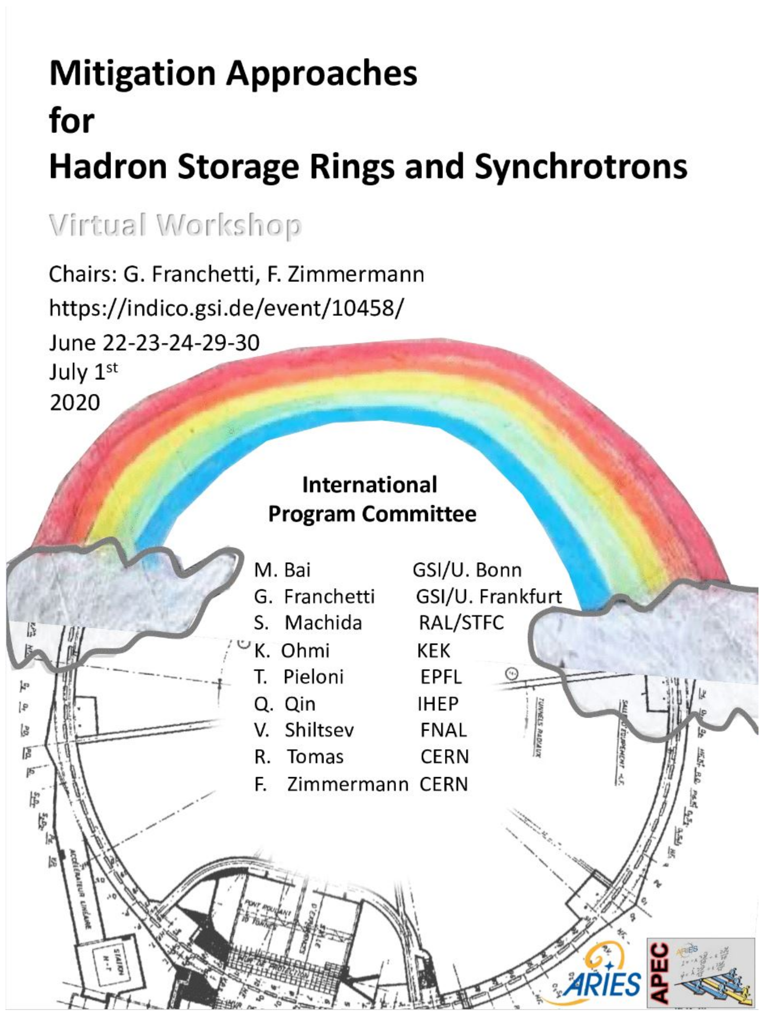
Workshop:
'Mitigation Approaches for Storage Rings and Synchrotrons'

Workshop Minutes
June 22nd - July 1st 2020



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1 June 22nd

Scientific secretaries: A. Lauterbach - G. Russo

1.1 Welcome

Giuliano Franchetti welcomes all participants and says a few words about the goal of APEC (Accelerator Performance and Concepts). He informs everybody about the organization of the Workshop, such as the timetable and the rules regarding to the virtual meeting. Permission to record the sessions is requested.
Q. Qin welcomes all participants, introduces himself as session chair and hands over to Tobias Persson.

1.2 Limits of LHC Optics Corrections

Speaker: Tobias Persson, CERN
Scheduled: June 22nd, 2.30 pm - 3.00 pm

Tobias Persson goes through his talk: the aim being to highlight which are the reason why optic corrections are needed in LHC. During his talk three main topics are addressed: machine protection, increase luminosity to experiments and mitigation of beam instabilities. Then, he goes into the details of the correction strategy adopted between 2010-2015, showing improvements and limitation of global corrections in measuring beta-beating. But in order to further increase the precision in measuring β-function, and therefore, optics, new techniques are needed: K-modulation of magnets close to IP and precise β-function calculation from the amplitude of oscillations. He concludes his talk by mentioning the limitations of K-modulation, a new local observable for linear lattice imperfections based on the phase advance between two elements, the importance and measurements of non-linearities when β^* is further reduced at IP(s). And in conclusions, he goes trough challenges and possible solutions to further improve optics corrections.

V. Shiltsev asks if $\Delta\beta/\Delta(\Delta P/P)^2$ is controlled, which is confirmed by T. Persson for the injection, but not at full energy. He adds that the chromaticity is measured at commissioning. The question, if the reason for the systematic space shift is understood yet, is raised. T. Persson answers that part of it comes from the corrections that were used and he highlights that the b_3 corrections are in fact the sextupole corrections at IP(s).

SY Lee wants to know if the $\Delta\beta/\beta$ increase by 3% in beam crossing due to bb tune shift correction in bb-crossing. Tobias Persson answers that this comes from the b_3 in the IRs.

1.3 Fringe fields: more relevant than the body?

Speaker: Thomas Pugnat, CEA
Scheduled: June 22nd, 3.00 pm - 3.30 pm

Thomas Pugnat goes through his talk: the aim being to understand if non-linearities in the longitudinal magnetic field distribution have any effect on the beam dynamics. Therefore, the goal would be to develop a non-linear transfer map for tracking studies, benchmark it with calculated or measured magnetic field maps provided by designers and finally study observables that could be sensible to longitudinal magnetic field distribution. At first Thomas shows how to build a new transfer map based on Lie Algebra. After the demonstration, he shows the analytical form to compute the amplitude detuning. The equations are presented in two different forms according to the mathematical model used to describe the importance of the magnetic harmonics in the longitudinal direction ("Hard Edge" or "Generalized Gradient" model). At IP(s) the beam has to be highly squeezed and non-linear correctors and Inner Triplets are used in order to properly achieve this goal. The high sensitivity to the non-linearities of the β^* as well as the non-linearities at the edge to the magnets made LHC an interesting case study. Thomas then describes three different models to be used in the characterization of magnetic field and it's harmonics: Hard Edge (HE); HE + Heads and the non-linear transfer map from Lie algebra (Lie2). Results on the detuning and dynamic apertures are then presented and discussed. Before the conclusions, Thomas presents some preliminary results on how the amplitude affects the β-beating. In the end, he has effectively showed that longitudinal distribution of magnetic harmonics affect the beam dynamics and therefore its stability.

Valeri Lebedev highlights that there is no separation between intrinsic contribution and what is produced by manufacturing imperfections. So considering a quadrupoles from the final focus there is a significant intrinsic contribution which cannot be corrected by octupoles since it's a cubic non-linearity. He therefore asks what will occur when a reduction of β-function is performed, and what is the ration between manufacturing and intrinsic contribution. Thomas answers that in the simulations the total integral strength is always the same and the weight of what is inside the extremities and the body plays a role. V. Lebedev adds that that the contribution he mentioned comes from nature, is well-known and has to be taken into account, because they start to play a bigger role going to smaller and smaller beta functions at IP(s). Amplitude and tune dependencies can be compensated but it's really difficult to completely eliminate the non-linearity. Barbara Dalena adds that the octupole-like contribution in the model comes from the derivatives of the gradient at the extremities and it has a small impact in LHC, where, on the contrary, is the longitudinal distribution that plays an important role.

SY Lee wonders if there has been any experiment to verify all these aspects in order to gain more knowledge not just for LHC but also for future machines. T. Pugnat replies that there are experiments where this could be studied.

1.4 What did we learn from machine learning?

Speaker: Elena Fol, CERN
Scheduled: June 22nd, 3.30 pm - 4.00 pm

Elena Fol makes a brief introduction to machine learning (ML), explaining why it is useful and the complexity behind the numerical implementation of this technique. Then she introduces three different approaches (supervised, unsupervised and reinforcement) where ML can be used. She then focuses on the first one and she gives a detailed explanation of the rationale. Therefore, she describes why ML can be used in particle accelerator and, in particular, for predictions and data analysis. The current applications of ML are tuning optimization control, virtual diagnostics, anomaly and and fault detection, surrogate and predictive modeling. She gives references for each of the previous fields. Then the focus is put on the detailed explanation on optics corrections. Two examples are reported: identification of faulty BPMs and quadrupoles errors. She then concludes highlighting benefits and present limitations in ML methods on accelerator problems.

Thomas Pugnat refers to slide 27 and asks why some BPMs are preserved despite of SVD and threshold. Elena Fol replies that in the figure one faulty BPM is left, because the method works with an expected fraction of outliers that has to be set beforehand. If the expected fraction is too high then it might happen that some good BPMs could be detected as faulty ones. Thomas Pugnat raises the question if beta-beating, but in general optics functions, is recomputed after removing a faulty BPM. Elena Fol replies that this is not the case, for with this method, the BPMs are removed before computing the optics. Thomas Pugnat adds the question if one could suppress some BPM which feel incoherent with the model while still being perfectly coherent physically. The answer by Elena Fol refers again to the idea of identifying the BPMs before the optics functions are computed, which is an advantage of the method. When doing it like in the model the optic functions would have to be computed again.

SY Lee asks for more comments on the possibility of using BPMs (commonly use to control and suppress beam instability) to parasitically monitor the machine during operations, i.e. keep having control on the accelerator even when there are no operators around. Elena Fol mentions that there are some examples which are related to this, At present, these techniques are not applied parasitically but for example it could be possible to detect some physical properties and then have a ML code running in the background that computes, at the same time, other properties of interest. She furthermore confirms, some data could be retrieved from log books, but in this last case ML codes should be developed for this specific case. SY Lee mentions that he made a proposal once to take data at certain BPM to monitor the machine and put the BPMs back to detect and correct beam instabilities.

Tatiana Pieloni adds that something similar is currently tried at LHC using the transverse feedback data for instability detection, So far offline work has been performed with data from past runs but the idea would be to have online data analysis. The problems behind this attempt is the speed at which the analysis could be performed due to the large amount of data. A more feasible attempt could be done with algorithms running parallel to operations and detect instabilities or anomalies. Elena suggests a possible improvement: it would be possible to train offline the algorithm and then predict the instability online, so that the algorithm needs a lighter amount of data.

— 30 min Virtual Coffee Break —

Yannis Papaphilippou introduces himself as the chair for the second session of the day and hands the word over to the next speaker.

1.5 Fighting nonlinear dynamics with nonlinear elements

Speaker: Alexander Valishev, Fermilab
Scheduled: June 22nd, 4.30 pm - 5.00 pm

Alexander Valishev starts his presentation describing IOTA/FAST facility and he suggests a new approach where non-linearities are the base of the design of the machine. He explains the basic criteria laying behind a non-linear design. There should be a strong dependence on the frequency and amplitude of oscillations, it has to be stable and integrable in 2D and it must be feasible to achieve with magnetic fields in vacuum. The main advantages for future High Energy Particle (HEP) accelerators are a reduction of chaos in single-particle motion and a strong immunity to collective instabilities with Landau damping. He then presents the solution of Danilov-Nagaitsev where the time dependency of the Hamiltonian has been removed. He continues showing from a mathematical point of view why it is possible to chose a non-linear potential and therefore, a possible solution where it is built with octupoles. He then explains the reasons why such a design is of great interest. On the one hand, academic reasons to prove the stability of non-linear systems; on the other hand, check if non-linear designs should be preferred to linear ones. The rationale in assessing the feasibility of this design should be composed by two phases. The first one where the single-particle motion stability is achieved by the use of electron beams and a second one where studies with high-intensity proton beams are performed. He then shows the present status of IOTA and the results achieved so far, focusing at the end on the future goals.

Giuliano Franchetti asks if the result presented in slide 42 (threshold of instabilities between the higher thermal response and the octupole one) finds an agreement with the work of Alexey Burov. Alexander Valishev points out that Alexey Burov is a member of the experiment and the comparison is now ongoing. At present, a theoretical model is under development.

SY Lee wants to know the reason why the beam splits in two and the instability mode. Alexander Valishev answers that the splitting is part of the design of the non-linear system. SY Lee adds a question if it was tried to use some kind of corrector when the beam shows a linear or quadrupolar error. The answer to this question is put on hold.

Yannis Papaphilippou raises the question of what is the importance of fringe fields in the presented machine. Alexander Valishev replies that at high amplitude of oscillations of the beam fringe fields in dipoles and quadrupoles start playing a significant role. Thanks to the modeling performed in collaboration with Berkeley Lab, fringe field shouldn't cause significant effect on the beam because dipoles and quadrupoles have a large enough bore. Systematic changes from ideal conditions and related studies are foreseen.

1.6 Optics corrections and schemes of conventional advanced accelerators

Speaker: Xiaobiao Huang, SLAC
Scheduled: June 22nd, 5.00 pm - 5.30 pm

Xiaobiao Huang goes through his talk. At first he gives an overview of optics characterization and errors and then he explains different ways to perform global optics corrections: orbit response matrix, turn-by-turn (TbT) BPM data and β-function measurement through quadrupole modulation. The aim being to use Model-Independent Analysis (MIA) and Independent Component Analysis (ICA) to extract phase advance and β-function. Then, the accuracy of the estimation obtained with ICA is performed and it is compared with the results of numerical simulations (NAFF and Castro+NAFF). Moreover, the optics functions just computed can be used for the fitting.

