun, two bunch compressors, and accelerating sections between the compressors, is designed to produce beams of required emittance and peak current. However, the energy spread from the gun appears to be too small—smaller by a factor of ten than that the FEL requirement. This would be fine except for the fact that such beams tend to develop instabilities and their energy spreads become so large in the process as to be unfit for the high-gain FEL operation. This dilemma can be resolved if the electron beam could be "heated," that is, its instantaneous energy spread could be increased, by an amount large enough to suppress the instabilities but small enough so that the FEL gain is not affected. The coherent synchrotron radiation (CSR) was known to drive such an instability, which can be suppressed by a heater in the form of a superconducting wiggler in front of the second compressor.

The topic for this review is to evaluate whether another instability due to longitudinal space charge (LSC) force is a serious enough issue to justify a heater. A two-hour video conference was held during which Zhirong Huang, Juhao Wu, and Cecile Limborg presented their studies (carried out in collaboration with other physicists at SLAC and other institutions) on the LSC instability and its cure by a laser heater. Since the LSC instability occurs in the low-energy region near the first bunch compressor, a wiggler located near the second compressor for suppressing the CSR instability does not solve the LSC instability problem. Instead, a "laser heater" near the first compressor was proposed. The charge to the Committee calls for a clear recommendation regarding whether the laser heater should be incorporated into the LCLS project. Also requested are comments on outstanding questions and perceived problems in the presentations.

Sections 2 and 3 below contain a summary of the Committee responses, and Appendix A is the charge to the Committee. Detailed comments by Committee members are collected in Appendix B.

2. Recommendation on the laser heater

The Committee is unanimous in recommending that a laser heater be incorporated as proposed in the LCLS project. The LSC instability does appear to be serious and the laser heater is a low-risk, low-cost solution of the problem. The laser heater also suppresses the CSR instability and obviates the requirement for a superconducting wiggler. It is fortuitous that the threshold for these instabilities is such that they can be suppressed by a suitable heater without compromising the FEL performance. The laser heater provides an additional controlling parameter to this highly challenging project.

3. Comments on further study

The Committee wishes to compliment the SLAC physicists for their quality work on the LSC instability and its cure via a laser heater based on resonant laser-electron beam interaction in an undulator. They have carried out an original and compelling analysis of this relatively novel beam dynamics phenomena. Their study is a proper basis for recommending the use of the laser heater. However, the Committee feels that the understanding of the LSC instability is not completely satisfactory yet. The Committee therefore recommends that the study be continued and improved to clarify further issues listed below:

3.1 Origin of the input density modulation

The starting point of the SLAC study is the assumption that beams from the rf photocathode gun have density modulations of about 1% in amplitude with wavelengths in the range of tens of microns. Although reasonable, the origin of such modulation is not clearly understood. Two possible origins have been considered: intensity ripples in the photocathode drive laser and space-charge oscillation in the gun cavity. These speculations need to be confirmed by a combination of experimental and numerical investigations.

3.2 Theory and simulation

The transverse dependence of the LSC impedance and of the energy modulation induced by a laser heater have not been taken into account in the SLAC study. Although these effects are not expected to modify the results by more than 50%, a more complete understanding would be useful in designing and commissioning the heater. The prediction of simulation differs from that of theory significantly even for beam propagation of short distances in a drift space. The cause of the discrepancy should be thoroughly clarified. The size of time steps in simulation may not have been optimum.

3.3 Injector layout

The dependence of the instability gain on the layout of the injector was not discussed during the review. It is possible that the instability gain could be minimized in an optimized injector-accelerator-compressor layout.

3.4 Experimental study of the LSC instability

It would be useful to verify experimentally the LSC instability at laboratories other than SDL. Diagnostics for commissioning and operating the laser heater need to be developed.

Appendix A. Charge to the LCLS Injector Laser Heater Physics Review Committee

The scope of this review is the physics underlying the LCLS injector laser heater. You will hear presentations by Zhirong Huang, Juhao Wu, and Cecile Limborg that cover the calculations and simulations they have done to model an undulator-in-chicane inverse FEL where an IR laser beam will co-propagate with the LCLS injector electron beam. This device is designed to add an uncorrelated energy spread to the electron beam, to mitigate the effects of instabilities when the beam later passes through bunch compressors. The engineering of the device is not within the scope of this review.

