Comment on "Empirical condition of betatron resonances with space charge"

Ingo Hofmann*

Technische Universität Darmstadt, Schlossgartenstrasse 8, 64289 Darmstadt, Germany; GSI Helmholtzzentrum für Schwerionenforschung GmbH, Planckstrasse 1, 64291 Darmstadt, Germany and Goethe Universität, Max-von-Laue-Strasse 1, 60438 Frankfurt am Main, Germany

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In the article of K. Kojima, H. Okamoto, and Y. Tokashiki [Phys. Rev. Accel. Beams **22**, 074201 (2019)] the authors claim that the optimum working point on the tune diagram of circular accelerators at high intensity should be determined by using a framework of *coherent* resonances instead of the commonly accepted and widely used "standard" diagrams based on *incoherent* resonance conditions. However, their proposal is based on a questionable interpretation of Vlasov's equation as well as an over-interpretation of their multi-particle simulations in the case of Gaussian-like distributions. Furthermore, the suggested coherent diagram is not supported by detailed published data from operating synchrotrons at GSI and CERN, which are in line with the incoherent resonance picture. As far as waterbag (or similar truncated) distributions, the authors model of coherent resonance diagrams is not questioned in this Comment.

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Resonance diagrams have been used since the early days of circular accelerators. It is common to incorporate space charge on a diagram with zero intensity resonance lines by means of a "necktie" including the shift (and spread) of *incoherent* tunes. In order to avoid misunderstandings: The necktie diagram is nothing more than a feature of the matched beam with its spectrum of incoherent frequencies, but without any resonance activated. Resonance studies are well known to require detailed considerations going beyond this simple tool. The question of coherence is only one facet in this problem.

In the past, quite a number of authors have considered resonant phenomena going beyond the incoherent picture and investigating a possible role of *coherent* effects due to space charge. The emphasis has been primarily on resonances or instabilities of second order, like a favorable coherent tune shift in gradient error resonances, or the parametric "envelope instability." Efforts have been made to demonstrate these second order coherent effects experimentally in circular accelerators, but so far with very limited success. Successful studies on space charge effects in second order resonances in the S-POD trap experiment could trigger a discussion on this point.

As far as higher than second order *coherent* (including parametric) resonance effects in circular accelerators the situation is different. To the knowledge of the author unambiguous experimental signatures have not been reported yet. Thus, no need has been seen so far to revise the standard picture of nonlinear, incoherent resonances.

Yet the authors claim—already in the abstract—that their coherent resonance condition based on the 1D Vlasov theory in their Ref. [27] (Okamoto and Yokoya),

$$m(\nu_0 - C_m \Delta\bar{\nu}) = \frac{n'}{2}, \tag{1}$$

with $\Delta\bar{\nu}$ the rms tune shift and C_m a factor depending on the order of the mode, would lead to a new, coherent diagram to replace the standard one. $C_m = 1$ is equivalent to the standard incoherent resonance condition, whereas $C_m < 1$ stands for a coherent shift. Magnet driven resonances are described by even values of n', while odd values stand for half-integer, parametric resonances (instabilities) not contained in the standard diagrams. In order to construct their new diagrams the authors first employ an "empirical" extension of Eq. (1) to 2D, which they call a "plausible conjecture" from their Ref. [31],

$$k(\nu_{0x} - C_m \Delta\bar{\nu}_x) + l(\nu_{0y} - C_m \Delta\bar{\nu}_y) = \frac{n'}{2}, \tag{2}$$

where they claim that the tune shift factor C_m only depends on the order of the resonance $m = |k| + |l|$. Odd values of n' again describe the extra parametric resonances; and

*i.hofmann@gsi.de

working points should avoid stopbands around these coherent resonance conditions for even and odd n'.

We proceed with first commenting the authors' interpretation of Vlasov's equation including Landau damping; then their coherent and parametric resonance discussion leading to the C_m factors; their conclusions from simulations and, last but not least, the comparison with experimental results from GSI and CERN accelerators.

Section I of the authors' article states: "The Vlasov equation treats the distribution function of the whole beam in phase space rather than the trajectories of individual particles. Since the beam is regarded as a sort of continuum, there is no room for the incoherent motion to come in". This inadequate interpretation of Vlasov's equation appears to be at the origin of the authors' preference of coherent resonance effects versus incoherent ones.

First, the Vlasov equation indeed describes a "sort of continuum", which is incompressible in phase space. However, it does so while the phase space fluid moves along the trajectories of individual particles exposed to external and self-consistent fields and—contrary to the authors' statement—incoherent motion is essential. In fact, it is common knowledge that fully solving Vlasov's equation—a partial differential equation—is done by integrating it along its characteristics, which are the particle trajectories. In a perturbation analysis these trajectories are taken from the unperturbed beam, which yields an incoherent spectrum of tunes according to the nonlinearity of the confining potential. The resonant interaction of this spectrum with a coherent mode is the origin of Landau damping—well described by the full Vlasov framework. For mathematical simplicity, however, it is standard in transverse beam dynamics to sacrifice higher than second order terms in the unperturbed potential, thus losing the tune spread and Landau damping. This loss of spectral width thus results mathematically in a purely coherent response to a resonance driving force as, for example, obtained in Ref. [27].