Yannis Papaphillipou asks if the method of random walk optimization of skew quadrupoles correctors that has been tried at SLS could be used for coupling corrections. Xiaobiao answers that the approach he described is tuning based; moreover, the focus is on correction methods, that can predict the errors source by the measurements of data just using scan or measurements themselves. The main difference is that using these type of approach (like ADTs) the data can already show the result of the measurements without having the need to let it end. There are therefore two approaches one is Beam based correction and the other one is beam based optimization.

Vladimir Shiltsev raises the question if the fields induced by the beams (like impedance) and optics distortions due to space charge effects could be measured or not. Xiaobiao answers that there were some successful application to measure the effect of impedance. EPS has tried to measure orbit distortions at a different level of beam current and the results show the dependence of optics on beam current and it was also possible to find out the sources of impedance. Moreover, he adds that it would be even easier to use the TbT data for this kind of measurements. As far as space charge effect is concerned, Xiaobiao did some measurements at Fermilab where the tune shift for different beam intensity was measured. But more detailed studies, especially on the interpretation of the results, must be performed.

1.7 Discussion Session

R. Ainsworth asks which is the correct phase advance that has to be considered since space charge is gonna affect it's value. A. Valishev answers that the choice is strongly affected by the beam distribution. Then, H. Bartosik says that commonly an empirical value is computed to then minimize the losses, because the errors and non-linearities are driven by random error sources with unknown distribution around the machine. With a low to medium intensity beam this procedure should work for both HL and space charge. SY Lee points out that space charge effect is relative to beam centre.

SY Lee aks if anyone ever tried to use a beam with very small emittance to explore the non-linear phase space of the IOTA storage ring. Alexander Valishev confirms. S.Y. Lee points out that the beam decoheres fast so the emittance seems to be too large. A. Valishev confirms and adds that the intensity was kept low because of significant intensity effects on the momentum spread due to intrabeam scattering. The plan is to increase the energy and make the beam smaller. There is also research going on using single electrons and explore the phase space with a system of diagnostics.

Valeri Lebedev comments, that not everything can be measured in one shot. Furthermore, if the beam current could be reduced by factor five the beam would be more point-like and decoherence time can be increased.

Alexander Valishev raises a question about restoring machine parameters with small data samples, using the data of turns with good properties for example with the presented machine learning method. He points out that this would be an interesting area of research.

Yannis Papaphilippou adds the possibility of having multiple well synchronized BPMs in order to measure other quantities.

Giuliano Franchetti refers to the possible opportunities for measuring space charge and points out that it would be nice to think of a program of measurements to analyze optical functions of space charge dominated beams and how they scale with f. ex. the intensity of brightness. Furthermore he remarks that if there are tools that can measure optics functions to 1% as presented by the speakers, it would be good to apply them for measuring space charge optics distortions, also for rapid cycling synchrotrons.

Valeri Lebedev comments that direct space charge does not change turn-by-turn or LOCO, because there is not interaction between the beam and the vacuum chamber, so this is invisible in the optic functions measurements. But the effect of direct space charge on the focusing could be measured differently, f.ex. through quadrupole motion of the beam.

Giuliano Franchetti points out the difficulty of measuring optical functions for intense beams, for example intense gaussian beams.

Xiaobiao Huang adds that the measurement of effects due to space charge are possible and mentions experiments at the Fermilab booster where the tune shifted with the intensity of the beam.

Adrian Oeftiger points out that from second order perspective direct space charge effects could be measured via the change of the quadrupolar moment along the BPMs. These would be measurements not for the phase advance of the centre of mass, but for the envelope of the beam.

SY Lee adds that there are two kinds of space charge tune shifts, namely a coherent and an incoherent one. The coherent tune shifts aspects are important in low energy machines, but are expected to be not very large in high energy machines unless it is a very high intense beam.

Giuliano Franchetti adds the comment, that to interpret measurements correctly one needs a model or theory behind it, which becomes difficult with complex distributions.

Valeri Lebedev mentions that the quadrupole BPM measurements can be done with Ionization Profile monitors (IPM) which has been tried several times but causes other problems. Giuliano Franchetti adds, that they are also less precise.

Valeri Lebedev continues with an additional comment to the measurement process: the centre and the tail of the bunch are affected differently by space charge, though everything is measured. Furthermore he agrees, that there has to be a model for interpretation.

Vladimir Shiltsev agrees with Valeri Lebedevs comment to the effects on the centre and tail of the beam and points out that it is nevertheless possible to measure these different effects. In addition he remarks that research on beta function distortion due to space charge are necessary.

Giuliano Franchetti points out that the whole phase space matters to measure the effects on optics. For a single particle the phase space can be analyzed, but for the full beam with IPM there is no method.

Adrian Oeftiger remarks that quadrupolar moment measurements along the bunch profile is possible, so the information about transverse coherent second order moment with respect to longitudinal position is accessible already. Moreover he points out that the idea of measuring envelope phase advance as mentioned by him before, is connected to having quadrupolar BPMs around the ring which is not the case today.

Frank Zimmermann comments that a similar effect to space charge, but with a localized source, beam-beam also changes the beta function and optics at least for the core particles.

Tatiana Pieloni replies to Frank Zimmermann, that measurementsd of the beta beating due to beam-beam have been done at LHC with the AC dipole. Due to different effects on the different particles in the distribution the results are not obvious to interpret, so simulations are needed to determine what is expected to be seen. Tatiana Pieloni shares a link to the related paper.

Frank raises another comment to space charge, that there seem to be two kinds of beta functions, on regarding the envelope and one regarding the betatron oscillation of the beam, for space charge does not effect the dipole motion, but the quadrupole motion. **Adrian Oeftiger** replies that there is also a dependence on the longitudinal position.
Xiaobiao remarks that there are different effects of space charge on beam size and beta function distorsion and how to distinguish between the change of the beam size due to the beta function and the emittence growth. The beta function distorsion will decrease with the beam size while the emittance will continue to grow.

SY Lee mentions an experiment that was done around 1990 and that was not published, where a several meters long beam bunch was analyzed in very detail, measuring position and tune for every beam slice. It changed from a cigar-like shape in the beginning to a skrew shape at the end and eventuelly smeared out. The tune shift dependency on the position of the beam could clearly be seen and it would be interesting to do this kind of experiment again.

Vladimir Shiltsev comments that Alexey Burov and Valeri Lebedev measure intrabunch dynamics in the FNAL booster on a regular basis with very interesting results, which Valeri Lebedev answers with some words about the available technique to track each beam slice in high prescision, for example in an experiment about transmission stability near transition.

Chandra Bhat adds a comment on S.Y. Lee's remark on long bunches, independent on space charge topic, and refers to an experiment that has been done and published, observing the synchrotron spectrum, respectively frequency spectrum using barrier RF buckets.

Yannis Papaphilippou finally finds summarizing words to conclude and close the session.

2 June 23d

Scientific secretaries: G. Russo - T. Prebibaj

2.1 Marriage & divorce of "Feedbacks" with "Landau damping", transverse

Speaker: Nicolas Mounet, CERN
Scheduled: June 23d, 2.30 pm - 3.00 pm

Nicolas Mounet goes through his presentation, the aim being to presents pro and cons of feedbacks with Landau damping. At first the case of coherent instability is presented. It will induce a complex frequency shift that can be counteracted with a damping exponential. From a practical point of view, this action could be achieved with a kick with intensity proportional to the exponential and shifted of $\frac{\pi}{2}$ in phase. Then the mathematical model of the coherent instability is described. Feedbacks are then introduced and presented in the real context for LHC and in for PyHEADTAIL simulations. A small regression is performed on the possibility to use them as a way to compensate the real component of the frequency shift and as a way to create instabilities as well. Then the focused is moved to Landau damping, in particular to how sometimes tune spread could have detrimental effects. The topic is further analysed considering the non-linear chromaticity as a possible driving term for transverse coherent beam instability and, separately, modifications of the stability diagram. This overview has been performed to give an idea of the complexity of the reality and therefore, to explain the need of macroparticle simulations or the use of Vlasov solvers in order to account for all these effects at the same time. A brief explanation of Vlasov equation is performed in presence and absence of feedback. As a conclusion it is shown that also in the general case it is possible to identify a stability diagram and the theory on which it is based. The last presented result shows the dependence of the stability diagram on the chromaticity, and it has been highlighted how this feature is new with respect to the previous well-known stability diagram.

Ingo Hoffmann asks to what extend is it possible to test the Vlasov and feedback modeling that was presented using macroparticle computer simulations. Nicolas confirms that this is possible but a large number of macroparticles is needed and the number of simulated turns is limited. As a consequence, the stability of the beam cannot be certainly assessed due to possible latencies.

Rogelio Thomas wants to know how the definition of the dispersion integral changes if the linear and second order chromaticity are included in the model. Nicolas points out that in the case of the second order chromaticity there exist two effects: the first one is the modification of the modes, which means that different modes give different tune shifts (growth rates), and the second effect is a stabilization effect from the Landau damping. The two effects are mixed and is hard to distinguish one from the other.

Tatiana Pieloni asks a clarification on the use of the macroparticle simulations. She points out that some features of the stability diagram (i.e. slides 24, 25) can be detected with not so many turns or macroparticles and Nicolas agrees but explains that other features might appear only after a large number of turns.

Frank Zimmermann asks if the theory of Y.H. Chin is the most general available and Nicolas specifies that the model presented here went a bit beyond that because the ideal bunch-by-bunch damper and the 2D detuning have been added. Further on, Frank asks if space charge can be included in this formalism. Nicolas mentions that it is desirable but he is not sure how. Elias Metral comments that he tries to implement space charge in the code GALACTIC. Regarding this issue, more details will be given in his presentation "Mitigation of TMCI through space charge?" on Tuesday 30/06.

2.2 Landau damping in longitudinal plane for single or multi-bunch beams

Speaker: Elena Shaposhnikova, CERN
Scheduled: June 23d, 3.00 pm - 3.30 pm

Elena Shaposhnikova goes through her presentation explaining the effect of Landau damping in longitudinal plane for single or multi-bunch beams. She starts with an overview of the problems: in general Landau damping is lost when the coherent bunch frequency falls outside the incoherent frequency band that is modified by the beam-induced voltage. In the longitudinal direction the Landau damping of coherent modes is achieved increasing the synchrotron frequency spread. At CERN two different techniques are used: controlled the emittance blow-up and multi-harmonic RF systems. At first she analyses the case of Longitudinal Landau Damping (LLD) in LHC. During acceleration an instability was detected and it has been explained showing that LLD is insufficient to keep the beam stable, this condition has been achieved controlling the beam emittance or, equivalently, the ratio voltage to number of particles per bunch. Elena then shows that some experiments have been carried out and they agree simulations. Similar conclusions have been obtained also for the SPS ring, where the full impedance model was adopted). Then she describes the criterion for the loss of Landau damping and how it is possible to obtain analytical solutions for the coherent tune shift using the Sacherer and Hofmann-Pedersen formalism for some distribution functions. Comparing these analytical results with measurements, it is shown that there is a big discrepancy (factor of 3-4) between the two. It is therefore mandatory to use more accurate methods based on the Van Kampen Modes or Lebedev equation. Both methods give the same threshold and a good agreement with measurements can be observed for a bunch with $\mu = 2$. Then the talk's focus is put on double RF systems. At first the single bunch stability is studied, then bunch-shortening-mode is analysed and at the end multi-bunch beam is taken into account. Further considerations on intensity effects and controlled emittance blow-up are performed.

Alexey Burov comments that the physical mechanism of Landau damping is related to the energy transfer from coherent to incoherent motion of resonant particles. This should help to understand why the derivative of the synchrotron frequency (ω_s') equals to zero; in that area of the phase space, particle motion is dominated by macroparticle behaviour and there is not a real frequency spread. Therefore,coherent motion of particles does not lead to energy transfer to incoherent motion. He continues by asking why there is a non-monotonic behaviour of the threshold in the BS-mode that was shown in slide 19. Elena says that the reason for this it is not well known yet and maybe there are some features of this behaviour that need to be further studied.

2.3 Injecting ripples on beam: shall we?

Speaker: Claudia Tambasco, EPFL
Scheduled: June 23d, 3.30 pm - 4.00 pm

Claudia Tambasco goes through her presentation. In order to measure the tune, the beam has to be excited with a frequency sweep signal. CHIRP excitation allowed to have a fast measurement of the tune but it also lead to emittance blow-up and related losses. Therefore, Beam Transfer Functions (BTF) have been tried: in this case the frequency of the signal has been increased in steps. With this technique it is possible to measure the tune and chromaticity as well as beam-beam coupling and coherent modes. Some measurements have been carried out at RHIC in order to try to compensate the beam-beam tune spread with e-lens.