The review is in the form of a two-hour video conference. Kwang-je Kim has agreed to be the chairman of the committee. There will be time for questions and answers during the presentation, but the work of the committee will also include the preparation by each committee member of a communication to the chairman, which he will gather and summarize. Each committee member should identify any outstanding questions and perceived problems with the presentation, and e-mail them to the chairman. Committee members should also note those parts of the presentation that they concur with. The chairman's summary will discuss both those aspects of the presentation that are agreed to be sound, and any parts of the presentation that are problematic. The summary should make a go/no-go recommendation for the construction of the laser heater. A 'go' recommendation may include advice to clear up minor points. However, if there are significant problems identified by the reviewers, we will address them in further work.

Appendix B. Individual Comments

This Appendix contains detailed comments by the members of the Review Committee. Also included is the contribution by Yu-Jong Kim, who, although not a member of the review committee, participated in the discussion.

Comments from W. Fawley:

First of all, I think that the laser heater appears to be a relatively straightforward and inexpensive device to spoil the "instantaneous" energy spread at relatively low energies in the LCLS and strongly support its addition to the base LCLS project. As mentioned by S. Krinsky and A. Zholents, its functionality will be useful in terms of controlling both the "known" instabilities such as the coherent space-charge instability and possibly any "unknown" unknowns which might crop up during commissioning. Consequently, it is important that this "heater" have a laser power "knob" to allow the operators to tune the device for optimal x-ray lasing of the LCLS.

In addition to my support for SLAC deciding to go forward with the heater, I wish to offer some suggestions concerning the simulation effort that has been supporting the heater study. One problem that has been evident to me for quite a while is that some of the computational tools (i.e. **elegant**) that have been employed are not as robust as they should be, particularly in terms of including the transverse variation of the longitudinal space-charge fields. This problem is not due to negligence on the part of the actual code users such as J. Wu but rather stems from an unfortunate lack of resources available to support improvements by **elegant**'s author, M. Borland. If **elegant** is going to be the primary workhorse assigned to tracking the e-beam from just beyond the injector to the undulator entrance, it is very important that a reasonably good space charge model be implemented ASAP, and I respectfully request that greater resources from the LCLS program be given to M. Borland to do so.

Another problem in the code modeling appeared during J. Wu's talk in comparing analytical results vs. PARMELA and ASTRA simulations for energy modulations. The analytical curves disagree with simulation results more than I would like in the first 0.5 m of drift in the ASTRA case (slide #11 of Wu's talk). There is less of a disagreement in the PARMELA case, but the 500-micron energy modulation case is still off by > 20%. Similarly, for the density modulation case, the ASTRA and analytic results (slide #12) begin to diverge strongly after the first "bounce" between z = 0.5 and z = 1.0 m. While it first was bothersome to me that the calculation of LSC-induced energy modulation (slide #13) for an initial density modulation for the 100- and 250-micron-wavelength cases do not seem to asymptote to the correct values at z > 6 m, discussions with Wu and Huang indicate this is a finite pulse-length effect in the simulation.

Consequently, I think SLAC should make some effort to understand the causes of the discrepancies between analysis and simulation and then eliminate them where feasible (bearing in mind the finite resources in the LCLS project). Ideally, a set of benchmark runs could be set up where the analytic answer is well understood (i.e., believed) and then run ASTRA and PARMELA to see how closely they come. Discussions post-review with Wu and Huang indicate that it can be tricky to deal analytically with the existing lattice because the beam size changes significantly with z.

A similar but less severe disagreement between PARMELA and theory appears in slide 5 of C. Limborg-Deprey's talk. I would have expected that theory would give an excellent answer for the slope of delta-E versus z in the 1st quarter plasma oscillation right at the start of the simulation. Yet, there is a surprisingly large discrepancy for the 6-MeV case and a smaller (but still obvious one) for the 12-MeV case. Similar problems were apparent in a similar talk given at the Zeuthen meeting last August. I do not know if these discrepancies arise from a problem in PARMELA or in the theory (which does not include the radial variation of the longitudinal space-charge force). For a simple drifting beam, I believe accuracies of a few percent or better should be obtainable from both simulation and theory (unless there is some complexity arising from the beamline lattice of which I am unaware).

I also wish to alert the SLAC simulationists to possible difficulties that can arise from insufficient resolution, either temporally or spatially in the longitudinal direction. In plasma simulation, in order to prevent numerical phase errors accumulating over many hundreds of oscillation periods, the rule of thumb is to try to use a time step of ≤ 1/32nd of a plasma period. For the LCLS we are concerned with only a few oscillation periods, but one should check that the time step is appropriately small. Spatially, to get a good resolution of a sinusoidally-varying (in z) space-charge wave, one probably wants 16 or greater grid cells per period, otherwise, the effective force tends to be partially numerically suppressed. Another parameter that plays a role in terms of simulation noise and accuracy is the ratio of the plasma Debye length to the grid spacing. It would probably be useful for one or more of the SLAC simulationists to look at Birdsall and Langdon's book and extract from it assorted wisdom on this subject (I note that in post-review discussions with Huang and Wu, they were quite open and positive regarding this suggestion).

