Reportrd by Raymond Wasef

Hollow Bunch for potential SC Mitigation (A. O.):

O. B.: No one is using hollow bunched due to dipolar instability and beam loading.
A. O.: Actually S. Hancock saw this instability and cured it by leave few particle in the center, so instead of a really hollow bunch, it still have a very low density in the center of the phase space.

H. S.: Why are you using a Gaussian distribution at the beginning since we observed it is a parabola?
A. O.: This is just for a simplified comparison.

I. H.: Reducing the peak density will reduce the space charge effect. Did you think about the interesting observable you will use in the case? For a peaked distribution, it's the RMS emittance the most interesting, but for the hollow one, it should be different.
V. K: You could user polar coordinated to characterize a hollow distribution. Then, the projection on the radius may be comparable to a peaked distribution.

2nd Discussion session:

- *Next Space charge workshop:*
  There will be a 2nd Space Charge meeting in 2015 in the Oxford, the dates are not decided yet but most probably end-march.

- *Code release (benchmarked and with all necessary features):*
  J.H.: py_Orbit is almost ready, few modules to be implemented this summer.
  J.A.: When would it be important to have a 2.5D solver (Longitudinal slicing + 2D at each slice) instead of only a 2D solver (2D+longitudinal weighting)?
  J.H.: What we saw in our simulations, the only case where there could be a difference is when one includes transverse impedance or indirect space charge (boundaries).
  S.G.: J.Q. would still need input to complete the benchmarking of IMPACT.
  J.A: I could help, because I have the parameters and I understand the input of IMPACT.

- F.S.: We have to check the validity and reasons of choice between PIC and frozen space charge codes.

- Understanding the noise in PIC codes, for long-term tracking? Is it acceptable?

F.S: It is unavoidable, but we seem to understand it.
There is also an alternative used by the plasma community (delta-f instead of PIC)

I.H.: They use delta-f codes because their systems are huge, but I personally doubt it is needed for beams.
We should find a case to benchmark codes to keep the noise level low enough to be able to see phenomena like trapping. Code authors, should introduce in their codes some noise and evaluate at which level the noise could be tolerable.

F.S.: There is some hope after what J.A. showed. He showed that at some optimal parameters increasing the number of macro-particles doesn't change the emittance behavior, even if the particles are chaotic, they mix in a strange way so that the distribution is not affected. Then maybe, we could go to long-term simulations.

G.F.: To follow what I.H. was saying, I introduced noise the code in a controlled and known way to parameterize the noise for the case of a resonance crossing. This was written in the proceedings of IPAC13.

O.B.: It is interesting, but this is a white noise, while what we have in PIC codes is a self-consistent noise.

G.F.: It was just a first attempt to what I.H. was proposing. I think that one should first study the effect of the noise on the non-linear dynamics then consider the space charge.

J.Q.: Maybe the collisions which are provoking the noise are not completely useless because it could be comparable to intra-beam scattering.

I.H.: I think a group of people who are interested in that should meet offline, and define a strategy to give a good physics quality of trapping.

G.F.: Let's try to prepare a case that could be measured and benchmarked with the codes

- What kind of hardware do we need?

F.S.: It is important for us to know the hardware we need and we could have. J.A. was talking for example about GPU.

J.A.: We were not particularly motivated with respect to GPU, but if the CERN users think you are interested in, then we could invest more in it.

J.Q.: The problem of GPU is that the codes are not portable, so if the technology develops, the code needs to be converted another time. Additionally, it needs an important investment to convert a code.

O.B.: Yes, it is right, it is really time consuming. It's a huge effort and not portable. We started a project in GSI to convert to GPU but we abandoned it.

J.A.: Yes, it does even change very rapidly. That's why we try to study it and see how we could write our codes in a way, not to throw everything away if we need to change.

F.S.: Currently at CERN, we are not very comfortable with our computing resources situation.

O.B.: We should also include code performance parameters (how fast, how accurate…) in the benchmark.

I.H.: It would be great to have a benchmark, but a full benchmark is not meaningful, we need to specify a list of crucial parameters and look for a meaningful benchmark between codes and experiment.

F.S.: That's what we tried to propose in yesterday's discussion. We will try to have an experiment per machine (PSB, PS and SPS). We will try to prepare these experiments and make the data available for all codes.

G.F.: When we tried to make the benchmark of the SIS18 with S.M., it took us 2-3 months before we found an agreement. So before measuring in the machines, we have to find something meaningful, interesting, new and which we understand.

S.G.: We are not shooting the dark, since we are proposing something for which we already have measurements that we consider of good quality. We tested some simulation codes and they reproduced the effects up to a certain level and we have some theoretical understanding of the physics behind.

F.S.: Additionally, these experiments have both coherent and incoherent effects and we can test both frozen and self-consistent codes.

G.F.: An interesting subject for GSI is beam loss, I tried previously to make a benchmark but it didn't latch well because it is very sensitive. It would be nice to get closer than a factor of 2 in the prediction of losses.

F.S.: In the PSB, the losses are well expected.

J.A.: But in this case the losses were very large, almost all the beam, so it is much easier. I would doubt more moderate losses.

S.G.: We tried to start with an easy case, but we can propose other cases where losses are not that large.

S.M.: We need to extend the benchmarking by changing physical parameters to understand the basic physics (ex: Qs).

S.G.: We could make available the data we already have, then when we restart the machine we can agree on the parameters we want to vary.

- General impressions and discussions:

J.Q.: Shouldn't we distinguish between errors and noise? They are different and come from different sources. Errors coming, for example, from large gridding induce emittance growth, while noise it doesn't necessarily.

I.H.: It depends on the problem we study. To study resonances in circular machines, we need to define what would break the Hamiltonian mechanics, whether it is noise or errors, for us it makes no difference.

J.Q.: Using for example a 4th order or a 2nd order simplectic integrator will introduce an error, but we accept it as long as it is simplectic. So we should probably think if these particle collisions are really errors we can't tolerate.

G.F.: That's part of the test we should do.

V.F.: Can we see resonance islands on the wire-scanner?

S.G.: On the wire-scanners, you can recognize the boundaries. You can also reconstruct the phase space from the turn-by-turn data (for example MTE in the PS).

Names:

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