Frank Zimmermann comments that maybe we can learn something more from quadupolar ripple excitation similarly to the work of O. S. Bruning and F. Willeke of May 1995, where they used an external tune modulation to improve the beam lifetime and they suggested that the same principle can be used to control the diffusion and the slow extraction schemes. Claudia agrees.

Valeri Lebedev mentions that by considering intrabeam scattering then any ripple would result in loss of instability, because after the ripple the beam distribution function is changed. But if the perturbation is small enough relative to intrabeam scattering then the beam is stable. Furthermore, Valeri asks for the frequency that was used for the excitation which was around 3 kHz. In addition, he asked if they tried to simulate higher frequencies to excite intrabeam motion and Claudia answered that they have not tried yet.

— 30 min Virtual Coffee Break —

2.4 The curse of transition crossing

Speakers: Chandrashekhara Bhat & Kiyomi Seiya, FNAL
Scheduled: June 23d, 4.30 pm - 5.00 pm

Chandrashekhara Bhat goes through his presentation and he gives, at first, a brief overview of Fermilab accelerator complex and it's role in the history of transition crossing. Then, a bit more detailed explanation of the physics of transition crossing is performed, with a special highlight at the possible issues (Johnsen's effect, microwave instability, ...). Therefore, the different effects are analysed. At first he presents the beam losses due to large momentum spread, non-adiabatic and non-linear effects are then considered. Microwave, negative mass instability and Umstatter's effects descriptions follow the previous topics. Thus, γ_T-jump scheme is proposed as a possible mitigation technique. The key features of this technique are highlighted and the results of experiments at Fermilab are presented. Chandrashekhara shows also the limitations of this approach. The transition crossing in the Booster (RCS) is presented with experimental results. The same rationale is adopted also for the Main Injector, with the difference that in this case numerical simulation results are shown. As a conclusion, he summarize the current and future plans for the transition crossing in the two machines presented before.

Alexey Burov says that it is worth to mention that measurements from last year in the Booster detected that convective instabilities constitute a limitation in the intensity. In order to overcome this limitation a proper optimisation of the chromaticity and damping pattern in the transverse plane is needed. Chandra agrees that these effect will have to be analyzed in detail if they want to reach higher intensities, but for the moment it is not known how far they can go.

Alexandre Lasheen mentions that apart from the broadband component, also the high frequency (above the GHz level) resonant impedance must be taken into account, since it was observed to be a source of instability in the KEKB. Similar instabilities have been observed in more recent measurements in the CERN PS ion runs. Chandra replies that the Booster does not have any beam pipe so the beam is directly exposed to the magnets. Therefore, the modeling of the microwave instabilities it is not an easy task. In the case of the main injector this is not a big issue, for the current intensities.

Elena Shaposhnikova asks what are the difficulties related to the modeling: the absence of a accurate impedance model or simulation tools. She remarks that in SPS there were also instability problems when ions were crossing transition, which has been faced by applying higher harmonics in the RF system. Shes asks if something similar has been considered. Chandra remarks that the Booster is a 50 years old combined function synchrotron, so there were not any available data for the magnets. This fact, in combination with the absence of a beam pipe, makes the modeling of the machine very difficult and so they can only rely on beam-based measurements, rather than direct modelling. On the other hand, the main injector is a much more formidable machine and it was originally designed as a high intensity accelerator. The injector is easier for understanding the modelling.

2.5 Shaping injection - painting, laser stripping, etc.

Speaker: Sarah Cousineau, SNS
Scheduled: June 23nd, 5.00 pm - 5.30 pm

Sarah Cousineau starts her presentation explaining how charge exchange by the use of a stripping foil works and how the resulting beam can be used for painting. The main constraint is the reduction of circulating beam traversals, then the priority is given to the obtain the required distribution, control of collective effects and in the end to optimize the hardware. Several measures can be adopted in order to reduce the hits on the foil at high power: offset between injected and circulating beam and fast removal from the foil at the beginning are the most common. Measurements to determine the failure limit of the foil were performed, as a consequence the maximum power limit of the beam was computed. A strong constraint of the technique is the radiation level close the injection area that could possibly limit the power level. Therefore, Sarah introduces LACE (Laser Assisted Charge Exchange) as an alternative way to strip electrons from the H^- beam. Experiments proved the 95% stripping efficiency of this technique. Moreover, the possibility to provide power in a sequential way to excite electrons makes LACE operationally feasible. Shoe concludes her presentation with an overview on the next steps to be performed: complete design of real injection concept and try laser stripping for real injection in the ring.

Valeri Levedev asks if during the foil temperature measurement, optical transition radiation coming from the foil is detected. Sarah answers this radiation has been taken under consideration but it is not affecting the actual measurement.

Robert Ainsworth asks how far they can go in terms of power levels considering the radiation constraints. Sarah replies that it is hard to say; probably power no more than triple. She further comments that the problem is that the main shutdowns are too short to cool that region off enough to change the foils. So it really becomes a question of how often do you need to change the foils and how good is your remote foil handwork capability; in other words how well you can engineer the system so that you do not have to be there. She continues saying that sometimes they have a whole shutdown that goes for a month or a month and a half, during which they cannot get in at all. So far, they have been doing three major shutdowns per year as of target issues; they hope to go down to two longer shutdowns so that would mediate the radiation levels and allow the region to cool off. In the end, she concludes that going to higher power levels is a combination of energy and intensity, so it is not really a true doubling in the radiation levels at the injection region. Some of it is coming from the energy, which actually benefits the multiple Coulomb scattering to a degree. The variation in the intensity has not been performed yet, but the machine is still expected to be serviceable.

Dima El Khechen wants to know how wide is the spectrum of the laser that is used and Sarah replies that the laser wavelength must be very narrow in order to efficiently excite the electrons. Moreover, she asks if the laser field extracted after crossing the beam and Sarah confirms that indeed the laser power is being measured but there a high sensitivity on the photon reflections that occur in the beam pipe.

Alexander Krasnov mentions that the laser stripping is used to produce neutral beam for plasma heating. It would be interesting to compare the crossing techniques between laser beam and ion beam with multi-crossing.

Sergei Nagaitsev mentions that the painting scheme that is proposed assumes that the space charge forces are linear for self-consistent beams which would mean that theoretically the machine tunes can be adjusted so that the working point can remain the same regardless of the beam current. He wants to know what are the limitations of this idea: adjusting the ring focusing globally so that the footprint should remain compressed even with very intense space charge. Sara says that theoretically seems to be possible but still studies on self-consistent beams at resonance and their manipulations have not been performed yet.

Giuliano Franchetti wonders how to create a self-consistent beam. In theory, it seems possible but in real life if the way of painting is not perfect while constructing the self-consistent distribution there could be some problems. Sarah says that in reality self-consistent beams aren't perfect and in simulations some forensic machine errors are considered, as it has been done in the past, and they look quite robust against the imperfections existing in the machine.

Dima El Khechen asks how good is the synchronization between the beam arrival and the resonance and which are the challenges from the electronic and timing point of view for the next sequential resonance step. Sarah answers that timing shouldn't be an issue since some measures have been performed with a very precise timing in the orders of MHz micro-structure and the same structure is imported on the laser, so both have a very precise pulse micro-structure. In practice then, they're aligned on a timing master signal (that is the reference for the whole accelerator), the last issue to fix is related to the phase offset that is always present when dealing with laser stripping. This is performed making a scan of the phase of the laser until it is possible to observe the ongoing stripping phenomenon.

2.6 Discussion Session

Valeri Lebedev asks about longitudinal feedbacks in LHC to **Elena Shaposhnikova**. She answers that there is just a longitudinal control to avoid beam loading and this is not a real feedback. Therefore Valeri asks if it is a control on the RF rather than a bunch-by-bunch feedback; thus Elena replies that in the past the combination of large safety margins and Sacherer' criterion brought to conclusion that stability will be provided by natural (Landau) damping. But in reality Sacherer' overestimates of a factor of four. Thus, Valeri further asks if there are any future plans to put feedbacks in LHC and Elena says that no, it has been decided to slightly increase the bunch length because it's the most efficient way to increase stability. Moreover, since LHC is a single-bunch effect machine, an additional problems is caused by the fact that all bunches are different, this is the additional reason why shorter bunches are needed in order to have stable beam. Then at flat top the bunches are shrinking due to synchrotron radiation so there is the need to blow-up a bit the beam when performing physics in order to avoid instabilities.

Valeri Lebedev tries to answer to the question "What should be the next generation of fast beam feedbacks?": a modulated pulse would induce an excitation of higher order modes by a factor of two. In other words, it would require a factor of two small values in the octupoles. This is a big deal and it is possible to solve it for LHC. **Frank Zimmermann** replies that there is one intra-bunch feedback that has been developed for LHC but for FCC. It would be desirable to have a feedback that can damp the resistive mode instabilities over few turns. A fast feedback is needed both for the electron and lepton beam; in terms of time is quite feasible since it's a quite long time, but in terms of turns, it is just a few turns so maybe a different feedback system to damp this instability should be considered. **Valeri Lebedev** then replies that he doesn't mind to perform intra-bunch feedback but the complexity and the cost of such an approach are incomparable. **Frank Zimmermann** agrees with this last comments and he adds that for FCC a system able to damp low order modes within few turns is needed. It seems a feasible option but present systems are not really suitable for this type of damping. There are different suggestions from the scientific community: to just install many systems of the present type and damp within few turns but it might not be the best approach; or try to develop a new system with a narrow frequency to directly focus on the mode. **Valeri Lebedev** continues saying that it would be interesting to look at beam response functions measured at high frequencies which would touch intra-bunch motion. LHC instabilities are not given by lower order modes but rather by higher modes which are not suppressed by dampers but rather excited by the dampers themselves. It would be interesting to collect some experimental data but it requires some extra hardware work in excitation and in pick-up. The latter would probably work asap, but excitation would need a special kicker. **Frank Zimmermann** LHC has some different requirements from FCC. FCC is a very big machine with a very low revolution frequency and it would be desirable to damp the low resistive mode multi-bunch instabilities which have a very short rise time if expressed as a function of the number of turns. **Valeri Lebedev** continues saying that damping one mode will induce the excitation higher order modes. **Frank Zimmermann** asks **Vladimir Shiltsev** if a similar feedback for the SSE since it has a similar revolution frequency. But such a technique hasn't been developed yet. **Valeri Lebedev** adds an additional comment on intra-bunch damper: the harmonic kick in the main damper doesn't excite intra-bunch motion. For FCC if you join damping rates it makes a really strong kicks.

Robert Ainsworth has a question about the transition crossing at J-PARC, in particular if there are any drawbacks for having a imaginary γ_t for future lattice machines. **Elena Shaposhnikova** says that a long discussion on this topic has been done with **Valeri Lebedev** on the feasibility or not of lattices for future machines with imaginary γ_t or they should cross transition. **Chandrashekhara Bhat** J-PARC usually works a bit below transition not exactly with imaginary γ_t. At J-PARC they don't have imaginary γ_t lattice in a conventional sense. They don't have $\gamma^2 = -1$. **Shinji Machida** clarifies some features of J-PARC: it has nearly imaginary γ_t and momentum compaction factor negative. **Elena Shaposhnikova** asks if there could be any problems in running with imaginary γ_t. **Shinji Machida** replies that it is difficult to give an appropriate answer since he left the research centre long time ago. But he says that before making the design of this lattice, high intensity beams in this type of machine was a source of concern. The moderation of the dispersion and therefore the change in the β-function could cause the unnecessary excitation of resonances which effects could be relevant for high intensity beams. But, at present, it doesn't look anymore as a real problem right. **Chandrashekhara Bhat** adds a comment on the imaginary γ_t lattice, he refers to a talk during the main injector design period 1995-2000: it has been considered to use imaginary γ_t lattice for the main injectors, but for some reasons related to the lattice it wasn't really feasible. So this ideas has been abandoned. **Elena Shaposhnikova** agrees with Chandra and she further comments that a similar ideas was considered at CERN for the accelerator that was supposed to replace the PS. But it has never been built. The studies that were performed were used later on to change the γ_t in SPS and to improve beam stability of LHC to low down γ_t without the installation of any equipment.

