Comments on topics of study at the ATF extraction line meeting of April 30, 2008

1) Comparison of QM7 field maps computed at SLAC and LAL and conclusion on multipole fitting method (not presented at the meeting)

→ This needs to be completed soon based on the information from Maria Alabau, Guy Le Meur & François Touze, in the form of a note being drafted by Angeles Faus-Golfe.

2) Consistency of strength of skew quad added to QM7 to account for coupling measured through quad scans, with typical amplitudes of vertical offsets in QM7 and spot size increases seen on OTR (not presented at the meeting)

→ The inconsistency found between predictions by Maria Alabau, Cécile Rimbault and Philip Bambade based respectively on tracking and sigma-matrix propagation should be resolved before trying to make sense of comparisons with measurements.

3) Set of hypotheses for the incoming beam parameters and skew components added along the line (e.g. in QM7) to explain normal and skew parabolas measured in March, with and without the attempted correction. Are the parabolas used sufficiently constraining given measurement errors, or would additional ones be needed ? Impact of systematic errors from effective length and hysteresis effects (Cécile Rimbault).

8) Report on Thursday 24 April quad scan measurements shift with remote participation, including some preliminary analysis (Kuroda-san, Alexander, Mark, Cecile)

→ By combining skew quads at QM7 and at the bends BSnX it seems possible to explain the measurements (consisting of one normal and one skew parabola with QF5X and QK3X respectively) just as well as with a single skew quad at QM7. This leads to three questions:
1. The skew components arise from vertical beam offsets in the sextupole components present in these elements. To what offset would the strengths found possible in BSnX correspond, given the sextupole components in these elements ?
2. What would be the predicted vertical beam size at the OTR in the different cases studied ?
3. Could the measurement of an additional skew parabola, with another skew quad at a different enough phase, allow a better discrimination between the different possible sources of coupling studied ?
4. More generally: is it feasible given experimental errors to determine the full 4D sigma matrix from one normal and two skew parabolas, or is one better off testing different hypotheses for the presence of coupling, in order to compute a correction ? Could this be studied by using as input the 4D x-y coupled beam phase-space files recently made available by Kiyoshi Kubo from his DR simulation ?

→ In the measurements from April 24th, what is(are) the reason(s) for the difference between Cécile's and Mark's determinations of emittance and Twiss parameters ? Why is it concluded that the minimum of the QK3X parabola is below -5A ? From the data shown

it could well be around -3A or -4A... What skew quad strengths in QM7 (and/or in BSnX) could explain the QD8X and (partial) QK3X parabolas, and would the effect observed from the correction computed in March and implemented in April (QK1X=-2.2A & QK3X=-4.5A) then be reproduced ?

→ Concerning the dispersion corrections (performed in the first half of the 24 April shift ?), is it established that that horizontal dispersions must be considered quadratic rather than linear (e.g. from the better agreement with the modeling, or just better fit results ?). Does it matter for the convergence of the correction procedure ?

4) Error analysis and propagation in emittance and Twiss parameter determinations. Simulation of the measurements by multi-wire and quad scans. Is the precision on back-tracked Twiss parameters sufficient to be used as input and enable stable results when re-matching the optics for large phase-advances in the diagnostic straight (Anthony Scarfe, Julien Brossard).

5) Progress with Twiss parameter fitting methods to achieve reasonable phase advances in disgnostic straight section given backtracked values inferred from quad scan measurements (Robert Appleby, James Jones, Julien Brossard).

→ Results shown by Tony are quite different from what was shown by Julien at the previous meeting: a detailed comparison is needed between Julien and Tony - is the propagation of the errors done the same way ?

→ It is a detail, but the notation for the errors in slide 2 of Tony's presentation should better be in the form "fitted emittance +- determined standard deviation" to avoid confusion. In the case of very non-Gaussian distribution, probabilities might also be better to consider to define the (not necessarily symmetrical) errors. In slide 3, it is difficult to understand why the blue band (corresponding to the +-1 sigma) is not within the green one (corresponding to the extreme errors) throughout the plot.

→ Could you simulate the process of determining the Twiss parameters (and emittances), through multiwire measurements as in 4) and with a reasonable representation of the errors, for use as input to the Twiss parameter fitting described by Robert, in order to see whether the precision is really good enough to achieve the reasonable phase advances needed in the diasgnostic straight section ? Presumably, Twiss parameters determined from quad scans should be more precise in the present EXT line; could they be used as well as input to the fitting and would that improve the convergence ?

→ Could the optics fitting tools described by Robert be made available to the whole group, maybe with some README file ?

→ Twiss parameters determined by either multiwire or quad scans can be considered "effective" or "projected" ones in the presence of x-y coupling. This may affect the propagation and outcome of the fitting. Could this be studied by using as input the 4D x-y coupled beam phase-space files recently made available by Kiyoshi Kubo from his DR simulation ?

6) Extracting information on QM7 from fitting nearby BPM data taken during March shifts (Mark Woodley)

→ Consistency of 0.80 reduction in QM7 gradient as predicted from BPM measurements and field maps computed at SLAC and LAL ?

→ Would there be a way to evaluate uncertainties in the Twiss parameters determined from the DR model, e.g. given the newly improved orbit response matrix analysis ?

9) Discussion of goals and tentative shift plans for bumps + OTR + emittance/Twiss and skew measurement and correction (Maria Alabau, Cécile Rimbault, Anthony Scarfe)

→ An updated version should be prepared and discussed before next week, involving also the remote participants. Who can take the lead to write the list of things to be achieved, propose a prioritisation and circulate to interested parties for comments and suggestions ?

→ One thing which I think could be included is to repeat more carefully the BPM measurements around QM7, both in y and in x as function of a monotonically varied vertical bump. Trying to analyse the x BPM data from the last shifts, Mark made the following comment: "I don't see consistent behavior among the EXT BPMs. It looks to me like the order in which we took the data (start at zero bump, scan negative bump values, go back to zero bump in one step, scan positive bump values) made the data for two sides of the bump scan inconsistent with each other. I would recommend retaking the data, making sure to scan the bump monotonically (maybe start at zero, scan negative to negative max, scan positive through zero to positive max, scan negative back to zero)."

→ Another thing is to coordinate how we will e.g. use back-propagated Twiss parameters from quad scans as input to fitting the optics during the shift to improve phase advances in the diagnostic section. It seems that this should be important both to enable good multi-wire measurements and to calculate a correction for the residual x-y coupling without maxing out the skew quads (as they tend to fight each-other when phase advances is insufficient...).

7) Preparations for flight simulator testing in May. What is planned to be done with KEK control system experts in the first week ? EXT trajectory correction simulation and implementation through the flight simulator (Glen White, Yves Rénier).