Diagnostics:

Although this was not an engineering review, I wish to bring up a point concerning the operational utility of the laser heater, i.e., how do we know it is actually working? Unless one can measure the longitudinal beam bunching (and/or emittance growth) due to instabilities, it is unclear to me how one will know if the laser heater is in fact suppressing such instabilities. Unless one is so lucky that the LCLS does lase with the laser heater ON and does not with it OFF, I think pre-undulator diagnostics will be very important. SLAC management and scientists should be concerned with this diagnostic issue "right now" and not wait until deep into the implementation of the heater to start worrying about how to measure various relevant parameters. On the other hand, the success of the SPPS project and the excellent diagnostics developed there suggest to me that the SLAC people are up to this sort of diagnostic challenge.

Comments from S. Krinsky:

I believe the incorporation of a laser heater into the LCLS is a very good idea. One wants the electron beam to be cold enough to undergo the FEL instability in the long undulator, but warm enough not to suffer unwanted instabilities during beam transport. Detailed calculations were presented at the review, demonstrating that there is a real concern about instabilities during transport. Calculations also show that the heater can reduce or eliminate these instabilities. The flexibility provided by the laser heater in allowing one to adjust and optimize the temperature of the electron beam will be a very valuable knob to have during commissioning and operation of the LCLS. In addition, it may be possible to use the laser heater to shape the temporal profile of the FEL output.

The detailed analysis presented at the review is of high quality and provides a proper basis for design of the system. However, these calculations should continue to be developed and improved. Several aspects of the analysis that need clarification were discussed at the review. Further work should be done to characterize the energy distribution induced by the heater and its effectiveness in reducing the instabilities during transport. In particular, the effect of the transverse dependence of the energy modulation deserves further study. Also, the Landau damping resulting from the transverse electron beam emittance should be more clearly presented. Computer simulation of the transport of the electron beam from the cathode to the entrance of the first bunch compressor is important and worthy of further study.

In conclusion, I was impressed with the quality and depth of the analysis presented at the review. I believe that the present state of the analysis is sufficient to proceed with detailed design of the laser heater. Continued development of the theoretical description of the transport of the electron beam is of great importance and should be given high priority at the LCLS.

Comments from J. Rossbach and E. Schneidmiller:

Recommendations:

The laser heater as presented is certainly an appropriate tool to cure the space charge instability in case it turns out to become a serious problem for LCLS. The studies on this instability presented by Z. Huang, J. Wu, and C. Limborg indicate convincingly that there is a potential danger and that the laser heater will do this job.

Still we recommend that the simulation studies should be refined in order to better understand the gain effect, how it depends on various parameters, and how the laser heater has to be operated in detail. It became apparent during the meeting that the numerical simulations performed so far do neglect some aspects, like 3D field calculations (including bunch compressor region) and beam dynamics in the gun region at shorter modulation periods, that potentially could reduce the gain.

We also recommend that other ways to ameliorate the situation be considered. One might think, e.g., as proposed by Y. Kim, of a modification of the compressor scheme. The dependence of the instability gain on machine design parameters was not considered at all during the meeting. It might be worthwhile to identify, if possible, ways to decrease the instability gain to be expected, such that the laser heater still remains desirable for safety but is not any more a matter of survival.

In any case we agree that the laser heater provides another free parameter for optimizing the FEL performance which will be very useful if it is available.

Comments from Y.-J. Kim:

Personally, I agree that the laser beam heater will help in curing the possible microbunching instability at LCLS project. I recommend that the following issues should be refined further to understand the microbunching instability at the LCLS project cleanly and to estimate the gain of the microbunching instability at the LCLS project properly. This is a short comment. You can find its full comment in reference [1].

Since the estimated gain is maximum for around 15 μm modulation (= 50 fs in time), and the gain is high only when the modulation wavelength is shorter than 60 μm (= 200 fs in time scale), first of all, LCLS friends should try to measure the exact modulation wavelength in their Ti-Sapphire laser beam to estimate the gain properly. And LCLS friends may also check the possibility of the laser beam operation with a somewhat longer rising/falling time to reduce such a fast modulation.

According to recent simulations by Martin Dohlus with his CSRtrack code, the uncorrelated energy spread is about two times higher if we consider 3D space-charge force as well as 3D CSR wakefield in the bunch compressor. Therefore Z. Huang's analytical method and M. Borland's ELEGANT code should be improved to consider 3D space-charge force and 3D CSR wakefield in the bunch compressor, and to consider 3D space-charge force in the drift space and in the accelerator.