Alexey Burov refers to the talk of **Nicolas Mounet** were dampers drive instabilities and also **Valeri Lebedev** mentioned it; so for this specific case of phenomena, he wants to mention that space charge is also part of the phenomena that should be considered. A damper (conventional bunch-by-bunch damper) could provide additional source of instabilities because of feedbacks from the tail to head which are suppressed by space charge but the damper could bring it in and transform the relatively moderate convective instability into absolute instabilities making therefore the situation worse. This is an additional issue that has to be taken into account when dampers are considered. And also, related to **Elena Shaposhnikova** talk he would like to know, in particular about slide 19, why there is a non-monotonic behaviour in the threshold of the number of protons. There is a change in the emittance of almost one order of magnitude, so he wonders if the results were reproduced using Van-Kampen or Lebedev approach. **Elena Shaposhnikova** answers that during simulations any form of self-stabilization from adjusting the distribution it's not easy to detect. The physical interpretation underlying behind the phenomenon is that while approaching the threshold from one side you don't have particles enough on the other side to make derivation of distribution function zero. because in threshold you have distribution function derivative divided by derivative of synchrotron frequency, so you can see that synchrotron frequency distribution derivative goes to zero at the centre of the bunch but then you usually have also zero from distribution function. And when it is happening on the tail and you're just approaching the threshold, you don't have particles on the other side of this singularity that could compensate. Moreover, when you pass over it then a small change in distribution function can stabilize the beam and maybe it's not even visible as a threshold but it should be checked again. **Alexey Burov** asks if a small change in the distribution function may actually remove the instability and this may be the reason why you have such a strong dependence because it's not only an issue of the threshold but also if the threshold is removed by small changes in the distribution it's not effectively a threshold. **Elena Shaposhnikova** answers that the threshold that was observed in simulation is real, what was not seen was the mechanism of how it was happening. **Theodore Argyropoulos** agrees with Elena and adds that the threshold depends on the incoherent higher order frequency distribution which means increasing the bunch maybe also the synchrotron frequency distribution. **Elena Shaposhnikova** and **Theodore Argyropoulos** agree on going back on this topic and figure out the real mechanism underlying behind it. **Alexey Burov** insists on the fact that the dependence is very strong on the threshold. **Elena Shaposhnikova** continues saying that it is very important since in SPS and future machine relies on running with higher harmonics RF systems and we try to avoid this region by all means if it could run well above then it will give us completely different situation. **Alexey Burov** it would be interesting to also confirm it with Van-Kampen or Lebedev approach just to be sure that there is no uncertainty. After you demonstrated such a beautiful agreement between Van-Kampen and Lebedev approach then we may rely on both of them. **Theodore Argyropoulos** says that they will cross check the Lebedev and Van-K with a new approach in all the SPS and other machines. **Ivan Karpov** concerning the name the mode the going outside the incoherent blend and then it is a discrete mode which was considered by Van-Kampen many years ago so in principle, the interpretation of Lebedev equation hasn't changed, so in principle the method (i.e. the Van-Kampen interpretation of Landau damping) the differences that Van-Kampen uses normal-mode expansions, while for Lebedev we just solve it directly.

Chandrashekhara Bhat further comments on convective instabilities, some measurements induced the convective instabilities in the Fermilab booster and he wonders if something analogous has been performed at CERN as well. **Alexey Burov** answers that for the PSB some observations were made by **Valeri Lebedev** just by making zero chromaticity. The same in all accelerator except in LHC where there is not a strong space charge.

3 June 24th

Scientific secretaries: T. Prebibaj - G. Russo

3.1 Keeping the GSI/FAIR Vacuum "high"

Speaker: Andreas Kramer, GSI
Scheduled: June 24th, 2.30 pm - 3.00 pm

Andreas Kramer presents the vacuum techniques used at GSI/FAIR. He discusses the various concepts of how vacuum is characterized and how it affects the beam quality. He goes through the beam vacuum requirements of GSI & FAIR with more detailed descriptions of SIS18, ESR, SIS100 and HESR.

Wolfram Fischer asks what is needed to further reduce the pressure in the SIS18 by 3 or 4 orders of magnitude, compared to the current value of 10^{-11} mbar. Andreas replies that SIS18 is a 30 years old machine, so the older pumps must be substituted because they are not sure about the quality of the magnetic fields. Moreover the average pressure is not the only problem because there are spots where the pressure is slightly higher than expected; a more detailed check will be made to locally increase the pumping speed.

Frank Zimmermann comments that the target vacuum at SIS100 is 1000 times better than the LHC: the hydrogen gas density is to be below 10^{12} m^{-}3 (slide 18), while LHC was designed for 10^{15} m^{-}3. He asks what features would the LHC need to have in order to reach these values. Andreas replies that he needs to cross-check these values.

3.2 Strategies for electron cloud mitigation at future accelerators

Speaker: Roberto Cimino, LNF-INFN
Scheduled: June 24th, 3.00 pm - 3.30 pm

Roberto Cimino's presentation begins with an introduction to the importance of the vacuum systems and the e-cloud effects on the beam quality and lifetime. He continues by presenting the general e-cloud mitigation strategies, focusing mostly on the geometrical modifications and the intrinsically low SEY materials. Examples are shown from LASE/LESS. He concludes that in general all new materials should be validated in a global approach and their studies must be done as close as possible to operational conditions.

Robert Ainsworth asks: "What experience do you have of using amorphous carbon with beams". Roberto replies that there has been lot of testing in the CERN SPS done with carbon coated beam pipes and there they could not observe any e-cloud. There have been some vacuum pilot sectors that have been experimented with carbon and this is the baseline scenario for both CERN SPS and HL-LHC. He highlights that the issue with the impedance is hard to be defined a few meters of beam pipes are covered with carbon over a few kilometers machine. This impact on the impedance is still an open issue because it is difficult to scale what you measure in one meter with what you will observe in 100 kilometers of carbon coated pipes. And it depends of course on the thickness of the coat. He suggests that would desirable to try to develop methods to coat with a thickness as low as possible. **Elena Shaposhnikova** adds that there were a lot of measurements to try to evaluate the impact of coating on the beam and the conclusion is that is absolutely acceptable to use it for high intensity beams in the SPS. Also, even during this long shutdown period, parts of the SPS are being coated so there are anticipated many more results when the beams are again available.

Frank Zimmermann asks for an explanation of the choice of temperature for the beam screen. **Roberto Cimino** the aim is to keep the temperature as low as possible to maintain low impedance. In fact, impedance or surface resistivity increase with the temperature, so the higher is temperature the worst is the cooper impedance. This suggests to keep the temperature as low as possible, the main drawback is the economic aspect of such a choice; therefore an intermediate temperature has to be found. Referring to the diagram at slide 18, Roberto makes a practical example: assuming to have cooling system giving a pressure of 10^{-11} Torr at a temperature of 2K all the Hydrogen sticks to the beam screen and it gets absorbed, but as soon as the beam circulates, either the pressure increases a bit or the temperature fluctuates, then an immediate release of the gas that has been absorbed occurs. This will be a huge contribution to the pressure in the machine and will induce instabilities. If any fluctuation in either pressure or temperature will bring the gas density above or below the isotherms lines then: if below, the gas get stacked to the surface and will be pumped away by the system; but as soon as the working point is moved above the lines then the gas is released into the vacuum chamber where the beam is circulating and this will cause instability.

Wolfram Fischer how to choose between the options available to suppress e-cloud (in a practical machine there is a difference between whether there is a localized problem like triplets for example or if there is the need of global solution, which can be many kilometers). What would be the best solution for the two cases? **Roberto Cimino** answers that it is still on-going research. If after some studies on how e-cloud could affect the performance of the machine on the long scale, it is possible to assess that e-cloud only occurs in 1m then the only concern could be to try to reduce the heat load. If the aim is to improve conductivity a possible solution would be high-TC coating to reduce impedance, assuming it works without vortex, a low secondary coating to guarantee low electron cloud is also needed. For sure it is a complex and elegant solution to reduce impedance and secondary electrons. At the moment it's difficult to say which is the best solution since it should also be the most affordable solution and the most implementable one. It also depends on the geometry of the system, eg. coating is more easy on a simpler geometry and this is true also for laser borification. If position where e-clouds are localized is known, then it is possible to put stripes there (this is also a solution that is under evaluation for Fcc top and bottom). Therefor, there is not a general good solution, there are several solutions that could be considered according to the type of issues you're trying to solve.

Valeri Lebedev asks for the diagram in slide 18, in particular how temperature, pressure and density of the gasses are related one to each other since the density is changing in time due to the removal of atoms. **Roberto Cimino** answers that cooling down the system the density decreases, as well as the pressure, in the chamber, but the number of molecules that will stick to the wall depends on the temperature.

— 30 min Virtual Coffee Break —

3.3 Taming the high intensity beam-beam & impedance

Speaker: Xavier Buffat, CERN
Scheduled: June 24th, 4.00 pm - 4.30 pm

Xavier Buffat goes through his presentation. He highlights the importance the beam-beam and impedance repercussions and how these affect Landau damping. The mode coupling instability of colliding beams is presented with examples at the HL-LHC. He concludes that the impact of beam-beam interactions on amplitude detuning can be both beneficial and detrimental for Landau damping and the interaction of coherent beam-beam modes with the machine impedance can lead to mode coupling instabilities.

Sarah Cousineau asks if weather or not there are any plans to experimentally validate the idea of separation bump parallel to the crossing angle. Xavier replies that at first he needs to make sure that there is no issue with the convergence of the model and be clear which ones should be the observables. If this is confirmed they will proceed with the experiments. Sarah asks if they have all the required hardware. Xavier mentions that in principle it is very easy to achieve this since there are already bumps able to correct the position of the beam in both planes.

Wolfram Fischer asks about the observed instability which has a raise time of several seconds (slide 52): "What is the range of the observed rise times?". Xavier answers that in the LHC for single bunch cases it is expected to be in the order of seconds (approximately 10000 turns) and then with high chromaticity and the damper it can be even slower. So, all the instabilities that are impedance driven and those coupling with the beam-beam are of the order of seconds.

Alexey Burov discusses about the long staging problem of loss of Landau damping while adjusting the beam. He highlights that in octupoles, whatever is their polarity, in one moment or another Landau damping will almost be lost. He explains that this problem can be avoided in electron lenses and he asks for more clarifications on the approach in the octupoles. Xavier replies that you can avoid this problem and proceeds in showing an example on slide 25. In this example, at the separation you have not fully suppressed Landau damping and if octupoles are used, the situation can be improved a bit. For the LHC there are many solutions with the octupoles and for the HL-LHC they seem to be rather at the edge because of many constrains, but there are solutions as well. He agrees that using an electron lens the situation will be eased in many aspects.

3.4 Taming the high intensity space charge

Speaker: Hannes Bartosik, CERN
Scheduled: June 24th, 4.00 pm - 4.30 pm

Hannes Bartosik goes through his presentation. Space charge induces tune spread which in combination with resonances can limit the machine performance. Measures to mitigate the space charge effect include the optimization of the working point, bunch flattening for reducing the tune spread and resonance compensation using corrector magnets. Examples are shown from CERN PSB, PS and LEIR.

Adrian Oeftiger asks if the bunch flattening in the LEIR with the adiabatic capture using the slightly off-frequency is operationally being used. Hannes replies that this has been used operationally in 2016 and it worked, however it turned out to be slightly sensitive for reproducibility. In the end, Simon has developed a slightly different scheme which does not look as spectacular as the one shown in slide 27, but is operationally more reliable and results to a similar bunching factor.

Alexey Burov comments that there are two more measures for space charge mitigation that in principle should be possible. The first on is space charge compensation using an electron lens. Hannes replies that he did not mention anything because there will be another talk dedicated to this subject. The second one is the circular mode instead of planar optical mode. If there are two different emittances, one much smaller than the other, in the circular mode only the bigger emittance is responsible for the space charge tune shift. This way in principle one can increase the brightness in terms of the smaller emittance, which can be reduced significantly. Hannes acknowledges that the list of methods that were presented is not exhausted because he tried to show only the methods that are used operationally.

Robert Ainsworth mentions that the use of sextupoles to compensate resonances can excite other third order resonances, so he asks how this is avoided in the cases presented. Hannes agrees that by curing some resonances, other might be excited and he shows an example from the PS Booster (slide 27). An ideal resonance compensation can be done only if enough correctors are available and the sources of the errors are exactly know. In the example shown, the tune diagram area that the working point is located during operation is resonance free and is far from the resonance excited by the sextupoles, but in general indeed this is an important issue. Robert continues by asking if the same settings are used also for the high intensities. Hannes mentions that the resonance identification and compensation is done using a low intensity beam and after the same settings are used also for the operational intensities. After the machine upgrade, more detailed studies are planned for this.

Giuliano Franchetti asks about the brightness curve of the PSB (slide 7): he wants to know if the linear emittance increase with the increase of the intensity is due to the integer resonance crossing. Hannes confirms and continues by adding that in the PSB this happens not only because of quadrupole errors but also because of the fourth order resonance due to space charge (PSB has a periodicity of 16). Giuliano continues by adding that it would be interesting to test how the intensity can effect the resonance compensation in the case of space charge driven resonances because then it is not an actual resonance compensation but a compensation of a driving term. Hannes points out that this is true since the phase advance between the error and the corrector might slightly change. This will be addressed in a more systematic way in the future.