In spite of all this the authors conclude in Sec. II: "However, the core stability analysis based on the self-consistent set of equations predicts no resonance under the incoherent condition. Assuming that the Vlasov theory covers the whole relevant physical processes, the most reasonable conclusion should be that no serious resonance occurs within the matched beam core at the incoherent tunes (ν_x, ν_y) of individual particles." Here is the first objection: why should a simplified Vlasov model describe interaction with an incoherent spectrum if it was eliminated before for mathematical simplification?

Apparently this model simplification supports the authors' assumption that the coherent half-integer (parametric) space charge driven resonances (odd n') lead to "twice as many resonance stop bands as predicted by common resonance theories." While this "doubling" of stopbands is not questioned in second order—due to a coherent tune shifted outside of the incoherent tune spectrum—it is questionable for higher orders and Gaussian-like beams, where Landau damping is expected to occur (as suggested in Ref. [47]). Hence, further computer simulation is needed to strengthen this issue.

It may be appropriate here to elucidate somewhat the nature of collective, coherent and incoherent response. The full Vlasov equation takes into account all collective aspects of interaction. This includes the well known Debye shielding effect, which flattens the beam profile at high levels of space charge in a self-consistent manner. As a result, the spectral distribution of incoherent tunes is changed—a collective effect. A coherent response implies, in addition, phase correlations between particles leading to a coherent frequency—unless suppressed by Landau damping.

Before examining their simulation results we briefly comment on the authors' discussion of Vlasov models besides their Ref. [27], which is a 1D sheet beam model not directly leading to 2D coupled resonance conditions. In chapter II the authors mention "Since it is hopeless to solve the 2D Vlasov-Poisson equations mathematically for arbitrary AG lattices, a plausible conjecture was made recently by the Hiroshima group [31]".

In fact, it is not hopeless to solve Vlasov's equation in 2D: a remarkable step ahead in fully self-consistent transverse 2D Vlasov analysis was published by Li and Jameson [1]. It unifies the space charge driven 2D periodic focusing Vlasov analysis described in Ref. [12] with the 2D anisotropic smooth focusing Vlasov approach of Ref. [25]. Li and Jameson's recent work thus allows self-consistent 2D stopband calculations for arbitrary k and l. As common to all Vlasov models discussed here, the Landau damping mechanism is excluded. It is nonetheless unfortunate that this work—published in PRAB—did not get referenced by the authors. It would have allowed them to calibrate consistently the 2D C_m factors rather than taking a heuristic extrapolation from 1D.

Computer simulation with the WARP-code is used by the authors to support their conjectures leading to the construction of 2D resonance conditions out of the original 1D C_m factors. Results of their high resolution scans for Gaussian beams (Figs. 4 and 6 for odd n') show that strong second order parametric (half-integer) modes are clearly visible; third order parametric modes are shown to have an emittance growth of few percent; fourth order modes around one percent, and still higher order modes—if visible—even less. Needless to say that such small values are within the error bars of most emittance measurement devices, and nobody would worry about them anyway. But more important is the justified concern that the detected small rms emittance growth features could be equally due to space charge driven resonances of doubled order, which are not of the half-integer, parametric type (see below Ref. [2] for a detailed discussion). A more coherence-sensitive data analysis could have helped to resolve this ambiguity.

From an experimental point of view the main issues of concern for real accelerators are large values of rms emittance growth (few tens of percent) and, most of all, beam halos which might lead to beam loss. No evidence is given in the paper that for Gaussian beams and more than 200 cells such an exponential growth indeed occurs, and that Landau damping is not effective to prevent it. So why worry about the claimed higher order half-integer lines?

Finally, it is worthwhile testing the claimed coherent behavior of third order, but *externally* driven resonances (Fig. 13) against experimental data obtained from real accelerators, which have storage times several orders of magnitude longer than the authors' simulations.

We refer to published data from carefully conducted high intensity beam dynamics experiments using sextupole errors at the GSI SIS18 [3] and the CERN Proton Synchrotron [4]—none of them referenced by the authors. In the comprehensive SIS18 benchmarking campaign, with results shown in below Fig. 1, significant levels of rms emittance growth and beam loss have been measured and simulated by scanning the working point across a horizontal third order error resonance (at $3Q_x = 13$), with results for each working point shown after as many as 10^5 turns. Note that here Q_x stands for the bare machine tune. Space charge is simulated for the large number of 10^5 turns by adopting the frozen space charge model (FSM), which uses the unchanged initial density distribution for space charge calculation. Thus, any coherent motion—if it would occur—is suppressed, which is equivalent to $C_m = 1$ for all m. Shown are the results of measurements and simulations for a coasting (left column) and a bunched beam (right column). The simulations are performed with a Gaussian beam consistent with measured profiles.