As to PARMELA and ASTRA simulations at the 5.7-MeV region, LCLS friends should use a real bunched beam that starts from the cathode (instead of uniform, or coasting) to consider the nonlinear space-charge force and transverse variation of the space-charge-induced longitudinal electrical field. At low energy, their smearing effect can not be ignored at the head and tail regions.

Even though we do not apply any density modulation initially, an artificial energy modulation or microbunching instability can be generated if we use too many BINs in an ASTRA (or **elegant**) simulation with limited macro-particles [1]. For example, on page 8 of C. Limborg's presentation material (Z = 0.15 m, E = 6 MeV case), we can see a modulation period in the current density amplitude. But on page 9 (Z = 1.4 m, E = 6 MeV), we can not see any clean modulation pattern in current density amplitude. From Z = 0.15 m up to the end of the injector it is not easy to distinguish the real density modulation signal and the numerical noise signal. Certainly this type ofnumerical noise amplifies the energy modulation. To check this artificial modulation, I recommend that LCLS friends should check both cases (no modulation case and a modulation case) with the same BINs number and the same total macro-particles. Then LCLS friends should analyze both results with the same analysis technique. If the no-modulation case does not exhibit any artificial modulation, the selected BINs number and total macro-particle number is good enough to simulate such a very short-period modulation [1].

If LCLS friends improve their bunch compressor layout with a higher compression factor at BC1 and a lower beam energy at BC2, which is also helpful in reducing the CSR effect at BC2, the uncorrelated energy spread at BC2 can be increased by about ten times without any heater [1]. Therefore the gain will be largely reduced. After improving the bunch compressor layout, we can increase the relative uncorrelated energy spreads of TESLA XFEL project (PAL XFEL project) up to 4.8E-5 (4.3E-5) at BC2 without any heater [1].

Reference:

[1] http://www.desy.de/~yjkim/LCLS/YJKIM_comment_LCLS_HEATER_FULL.doc

I hope that these comments help in understanding and curing the microbunching instability at the LCLS project.

Comments from A. Zholents:

I would like to note high quality presentations at this review. Personally I enjoyed most parts of it.

1. I am convinced that LCLS performance will not be compromised if beam energy spread will be increased up to 1×10^{-4}.

2. I agree with presenters that there is not much else one can do to reduce or eliminate spatial modulation on a beam other than to increase uncorrelated beam energy spread. I have not heard about any other proposals.

3. I agree that laser e-beam interaction in the wiggler offers an excellent opportunity to do it. I find it a bit surprising that a regular energy modulation in the E-z plane at the end of the wiggler appears totally random at the end of the chicane. However I tend to believe that a combination of Gaussian transverse profiles of electron and laser beams and coupling of energy modulation to the x-x' plane can produce it.

4. I think that the "laser heater" is not extremely difficult technically, and I agree with estimates for a laser power needed to produce adequate energy modulation to the beam. I think that the commissioning and maintenance of the "laser heater" will require dedicated diagnostics which can be made, but was not discussed at the review.

5. Since the electron beam emittance is not increased notably by the "laser heater," I don't see any harm it can do.

Having said all that, I will endorse the "laser heater" as a useful tool for beam manipulation and instrumentation.

Now I would like to comment on some different issues only partly related to presentations at the review.

1. I’ve seen several times the experimental data from BNL’s SDL gun and no similar data from other gun facilities, and I wonder if it is reproducible on other facilities or is unique to SDL. However, I admit, I may lack information, and if it exists then it may be a good idea to add it to a package.

2. Along the same line, I wonder if it is possible to sort out the photo-injector laser by bandwidth and see if wideband lasers are more harmful.

3. I am also interested to find out what role the virtual cathode plays in a space-charge-dominated regime. Does it smooth out intensity modulations or, oppositely, amplify them?

Comments from L.-H. Yu:

1. Laser heating increases a knob on the system without significant cost, and may solve the instability problem, so I think it is good to have it.

2. I noticed that the matched laser beam generates a Gaussian energy distribution for the projection of the whole beam onto the energy axis, but for a part of the transverse electron beam the distribution still has double peaks, which might be a problem even though I am not so sure about it. I understand that the present simulation would not answer this because it treats all the electrons on or off-axis by 1-D. I speculate that when the modulation wavelength is smaller than the electron beam size and if the betatron wavelength is large, different transverse parts may behave independently from each other. In this case the double peaks of the energy distribution within a small transverse part may still create an instability problem, even though different transverse parts are incoherent with each other.

   I understand that the 2-D simulation might be very complicated and difficult, so maybe some analytical estimate is needed to show this is not really a problem.