Chandra Bhat asks if a double harmonic RF system can be used during beam capture in the case of the PS. Hannes replies that technically it is possible but this has not been studied extensively for operational use because it is quite complicated to achieve synchronization between the two machines and then be sure that the bunches are matched in the double harmonic system. This could be tried in a later stage but the operation will not be relied on this after the upgrade. Chandra mentions the case of the PSB on which the acceleration is done with a double harmonic system. Hannes confirms that PSB is operated with a double harmonic but notes that at the end of the cycle the second harmonic is used only to slightly reshape the bunch so practically the LHC beams are at $h = 1$. Finally, it is mentioned that some test were done in the PS to inject with double or even triple harmonic and some transverse instabilities were observed. This was not a dedicated work and this will probably continue in the future.

SY Lee comments that is it more effective to correct the half-integer resonance instead of correcting the beta-beating. Hannes replies that so far PSB had a classical betatron multi-turn stuck injection. Therefore, what has been done operationally up to now in the Booster is to cross the half-integer resonance with a low intensity beam, choose some orthogonal correctors for the half-integer driving term and use the strengths that minimize the beam losses. This way, the high intensity beams were injected above the half-integer and after the injection the tune was was moved below the half-integer. This was possible because the optics distortions from the lattice were very small. Currently, PSB has received an H^- injection system and so strong perturbations are expected to be present, so it is not clear so far if the half-integer will be fully compensated. SY Lee also mentions that the fourth order systematic space charge resonances have also been observed KEKB and they cannot be avoided.

Sarah Cousineau needs a clarification about slide 16: she asks if they are considering tails halo that are of the order of $10^{-3} - 10^{-4}$, since they do not cause beam loss. Hannes replies that in general at CERN the apertures are huge compared to the produced beam. Therefore, in the LHC they are not concerned much for halos in the sense of losses but rather for a degradation of the emittance, so these levels are not acceptable by no means. Giuliano adds that the halo definition is controversial and small halos are really hard to measure. Finally, Sarah mentions that for them, the definition of halo is the particles which cause beam loss in a regular operational cycle.

3.5 Discussion Session

Discussion about the e-cloud.

Giuliano Franchetti mentions the periodic crossing of resonances due to space charge that Hannes was describing; he comments that something similar might happen with e-cloud. There was a suspicion that in the LHC there is still some residual effect of incoherent-nature e-clouds. He wonders if this going to be more important of not for HL-LHC?

Elias Metral confirms that in the LHC there are many incoherent e-cloud effects during the betatron squeeze and it is being analyzed in detail in close collaboration with the e-cloud experts (Gianni Iadarola et al.) and incoherent effects experts (Yannis Papaphilippou et al.). When the crossing angle is reduced during the betatron squeeze, there can be clearly seen effects of e-clouds. This subject is very important and of high priority and it has for sure to be taken into account for the future.

Xavier Landau argues that this is already an important issue for HL-LHC because there is the coating of the triplets and it is very likely that for the FCC a similar solution would be used.

Valeri Lebedev discusses super-periodicity on synchrotrons.
He notes that for super-periodic synchrotrons we do not need to compensate high order resonances: essentially nothing has to be taken into account after the fourth order. So if a machine is built to be super-periodic from the beginning, then the tune shifts due to space charge account to the period and not to the entire ring. He mentions as an example the J-PARC. The question he raises is to which accuracy this must be done: what optics must be created in order to have less than 5% beta-beating or even smaller (1%). He mentions that for the Booster in the Fermilab, some preliminary simulations show when the beta-beating is below 5 % the machine is in the super-periodic regime already.
Hannes Bartosik mentions that the CERN PS Booster before the upgrade was actually super-periodic with a periodicity of 16 (by lattice design). For this reason the injection could be made above the half-integer, after the correction of some residual optic errors. With the installation of the new H- injection the symmetry broke. The magnets of this injection system create strong edge effects during the dynamic collapse of the chicane. So to the best of his knowledge there is not an H^- injection without optics distortions.

Discussion about the e-cloud.
Chandra Bhat notes that simulations are usually done with a beam pipe but there are cases when there is no beam pipe and the beam is exposed directly to the the inner parts (walls) of the magnets. This is the case of the Fermilab Booster for example. He asks how do we model/understand the e-cloud developments in a machine like that.
Elias Metral says the procedure is similar: you model the vacuum walls that surround the beams with the proper electromagnetic fields that you have. There can be problems when there are 3-D fields, like in crab cavities for example. At the moment there are people at CERN and LBNL working on this, but generally the simulations can be done as well by using the 3-D boundary conditions with the electromagnetic conditions.

Robert Ainsworth follows up on **Giuliano**'s question.
He comments that what was demonstrated is resonance compensation at low intensity but no at high intensity. He continues by shows some plots of tune scans that were done in Fermilab. They tried to compensate the $3Q_x$ resonance with sextupoles. The question is what about high intensities: instead of doing a full 2-D scan to do a single-line scan and start increasing the intensity. These plots show how the intensity affects the compensation: with high intensity the compensation is slightly larger but it is still there. This shows that keeping the same settings works relatively well for high intensities as well.
Giuliano Franchetti asks about how strong is the intensity. Robert answers that is it not huge compared to other machines but it is not negligible as well. **Giuliano Franchetti** points out that he has made some simulations and has seen that you can compensate very well at low intensity, but when you increase the intensity you get some residual effect to remain. This seems to be confirmed by the plots that Robert showed: on the right picture the intensity increases and you basically see that you are a bit out of compensation. Robert agrees but mentions that it is not clear if this is because the machine it is not tuned up correctly of because of a higher order effect.
Hannes Bartosik says that the emittance will increase with intensity and the sensitivity on losses will increase as well. He continues by saying that on the other hand the integer part of the tune is 25 and the space charge tune spread is of the order of 1% of this. So the change of phase advance because of the space charge should be very small I think, so he wouldn't expect a big difference for the high and the low intensity.
Giuliano adds that it is a nice experimental finding and of course it would be very interesting to get it with more intensity to see how robust the compensation can be; this maybe can be tried in another machine.

Giuliano Franchetti refers to the circular mode mentioned by **Alexey Burov**.
He suggests that this would be a good thing to try as a mitigation of space charge. Alexey briefly describes the method: there are two emittances very different to each other. The large emittance stays roughly the same and is the one that determines the space charge tune shift. The tune shift does not depend on the smaller emittance. **Giuliano** asks how practically this type of beam can be generated, if it is relatively simple or more complicated. **Alexey** replies that he does not think it is very simple: not planar but circular optical modes are needed. It requires a special configuration (special optics) of the machine, possibly skew

quadrupoles, help from solenoids etc. It is an open question but hopefully this will be done in IOTA.

Elias Metral discusses about space charge mitigation measures. He mentions that at the 2013 space charge workshop a list was made that summarized all the mitigation measures as an outcome of the workshop. Elias proposes that something similar can be done in this workshop as well: a table can be created that includes all the proposed mitigation methods and whether or not these has been checked with simulations, measurements, what are the proposals for the future and what is the plan for each machine. The rest of the participants agree and an extra slot for a dedicated discussion is organized for the next week.

4 June 29th

4.1 Mitigations for J-PARC upgrade: RCS and MR

Speaker: Susumu Igarashi, KEK
Scheduled: June 29th, 1.00 pm - 1.30 pm

S. Igarashi presents the mitigation methods used at the main ring (MR) at the Japan Proton Accelerator Research complex (J-PARC). A brief overview of the MR design and it's operation modes is given, highlighting amongst others the use of an imaginary γ_t lattice and recently achieved beam power. Various mitigation methods used in the MR are presented, such as the use of a 2nd harmonic RF and correction of the sextupolar resonances using trim coils. In the following, upgrade plans to achieve a beam power of 1.3 MW are presented, including new power supplies and the need for optimizing the working point using simulations including space charge.

G. Franchetti inquires on how difficult it will be to perform tune scans over the wide range of tunes shown on slide 26 from the operational point of view.
S. Igarashi replies that it will be very difficult and will require a lot of tuning. Efforts have recently been made but more beam studies are required.
R. Ainsworth asks if there are any particular issues with the use of the imaginary γ_t lattice.
S. Igarashi replies that no particular issues have been found. On the topic of the imaginary γ_t lattice, **C. Bhat** asks if coupled mode oscillations have been observed from the beginning or only later in cycle.
S. Igarashi replies that no strong coupling has been observed.

4.2 Performance and limits at CSNS

Speaker: Sheng Wang, IHEP
Scheduled: June 29th, 1.30 pm - 2.00 pm

S. Wang's presentation focuses on the performance and limitations at the China Spallation Neutron Source (CSNS). After presenting a short overview over the CSNS design and history, the recently achieved beam power of 100kW is highlighted. Result from the beam commissioning are presented, such as the tune optimization and reduction of the tune variation during the ramp as well as studies on improving the painting scheme. Next steps in order to push for higher beam power are presented with upgrades supposed to start soon.

M. Bai inquires about the tunes plots on slides 18. In particular, if the measured tune or the set tunes is displayed and how big the difference between those is.
S. Wang answers that both design and measurement are displayed and are very close to each other.
G. Franchetti inquires about the tune optimization done to reduce the beam loss due to space charge and the incoherent tune shift at injection energy.
S. Wang replies that the nominal space charge tune shift is 0.28.
Furthermore, **G. Franchetti** asks why the tunes do not change after 6 ms mark.
S. Wang explains that at this point the beam energy has increased significantly.

4.3 Mitigation strategies for the transition from FAIR phase0 to FAIR

Speaker: Mei Bai, GSI
Scheduled: June 29th, 2.00 pm - 2.30 pm

M. Bai starts the presentation by introducing the current accelerators at GSI, highlighted in particular the flexibility in terms off beam species. The FAIR project and projected timeline are shown and studies in the CRYRING and SIS-18 in preparation for the future accelerator are presented. Further challenges and measures taken to mitigate those are shown.

G. Franchetti asks which problem is the most critical in the speakers opinion and where most of the attention should be put on.
M. Bai replies that given also the limited resources, the interconnect between groups is one of the most critical points moving forward.

4.4 Discussion Session

M. Vretenar opens the discussion session and remarks that from the presentation he concludes that life starts after the commissioning and that the road to nominal performance can be a long and difficult one and only after mastering the facility, upgrades can be contemplated.
G. Franchetti remarks that he would be interested in how other laboratories have handled the communication between groups to avoid problems.
The speakers from J-PARC and CSNS are not available for answers. **M. Bai** comments, that the situation at GSI/FAIR is rather unique and that the experience from others might not apply easily to GSI.
F. Zimmermann remarks that J-PARC and SNS were built as a joint ventures and both succeeded. As such he wonders why the situation for GSI is more difficult.
M. Bai replies that they are currently not experiencing any issues but when going forward, it is critical to avoid those.
F. Zimmermann would like to know if any delays are more likely to come from management issues or from beam dynamics problems.
M. Bai specifies that interfaces should be well defined to allow for efficient use of the resources.
F. Zimmermann wonders if organizational problems should be added to list of performance limitations in hadron rings.

M. Vretenar is worried about that tunes at high intensity differ from the nominal and need to be found experimentally. He wonders if this is normal, how long this set up can take and how much of a risk it is.
M. Bai states that she is very impressed by the accuracy of the tune control in CSNS and that it speaks for the predictive power of their model. She wonders if this experience is shared by the colleagues from J-PARC.
T. Toyama notes that in J-PARC's RCS also tune varies strongly during the acceleration and that for chromaticity control AC-sextupoles are used.
M. Vretenar states that this speaks for the flexibility of the machine.
T. Toyama notes that this however was not available from day one but took some time to get it operational.

M. Vretenar asks G. Franchetti and F. Zimmermann where the priorities are now, given the experience reported by J-PARC and CSNS.
G. Franchetti replies that they had a workshop 2 years ago to collect issues and that the idea for the discussion in this session is to compare the experience between different laboratories.
F. Zimmermann noticed that higher harmonic RF systems appears to be a common tool and sextupole resonance compensation also appears to be in high demand. He also noted that CSNS appears to cross the coupling resonance during the store and how well coupling is corrected there. Furthermore, we wonders why they observe a horizontal instability rather than a vertical one, given the flat chamber and a higher resistive wall impedance in the vertical plane.
G. Franchetti would also like to know the agreement between magnet design and measurements, how well this translates into simulations, and strategies implemented to refine those.
Given the unavailability of several colleagues, it is proposed to continue this discussion in another session.

R. Ainsworth comments on the sextupole resonance compensation in J-PARC, likely requiring also good modeling of their machine. He wonders how this could be done with imperfect models and assuming no magnet measurements.
F. Zimmermann notes that two orthogonal knobs could be used and correction is done empirically until losses reach a minimum.
R. Ainsworth comments that this would still require crossing multiple lines in the same scan and finding a trade-off might be difficult.
G. Franchetti notes that he saw in experiments conducted at GSI, finding the right pair of sextupoles may also prove rather difficult. He would be interesting in further details on the compensation done at J-PARC.
M. Bai would also like to know how well the model need to be and how this relates to the general goal of the machine.
I. Hoffmann would like to remind everyone that both J-PARC RCS and CSNS are rather fast machines compared to machines like the SIS-18. As such, due to their short cycle time they may not need to know their tunes to the high precision required in other machines. Furthermore, they may not even be able to achieve the accuracy of other machines. He comments that the statement on the small difference between set tune and measured tune is to be seen in relation to the cycle time. Vice versa, slower machine may be more thus be more sensitive to tune drifts. Additionally, he notes that SC should not create any additional difficulties in regards to the tune setting.
M. Bai responds that she is not only interested in the tunes but also the optics model in general. From **I. Hoffmann**'s comment, she concludes that in RCSs, the requirements on the model accuracy are relaxed.
I. Hoffmann agrees and adds that other processes may be more problematic in those machines.
G. Franchetti would like know the experience at CERN, given the large effort put in the optics modeling there.
No colleagues from CERN are available to comment on the situation in CERN's injectors.
M. Bai adds that if for RCS the model is not so important, what is the benefit of machine learning for overcoming performance limitations.
G. Franchetti notes that for ML, specific applications still need to be found.
E. Fol comments that ML is particularly useful when targeting multiple parameters. She is not aware of any big synchrotrons where ML is used in operations but notes that at CERN, in the AWAKE beamline and in SPS reinforcement learning is used to tune the machines.
R. Ainsworth states that at FNAL, model are used for establishing corrections but are in general not good enough to find nonlinear correction in the real machine.
G. Franchetti notes that accurate models may not be needed in terms of operation if the machine is working well, however, for optimization and upgrades beyond the current state, the model should be as exact as possible. He notes further that scans for example are a rather practical method, but do not allow for any predictions.

— 30 min Virtual Coffee Break —

4.5 Beam loss mitigations for High intensity operation at ISIS

Speaker: Robert Williamson, UKRI-STFC
Scheduled: June 29th, 4.00 pm - 4.30 pm

R. Williamson presents the various measures taken at the ISIS synchrotron in order to reduce the beam loss which have so far been the limiting factor. Studies on a head tail instability driven by impedance together with further developments on the hardware side have allowed for a record beam current, such that the maximum current is now limited by the targets.

A. Burov comments that there are two methods for mitigating instabilities, either by manipulation of the chromaticity or by the use of a transverse damper. He asks which one was used in ISIS.
R. Williamson replies that all sextupole except one have been remove from the ring and chromaticity is not corrected. As such, only one BPM and a vertical tune kicker are used as damper.
On the same subject, **V. Lebedev** asks what the bandwidth of this feedback is.
R. Williamson replies that it is around 20 MHz, but improvements to this system are currently going on.
C. Bhat asks if ISIS is cycling with a frequency of 50Hz and how it is created.
R. Williamson explains that large capacitor banks are used, basically creating a large LC circuit to generate the 50 Hz cycle rate.
A second question from **C. Bhat** is on the jitter from injection and what type of jitter is observed.
R. Williamson confirms that jitter is seen, but damps down in the first ms. The effect on β-beating created by the injection dipoles remains to be understood as well as dependence on intensity.
R. Ainsworth raises a question about the dose shown on slide 16 and if it really is measured in one location.
R. Williamson confirms that it is measured at one location and is something that is taken into account when conducting maintenance.
On the same slide, **V. Lebedev** asks on the layout of the displayed dipole chamber.
R. Williamson explains the layout, showing a ceramic beam pipe, 2mm thin wires used as RF screens. For accurate prediction of instabilities, the model of the dipoles is currently being refined, since they make up a large part of the ISIS accelerator.

4.6 Mitigation strategies for RHIC and EIC hadron beams

Speaker: Wolfram Fischer, BNL
Scheduled: June 29th, 4.30 pm - 5.00 pm

W. Fischer presents examples used in the 20 years of operation of RHIC which go beyond standard mitigation solutions and the role of beam cooling in EIC project. Such examples are the use of an e-lens for head-on beam-beam compensation and the 3D cooling used for heavy ions, which have allowed for special runs such as the Zr/Ru run. For the EIC, various cooling concepts are presented, which were also discussed in a recent cooling workshop in FNAL.

V. Lebedev asks how chromaticity was measured during the operation with chromatic feedback.
W. Fischer replies that radial modulation was used for this case.
V. Lebedev further inquires if a change in beam stability was observed when operating with an e-lens.
W. Fischer answers that from simulations, a decrease of the stability is expected, however, this was not observed in operation.
M. Bai asks which of the two beam cooling options discussed on slide 22 is planned to be used in the first stage of the EIC.
W. Fischer replies that discussions are still ongoing.
V. Shiltsev's question is on the impact of cooling on the integrated luminosity.
W. Fischer replies that the difference is about a factor 3.
M. Bai asks if there is any change in polarization due to beam cooling.
W. Fischer replies that no effect is expected.
F. Zimmermann adds that a change may be expected from beam-beam effects.
W. Fischer replies that no direct effect on polarization is seen, but rather via the tune shift.
V. Shiltsev comments that to first order, no effect on polarization from electron cooling should be expected.

M. Bai further adds that in HERA, an increase in polarization was observed when colliding, but agrees the direct effect from beam-beam should be weak compared to impact of magnetic fields.

F. Zimmermann asks why two different types of links are used for the transverse and for the longitudinal cooling.
W. Fischer replies that the microwave links may sometimes fail during bad weather periods but in general are the preferred option. He suspects that the reason behind the use of different types is probably cost.

4.7 Mitigations for Fermilab complex upgrades

Speaker: Eduard Pozdeyev, FNAL
Scheduled: June 29th, 5.00 pm - 5.30 pm

E. Pozdeyev starts the presentation by giving an overview over the Fermilab accelerator complex and the planned upgrades as part of PIP-II. The current challenges are highlighted with planned near term and long term solutions. Experimental studies in the booster on emittance and beam loss vs. intensity are presented as well as beam dynamics simulations on RF gymnastics in the recycler and a γ_t jump in the main injector.

S. Machida asks on what type of beam chopper is planned to be used, a conventional system or a laser based.
E. Pozdeyev replies that for the time being a conventional kicker system is planned, but space for a laser based system is left for possible later use.
S. Machida further inquires on the limitations of that system.
E. Pozdeyev answers that long pulses will be become problematic due to power load.

G. Franchetti remarks that the $\frac{1}{2}$-integer correction was difficult to achieve in the FNAL booster experiments and would like to know what is done on this front.
C. Bhat replies that there is still a lot of effort being put into the correction and that 1 shift per month is provisioned for such studies.

4.8 Discussion Session

M. Giovannozzi starts the discussion by asking if he understood correctly that ISIS beam performance is now limited by the target.
R. Williamson replies that this is partly true. The targets would allow for slightly higher beam intensity, however, they are still interested in reducing beam losses and understanding the involved mechanism, also in view of upgrades and future machines.
M. Giovannozzi asks for a clarification about the maximum intensity allowed by the targets.
R. Williamson specifies that two target stations are in use and an upgrade of one of them is currently going on. The other station would allow already from the design a higher intensity. However, no major increase is expected.

M. Giovannozzi asks which limitations are still present in RHIC and if more studies on those will come in the future.
W. Fischer states that the current focus lies on the low energy runs and after installation of a new detector, operation will shift back to high energy. A slight increase in intensity is planned but no large upgrade for RHIC will come. Focus is put on the design of the EIC and the technical infrastructure.
M. Giovannozzi inquires if there are any particular beam dynamics studies planned in RHIC in support of the EIC program.
W. Fischer confirms that studies are ongoing, for example that the hadron beam in EIC will need to run with a large radial offset of 1cm. He also mentions that certain effects like ecloud will only be possible to study once the infrastructure is in place.

V. Lebedev asks if octupoles are used for beam stabilization in RHIC.
W. Fischer confirms that octupoles are used, but only at transition, where they run at almost full strength.
G. Franchetti comments that with the large radial offset, large feeddown effects are to be expected.
W. Fischer confirms and notes that cooling would help.

M. Giovannozzi asks about particular considerations to overcome the limitation in the FNAL accelerator complex and ongoing beam studies in support of that.
V. Shiltsev answer that the campaign last year was very successful and some fundamental studies are still ongoing to ease operations. He hopes more campaigns can take place in the future, provided support by management.
C. Bhat adds that in booster studies are still ongoing in view of PIP-II to overcome current beam intensity limitations. In particular, modeling of the booster proves difficult and beam based studies help refine these models. He notes that with support from management, on average one day per month is dedicated to studies in the booster. Similar efforts are ongoing in the main injector.
G. Franchetti asks how the IOTA program may help other accelerators at FNAL.
V. Shiltsev comments that there is a rich experimental program at IOTA, with particular focus on reducing losses and SC compensation via elens/ecolumn. If successful, these can be implemented in the successor project to PIP-II, as PIP-II is already approved and being implemented. He also wants to emphasize how difficult modeling of the booster is and how difficult prediction of SC is with current tools.
G. Franchetti notes that at CERN, a lot of time is spent in machine studies, refining models and adding beam diagnostic devices. He adds that accurate modelling of SC requires input of various parameters where more diagnostics helps.
C. Bhat comments that the age of the accelerators at FNAL is an additional problem and in the example of the booster, not even magnetic measurements are available. Diagnostics is also limited by the available space. For the booster, the lack of a beampipe causes further complications.
G. Franchetti notes that especially predicting beam loss are particularly difficult.
E. Pozdeyev comments that he was particularly impressed by result presented by Hochi-san from JPARC in a workshop last November, showing good qualitative agreement.
V. Shiltsev comments that they are mostly after quantitative agreement.
E. Pozdeyev adds that in the presentation, in some cases there is even quantitative agreement between simulations and measurements. He adds that at JPARC, the machines seem to be very well understood.
M. Bai asks about the beam loss in high intensity machine and if more focus should be put on understanding where losses come from or on how they can be best controlled.
G. Franchetti replies to M. Bai's question, stating that understanding the mechanism is more important as good control can be very dependant on outside influences.
R. Ainsworth notes on the success at J-PARC, that the large beampipe could be one of the big contributions to the success of the models.

F. Zimmermann wants to raise a previous questions about the optics control at CERN. The question was up to which point should the optics be controlled.
M. Giovannozzi replies that for example at the LHC, optics is control is on the one percent level, however there are different phenomena to take into account. In the PS on the other hand, β-beating is in the order of a few percent, owing also to the well known field quality of the normal conducting magnets. Problems in the PS such as emittance growth could stem from a mismatch of the dispersion at injection. He states that the optics in the PS is well controlled in view of the current needs.
M. Bai wants to add to R. Ainsworth's previous questions, if she understands correctly that comprehensive modeling of a real, high power machine is particularly difficult.
R. Ainsworth agrees and adds that the implementation of very small details such as welds is very tedious.
V. Lebedev comments that for example in Tevatron, β-beating was around 20 to 25 % but there was no reason to fix it as there was no impact on operation and additional work on correcting it would have

not improved the situation. The only critical point is having equal β-functions in the interaction points. In supersymmetric machines on the other hand, optics control is very important as it allows for resonance suppression.
M. Giovannozzi adds that for the LHC, focus on the optics was put as it helps with aperture and optimal conditions for collimation, allowing to reduce the β^* below it design values.
V. Lebedev notes that in LHC, there is no point in investing more time to improve the optics control any further as it would not help and current levels seem sufficient.
M. Giovannozzi agrees, but emphasizes that due to the added focus on optics control have allowed for exceeding the design specifications.
M. Bai notes that having a good model may also result in cost saving at some point.
M. Giovannozzi agrees on M. Bai's statement.
G. Franchetti states that not only the modeling should improve, but rather focus could also be put to make the machine hardware more ideal as modeling every small detail may prove impossible.
M. Giovannozzi agrees on G. Franchetti's comment.
V. Lebedev comments on how good machines should be built. In his experience, it should be built in the simplest possible way such that it is not expensive, but commissioning steps and short-comings should be well known in advance. Design and hardware should follow the specific purpose of the machine but not exceeds the costs by spending to much on understanding the tiniest details. He additionally notes that investment in beam diagnostics should not decrease but rather increase.
R. Ainsworth adds that all machines are built with a certain purpose, however, during the lifetime of a machine, it's purpose may can change. He asks how one can plan for that or if instead it is cheaper to just built new machines and stop pushing machines way beyond their design.

5 June 30th

5.1 Capricious beam jitters

Speaker: Sophia Kostoglou, CERN
Scheduled: June 30th, 2.30 pm - 3 pm

Question from **G. Franchetti**, if there are plans to compare the presented simulated beam profiles with the results obtained by collimation team. Speaker answers that such comparison has been performed already using beam lifetime estimation obtained using simulated profiles and the measured ones. Beam lifetime is used for the comparison, because its estimation greatly changes if there are differences between the profiles.

Comment by **V. Lebedev**, noise from the damper and inter-beam scattering are two major effects which contribute to the emittance growth. According to the speaker, they are not included into the simulations – which is not a problem for the presented study, since the target is not to compute correct absolute values for the beam lifetime using the simulated profiles, but to show that with rippling there is a relative difference in beam lifetime between beam 1 and beam 2, as in the observations (about 20% resulting reduction).

Comments and questions by **F. Zimmermann** regarding noise origins and presented frequencies clusters. Speaker mentions that it is to be investigated, if the high frequency clusters are related to UPS, but the first suggestion is that there is no relation. Noise origin in beam 1 horizontal plane is still under study, some noise can be seen also in the vertical plane, but probably it can be related to coupling.

5.2 Mitigation of TMCI through space charge?

Speaker: Elias Metral, CERN
Scheduled: June 30th, 3 pm - 3:30 pm

Discussion on the differences in GALACTIC and BimBim modes regarding the effect of positive modes going negative in BimBim simulations. **A. Valishev, E. Metral, X. Buffat, A. Oeftiger** are discussing on the type of modes which could explain the effect, but also the effect might be due to the artefacts in the codes, not actual physical effect. Elias highlights the results shown in the appendix of his slides. **A. Oeftiger** notes, that such effect has not been seen in PyHeadTail simulations, only by restricting to radial modes. **E. Metral** mentions that for radial modes, air bag model behaves differently. Conclusion is that in such studies we need consistent models.

Question by **C. Bhat** about the measured intensity limit from TMCI point of view in SPS and LHC in the current mode of operation. **E. Metral** replies that first measurements in 2003 demonstrated the intensity of $1.7 * 10^{11}$ per bunch, later by applying some simplified model neglecting space charge and changing Q=26 to Q=20, the intensity has been predicted to increase up to $4 * 10^{11}$. This has been confirmed with the measurements.

5.3 Advances in collimation at GSI

Speaker: Lars Bozyk, GSI
Scheduled: June 30th, 4:30 pm - 5 pm

Question from session chair about the updates on SIS-100, when is the machine expected to be operating? Speaker answers that the tunnel is ready, most of components are purchased, the plan is to start around 2022, within the next years.

Electron capture and electron stripping are two approaches mentioned in the talk, competing for different ions– at which charge do these approaches become equally strong? Speaker answers that it has not been investigated, but it would depend on the energy.

Question by the chair, if it is planned to operate at spectrum of charge states or at particular state? Reference ion for FAIR operation will be Uranium 28+, but potentially with the possibility to accelerator arbitrary ions.

G. Franchetti raises a question about the collimation system - what is the capability and what is the accepted beam loss threshold? - Acceptable beam loss during operation should be less than 5%.

5.4 Active space-charge compensation

Speaker: Eric Stern, Fermilab
Scheduled: June 30th, 5 pm - 5:30 pm

I. Hofmann comments on how to benchmark the effectiveness of the presented approach using the lens compensation, there should be the reference to the tolerable intensity without using any compensation. Because the relative improvement depends on the starting conditions (tune shift in this case). Erik answers that no detailed study has been performed on this, the results are preliminary.

V. Shiltsev concludes that it should be studied, how losses depend on intensity, without having enormous tune shift and any compensation. Another point of future studies should be, how would it look like for future machines in case of injection with initial ΔQ=1, if it can be handled.

G. Franchetti comments having a tune shift of 0.9 on the machine which has a tune of 3.5 – already simulating this is tricky in terms of matching. So, before concluding something, it should be clear how the matching is done. In the presented study, there is no matching in the classics sense, the conclusion is that the beam has relatively small particle loss, even with enormous tune shift.

The definition of presented rms numbers is rms of the action of all the particles.

V. Shiltsev asks, why not to move the tune down further away from the integer, because that would further decrease the losses. Speaker replies that this has been experimented and empirically it was found that moving the tune down does not help, but exact reasons for this are not known. **V. Shiltsev** concludes that the tune dependency would be worth to study.

5.5 Advances in collimation at CERN

Speaker: Stefano Redaelli, CERN
Scheduled: June 30th, 5:30 pm - 6 pm

G. Franchetti asks about the maximum beam loss thresholds. **S. Redaelli** replies – there are 2 scenarios for the LHC studies: steady state losses which are limited by the quench of the magnet, the present system can accept steady state losses of 500mW on the collimation system before being dumped. Second regime - fast failures (synchronous damps): collimators must sustain the single turn passage of up to 8 nominal bunches.

F. Zimmermann is referring to FCC, asking if with crystals and other features, could it be possible to shorten the collimators of FCC? There are investigation studies aiming to save space in the tunnel, but the issue is that in case of protons, the losses probably still must be distributed over many collimators in the tunnel, but crystals could be possibly used for ions.

F. Zimmermann is asking about the next plans for collimation. **S. Redaelli** replies that there have been some ideas in the past to have magnetized collimators for ions, but not clear if it can work. In the near future the plan is to demonstrate how to use non-linearities to measure the beam size even more precisely and independent of the optics corrections quality in order to set the collimators.

V. Shiltsev comments: what is about low energy fast changing machine? What would be the strategy to mitigate the effects from beam losses? How effective might be the crystals for low energy machines? **S. Redaelli** replies, his team did not make any experience in low energy scenarios. Another team is applying crystals to slow extraction at the SPS to shadow the septum. There is a rich R&D.

V. Shiltsev comments that maybe crystals can be used for effective extraction of halo particles.

V. Lebedev makes a comment on the concept of electrostatic collimator, a suggestion he had made in the past, but which was never implemented for now. Major problem here is that if particle sees the secondary collimator with particularly large impact parameter, it drifts out and will be lost somewhere behind the collimator. In principle, such collimator can be just 15cm. Important is the impact parameter for the primary collimator and how fast the particle's amplitude grows. For example, in case of gas scattering – such collimation scheme would work very efficiently, orders of magnitudes better than existing techniques. But not in case when non-linearities are present.

5.6 General Discussion

Chaired by Elias Metral

E. Metral starts a general discussion and reminds on the workshop on Space Charge in 2013 at CERN (see slides). Two extreme cases have been identified – small beam vs. large beam in the vacuum chamber. Important parameters to focus on concerning space charge have been identified: the space charge tune spread, the time spent with the tune spread, the acceptable percentage of transverse emittance blow-up and the acceptable fractional beam losses. A list of mitigation approaches has been established: reduce bunch brightness (or keep constant), merging of 2 bunches in the moment when the space charge tune spread is small in order to double the brightness. Another trick is to increase the bunch length or flatten the bunch profile to reduce the peak density. One can also increase the dispersion, in order to increase the beam size, while maintaining small emittance. Moreover, one could decrease the machine radius (example of PS booster), increase the beam energy (LINAC 4), the time spent with the largest tune spread can be also reduced, better injection process (painting, h-) and optimizing the lattice are further solutions. Elias also mentions resonance compensation (issue with space charge induced resonances), optimization of both, integer and non-integer parts of the tune, localization of the losses with collimators and losses reduction with cooling, compensation of longitudinal and transverse emittance (e.g. with electron lens), investigation on the interplay with other mechanisms, e.g. electron cloud, beam-beam, incoherent vs. coherent effects. Elias proposes to review this list and extend it with further approaches, stating if there are simulations confirming these approaches. Elias clarifies how the table should be filled: are there simulations stating clearly if the approach will help and if yes, by how much (quantifying the effectiveness of the proposed method).

Link to the shared document has been sent to all participants for further modifications and proposals.

V. Shiltsev asks to add the reduction of the chromaticity to the list as an important effect - there is a need to drop chromaticities without having instability problems.

V. Shiltsev brings up the importance of quantification of the mitigation techniques. For example, considering the emittance growth, by how many percent does the chosen approach mitigate the problem/ solves the problem fully, etc. **V. Shiltsev** presents slides discussing possible ways to measure the effectiveness of mitigation techniques. In case of strong non-linear dependence on losses, one should verify how much one could gain by changing various parameters. For example, in order to check what is the maximum of fractional losses one can make an estimate using a given magnet power, efficiency of collimation systems, number of injected protons and frequency of circulation. The proposed formula in the slides demonstrates that efficiency of the collimation system helps relatively little, on the other hand brightness of the beam and gamma of injection are very helpful. Using non-linear optics, electron lenses can be also efficient and can be checked with the formulas shown in presented slides.

E. Metral comments that information given by **V. Shiltsev** should be added to the shared table.

I. Hofmann makes a general comment that one should consider the type of the machine since it changes the effective impact depending on the machine type.

V. Lebedev states a comment about the dependence of losses on the number of particles and extrapolation to space charge tune shift. The dependence on space charge tune shift is just a consequence of the number of particles, in case the emittance is constant.

G. Franchetti comments on the quantitative comparison. The approaches are acting differently according to what they are supposed to mitigate, e.g. dispersion can be seen differently according to the discussed effect. It is complicated to measure the overall effectiveness given the fact that several effects are coupled in regard of losses.

There should be more clarification on what is expected by taking into action a particular mitigation approach, such that an additional column in the proposed summary table might be needed (e.g. circular modes as mitigation approach – what is the addressed issue to be mitigated).

V. Lebedev comments that 4D brightness can be gained by using the circular modes. In ideal case we may reduce the smallest emittance and keep the largest emittance constant, such that space charge tune shift will remain the same. Emittance exchange between different modes should be considered much more significant. The comment is followed by a discussion of impact of circular modes and emittance impact on space charge tune shift.

V. Shiltsev concludes on the session, with a comment by **E. Metral** that participants should aim to summarize all the mitigations in different machines to clarify which approaches are suitable for which machines.

6 July 1st

6.1 Special schemes, slow extraction et al

Speaker: Peter Forck, GSI
Scheduled: July 1st, 2.00 pm - 2.30 pm

P. Forck presents measures taken at the SIS-18 at GSI to mitigate a spill microstructure, meaning beam current fluctuation on the 100 μm scale, in the slow extracted beam. After a brief overview of the SIS-18 design parameters, the requirements of the fixed target nuclear physics experiments, and the arising challenges, various studies are presented. Studies focus for example on the impact of power supply ripple to refine simulations and optimization of the sextupole strength to find an optimal trade off between the effective transmission and ripple. Next steps include studies on the main parameters for the knock-out extraction, which are also of interest for medical facilities, and hardware improvements.

V. Lebedev would like to know the reason why bunching helps.
P. Forck replies that the transit time is modulated by synchrotron oscillations and by that the synchrotron period might fall in the order of the transit time for the given parameters.

S. Nagaitsev asks about the effects of the space charge tune shift on the slow extraction.
P. Forck replies that the microstructure was also observed for higher current beams. During the slow extraction, the space charge effect also changes due to reduction of beam intensity. He adds that space charge will modify emittances, tune-spread and tune-shift, and that this effect has to be taken into account during the tune correction.

6.2 Manipulation measures including cooling for storing and decelerating highly charged isotopes

Speaker: Markus Steck, GSI
Scheduled: July 1st, 2.30 pm - 3.00 pm

M. Steck starts the presentation by illustrating why decelerated beams are interesting for nuclear astrophysics and which accelerators in the GSI accelerator complex are used for these studies. The design of the heavy ion storage ring ESR is presented and the two used cooling techniques are described in detail. The deceleration cycle, extraction and observed limitation are highlighted.

A. Burov asks about why the space charge tune shift of 0.1 was used, which in his opinion seems rather small and could be increased.
M. Steck replies that this number was just taken as an example. Limitations from SC are expected but not yet observed.

S. Nagaitsev would like to follow up on A. Burov's question. Given that Schottky diagnostics is available in both transverse and the longitudinal plane, was the incoherent space charge tune shift observed.
M. Steck replies that it was not observed as for high charge beams, the lifetime was too short for the diagnostics. New diagnostics has only just been recently installed and no studies with it were conducted so far.
S. Nagaitsev notes that in the shown plots the tune shift as function of intensity is displayed and would like to know how those were measured.
M. Steck notes as mentioned before that it was just available for a couple of days but will be permanently installed going forward and more studies are to come.

S. Nagaitsev notes that with this diagnostics, the space charge tune shift could then be observed directly.
M. Steck agrees and mentions that they could be done only once the system is fully available.
S. Nagaitsev notes that the lifetime depends on $Energy^2$, but from the theory point of view, this only holds true if the beam size is 0. As the beam decelerates, the beam size increases and thus this dependence is not correct anymore.
M. Steck replies that the presented measurements were not conducted on decelerate beam, but rather with stored beam and active cooling.
S. Nagatisev asks for confirmation that then for decelerated beam, the situation is worse.
M. Steck confirms and adds that this might be reason for the observed beam losses at higher intensity. Furthermore, issues with the vacuum may add to this.

V. Lebedev asks how electron capture from cooling relates to loss during deceleration.
M. Steck replies that the effects are easily seen. At high energy, the limitation stems from the electron capture of the cooling system as vacuum lifetime is in the order of hours, whereas the lifetime with respect to the cooler is minutes. During the deceleration, the lifetime from the cooler stays constant, but the lifetime due to residual gas drops to seconds.

F. Zimmermann asks about the use of two different cooling systems for the injected orbit and the stacked orbit. How is the orbit moved between those.
M. Steck replies that this is done via a change of the RF frequency.

G. Franchetti comments on the previous question of S. Nagaitsev, saying that measurements are planned in the ESR to see the behaviour of SC tune shift near the $\frac{1}{2}$-resonance.

6.3 Beam cooling in the storage ring CSR at IMP Lanzhou

Speaker: Lijun Mao, IMP/CAS
Scheduled: July 1st, 3.00 pm - 3.30 pm

L. Mao begins his presentation by introducing the accelerator facility in Lanzhou and the two housed synchrotrons, the accumulator ring CSRm and the storage ring CSRe. In the following, various experiments using the electron cooler in the CSRm and CSRe are presented.

V. Lebedev asks about the plots shown on slide 9, what the reason behind the observed asymmetry in the longitudinal Schottky spectra is. Furthermore is the momentum offset is considered.
L. Mao answer that $\Delta p/p$ is not taken into account.

S. Nagaitsev notes that bunching should be observed even in the coasting beam. He would like to know the relative strength of the bunching due to friction compared to the barrier bucket.
L. Mao thinks that the barrier bucket should be the dominant effect but no comparisons were done.

V. Shiltsev notes that there would be an opportunity to observe crystalline beam and if it has been observed so far.
L. Mao replies that this has never been tried but agrees that it would be very interesting.

M. Steck has a question about slide 14 regarding the loss mechanism when the repetition frequency of the e-pulses is not matched to the revolution frequency.
The connection of the speaker was interrupted and the speaker could not reconnect to answer the question.

— 30 min Virtual Coffee Break —

6.4 Loss control mitigations for PIP-II

Speaker: Luisella Lari, FNAL
Scheduled: July 1st, 4.00 pm - 4.30 pm

After a brief introduction of the goals and requirements of the PIP-II project at FNAL, **L. Lari** presents the design of the PIP-II linac and conducted studies on the losses in the presence of realistic errors. In the following, ongoing studies on handling losses in the transfer line from the linac to the booster and the booster injection and extraction regions are described.

G. Franchetti notes that the allowed power loss per meter is a factor 10 below the usual 1 W/m and would like to know the reason.
L. Lari replies that this number was taken to be on the conservative side.

6.5 Special tricks: cooling and friends

Speaker: Valeri Lebedev, FNAL
Scheduled: July 1st, 4.30 pm - 5.00 pm

In **V. Lebedev**'s talk, different cooling techniques are discussed, broadly separated into two categories, electron cooling and stochastic cooling. After an overview over the used cooling techniques in past and current accelerators and proposals, additional considerations such as the use of a special optics with strong local coupling component are presented. Lastly, proposed studies on this topic in the IOTA-ring at FNAL are presented.

A. Burov notes that the electron cooling rate and the IBS growth rate have a different dependence on γ. He would like to know if there is a simple physical explanation for this, given that both relate to particle scattering.
V. Lebedev notes that the difference might stem from the used emittances in the formulas. Furthermore, he notes that for example the revolution frequency also has an implicit γ-dependence.

W. Fischer asks what in the speakers opinion is missing to conclude on the cooling solution which should be used in the EIC.
V. Lebedev replies that in his opinion no proposal is so far ready. For the ring based cooler, the effect of the emittance exchange remains to be understood. Furthermore, he only looked into a low energy case and will look into the EIC case after the workshop.
S. Nagaitsev adds that both coolers used at FNAL and at BNL were built without prior testing at this high energies. In these cases, the principle was demonstrated at low energy and scaled to the higher energy. However, cooling using an e-beam has not yet been demonstrated, even at low energy, thus making scaling to the EIC case difficult.
W. Fischer notes that the FEL amplifiers are a demonstrated technology.
S. Nagaitsev replies that the FEL amplifiers are quite narrow band and as such might not meet the requirements for the EIC.
V. Lebedev adds that the noise in the e-beams is an unknown and the number of particles required appears not feasible technologically at the moment. He notes that in the cooling proposal from FNAL, the only unknown is the emittance exchange. If the cooling time is increased by a factor 5, the emittance exchange

will not be a problem in his opinion.

G. Franchetti would like some more explanations on the emittance exchange mechanism, mentioned in the previous questions.
V. Lebedev answers that here, emittance exchange via space charge driven resonances is the problem.

S. Nagaitsev would like to know more about the "enemies" of cooling, namely diffusion and instabilities. Are there any tricks to be played to alleviate the problem with diffusion but still keeping the cooling rate.
V. Lebedev replies that the IBS would be the real problem rather than diffusion from the e-beam. In his opinion, the real enemy is the absence of cooling at large amplitudes. He notes that the absence will significantly reduce the lifetime of the beam, as was shown in the recycler. Overcooling might be another problem. For the EIC case, he notes that electron cooling, coherent electron cooling and OSC have a limited cooling acceptance.

A. Burov comments on S. Nagaitsev's previous question that instabilities could be mitigated by the use of an e-lens. He hopes that this will be proved in IOTA experiments.
V. Lebedev notes that this could also be tried in RHIC.
W. Fischer notes that it is currently not possible, but only after the detectors upgrades and high energy operation resumes.

6.6 Discussion Session

Chaired by S. Nagaitsev

S. Nagaitsev asks P. Forck: What are the unanswered questions for accelerator physicists we should be thinking about preparing for the third order resonance slow extraction? What are the unanswered physics questions from your point of view?
An additional Comment from S. Nagaitsev: For example I have noticed that the space charge effects are still because some of the experiments requires bunched beams which is to be delivered to particle subtargets and of course there are space charge tune shifts which affect them.

P. Forck comments: Slow extraction is influenced by many device parameters (e.g. tune via quadrupole settings, sextupole settings, voltage of the electro-static septum, ripple of power suppliers etc.) but also on by beam quantities (like transverse emittance, [more precisely phase space distributions], momentum spread and bunch length [more precisely longitudinal phase space distribution], possible closed orbit distortions etc.). Moreover, slow extraction is a non-linear process. Therefore, it requires detailed particle tracking simulations to model the process and to achieve results which can be compared to the measurements (performed with non-ideal devices and beams).
There are some more challenges, but those goes more to the technical experts. One example is the modelling of power suppliers as the ripple in the order below 1e-5 of the full scale are relevant, but normally not investigated. A second example is the related to good particle detectors for the extracted beam with very high counting capability.

The wish list to the beam dynamics community is:
1. A good simulation code: This code should be able to take all settings and imperfections into account. It needs good output, e.g. the particle coordinates to visualize the behaviour. This code should also run fast to be able to make parameter scans. Presently, we use MAD-X and one parameter setting for 5e5 particles on the used computer need about 1 day. Good programming is needed here supported by good numerical and analytic solutions.

2. A collection of analytic formulas: A very comprehensive description of the slow extraction with many formulas exists, the so called PIMMS-report from 1999. However, quite some assumptions are done there to derive the formulas, which are not always clearly stated as the report aims for medical facilities. Those facilities might have quite different beam parameters compared to our beams and not all types of extraction methods are described. Maybe by the deriving formulas for the more general case, new ideas for the numerical implementation appear to speed up the simulation codes. (MAD-X is a general code and not optimized for slow extraction.)
3. A comparison of the different extraction methods: This would help to weight their properties. (There are many methods of extraction e.g. quad-variation, knock-out, longitudinal stochastic excitation, bucket channeling etc.) This should be accompanied by possible analytic descriptions and examples of simulation results. The goal is to find the best method for the given beam parameters and user requirements.
4. Physical interpretation: A descriptive interpretation should be available to enable a clear physical interpretation of the results. An example: For the micro-structure I presented the transit time as such a quantity, which is influenced by the parameter within the synchrotron and determines the properties of the extracted beam. This physical, but not measurable quantity greatly improved our understanding and enabled a valid forecasting of experimental results.
5. High current properties: The influence of space charge tune shift and tune spread should be included. Here it is a trivial, but important fact that the beam density decreases during extraction in a very particular manner (depending on the extraction method). The effect of the change of tune spread and shift should be estimated and possible counteracts can be discussed.

S. Nagaitsev asks P. Forck: How to remove the ripples?
P. Forck answered: You will have to use simulation tools to remove them.

V. Lebedev asks P. Forck: Do you use feedback?
P. Forck answered: Not in that timescale. It should be tried to get it, but in principle the transit time is getting unstable until reaching septum so the particle is very difficult to influence. It is a latency in the system.
V. Lebedev comments: There are some possibilities. One is to use quadrupole feedback simulator which Fermilab uses. In addition to this you can have very high transverse feedback which will be bringing your beam close up and significantly compensate everything.
P. Forck replies: Yes but you will have to remember that particles are getting unstable having an exponential amplitude growth and you can't detect them later on when they have left the synchrotron.
V. Lebedev comments: I understand feedback can work faster than 1e20 revolutions.
P. Forck replies: We are not much slower, lets say you have to put it on the 10mus scale so we are at 1000 turns. What we are doing is a slow feedback what I call microstructure. That is possible. It is even being used by some medical facilities in Heidelberg on regular bases. For the 100ms feedback has been very well chosen.

S. Nagaitsev asks M. Steck: What are the unanswered questions?
M. Steck answers: One issue is the space charge at very low energy which is certainly different to what you have to deal at high energy accelerator and high intensity machines. What we have is not at high intensity, but it is high comparing to the velocity of the beam. This has something to do by scaling to experiences of other accelerators and of course highly charged ions are special because of the strong IBS which you do not have anywhere else.

S. Nagaitsev asks M. Steck: Is it possible to manipulate the phase space of the ion beam as you decellerate or at low energy so that you move the IBS from one degree of freedom to another degree of freedom by manipulating temperature or momentum or energy spread in the beam?
M. Steck answers: For low energy we do apply electron cooling and there it is not easy to manipulate never

the less we have done that in the past that you can do this alignment. This way you can to some extent the transfer of temperature from one to another degree. You will "simply" have to optimise everything and then you have a longitudinal degree. In that sense you have to optimise cooing in order to get the best transverse emittance.

S. Nagaitsev comment and asks M. Steck: We know that IBS has a minimum when all temperatures of the beam in the rest frame are approximately equal. You can make the IBS work if your temperatures are not equal, but when all are equal you can reach some minimum of the IBS. So is this possible? We have done it in the past in the recycler by bunching the beam to make the temperatures close to be equal in transverse and longitudinal degree of freedom. Of course it is still not zero but it reaches a minimum. Can you do something like this with the ions?
M. Steck answers: Yes you can basically do the same things. You can bunch it but the question is whether or not you want to have a beam for an experiment with a stored beam an then it makes sense on the other hand if you just use the storage ring as a decellerator and transport the beam to another ring. Then of course this does not help a lot. I actually do not see that we are limited in intensity by IBS. I think IBS gives us a certain emittance from which we start but it does not limit the performance of the machine at this moment.

G. Franchetti asks M. Steck: How do you measure the temperature?
S. Nagaitsev answers: Yes it is measured by the emittance. The temperature of the beam varies along the circumference but it is the rms-emittance that gives you the temperature.
S. Nagaitsev comments: If you do the calculation to the IBS you can clearly see that there is a minimum depending on the parameters between the transverse and longitudinal rms-momentum and the rms-spread of the beam. You can not do anything about it but you can work within its range.
V. Lebedev comments: In the smooth lattice approximation there is no emittance for that the temperatures are equalized. You can make it quite smooth by using special optics and it can reduce IBS very significantly.
V. Lebedev comments further: At the very beginning of NICA it was one of the ideas to do that but the problem is that NICA is a collider and as soon as you use collider optics that is it, it will break everything down. Still we are in the regime when the temperatures are approximately equal but it would be much better if you would not have a collider.
M. Steck answers: Usually when you have a cooled beam you have a emittance probe in all directions which means that you do not only heat in one direction but in all. This is because of the high energy deposited in the beam and you have very tiny transfer from the beam momentum to the beam frame.
S. Nagaitsev comments/clarifies: The coupling of the kinetic energy of the beam comes only from the variation of the beta-function.
V. Lebedev comment: You can easily reduce IBS by many orders of magnitude by making the beta-function smoother which makes the variation smoother.
M. Steck comment: Yes of course but this is not realistic for low energy machines.

7 Scientific Secretaries

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