Note the excellent agreement of the measured and simulated data as far as center and width of stopbands. Assuming a $C_3 = 0.77$—as suggested in Fig. 17—and applying the above Eq. (1) would result in a hardly resolvable coherent shift of the stopband in ΔQ_x by -0.003 for the measured coasting, respectively -0.005 for the measured bunched beam. Yet there is no indication of such shifts or related coherent effects in the data analysis. This is not surprising, because the large emittance growth is explained in Ref. [3] in terms of repeated scattering (or trapping) of individual particles on a nonlinear resonance, which has no coherent constituent. This scattering of single particles is by nature an incoherent long-term phenomenon. It is absent in the short-term WARP simulations by the authors, but it is the dominant process for emittance growth and loss in synchrotrons or storage rings. Note that the tiny rms emittance growth in above Fig. 1 (coasting beam simulation) close to $Q_x = 4.35$—the very small amplitude core particles—is not unphysical, but also understood as result of the scattering on the resonance.

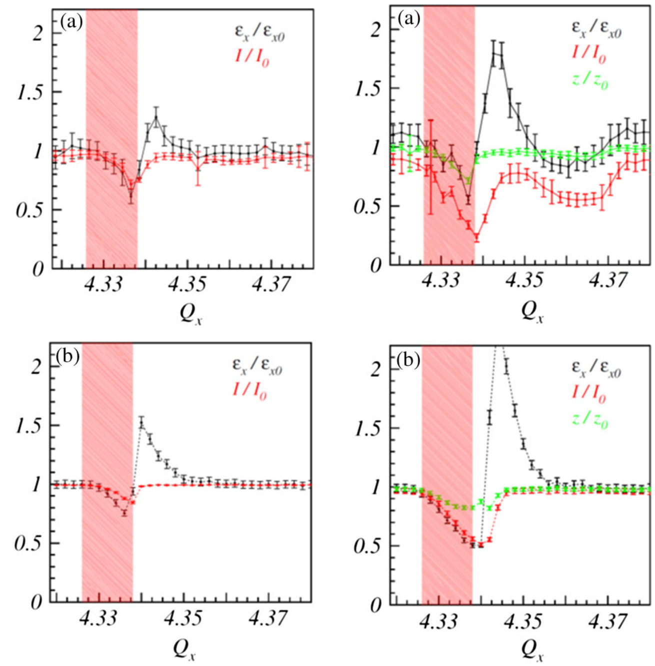

FIG. 1. Results of SIS18 measurements (a) and frozen space charge (FSM) simulations (b) near a sextupole error resonance at $Q_x \approx 4.33$ after 10^5 turns. The comparison is in support of incoherent resonance response for a coasting beam (left column) as well as for a bunched beam (right column). Maximum incoherent tuneshifts in x are -0.025 for the coasting, and -0.04 for the bunched beam. Shown are relative changes of rms emittances and current versus the bare tune Q_x, furthermore the length change for the bunched beam. Source: Ref. [3].

For comparison, the red hatched area in Fig. 1 marks the stopband obtained from measurements in the low intensity regime. The fact that the measured loss near this area is enhanced compared with simulations is attributed to the not sufficiently well-known dynamic aperture.

A similar measurement was carried out in the CERN experiment, which was compared with the FSM and an "adapted" FSM model employing rms size updates. The two FSM approaches practically agreed and were in good agreement with the location and width of the measured stopband.

So why should a coherent shift be used if there is no experimental evidence for it in these detailed and highly realistic third order resonance studies?

In conclusion, we find that for Gaussian distributions the authors' proposal of a new resonance diagram based on coherent modes is not sufficiently justified by their theory considerations and their accompanying simulations. Moreover, the CERN and GSI experiments suggest that there is sufficient agreement between the incoherent FSM concept and the experiment as far as concerns determining stopbands. The width of these stopbands, in particular, is determined by the effect of scattering of individual particles on the resonance over the large number of turns common to all real accelerators, which is far beyond the physics described by the author's few-turns WARP-simulations. Scans like those in Fig. 1 need to be carried out for specific lattices including working points, distribution functions, chromaticity, number of turns etc. Ideally, such studies should be carried out with self-consistent simulation codes, since FSM models are suspected not to model sufficiently well larger deviations from the initial beam. A discussion of the problems with self-consistent simulation of a large number of turns—besides impractically long cpu times—is beyond the scope of this Comment.

For further clarification: The authors findings on half-integer coherent resonances for waterbag or similar truncated distributions (not subject to Landau damping) are not questioned here. Therefore, it would be valued for future theoretical progress if S-POD experiments could shed light on the role of the initial distribution for Landau damping. This notwithstanding the fact that under normal operating conditions (not including scraping or other severe beam loss) all operating synchrotrons or storage rings are considered—with good reasons—to work with Gaussian-like distributions. It may be unnecessary to add that detailed measurements of actual beam profiles, rms emittance evolutions and loss effects as well as complete sets of lattice nonlinearities—as performed for the GSI and CERN experiments—are the key to successful benchmarking of theoretical concepts. More such accurately performed measurements are needed to further advance our understanding of this relatively complex area and clarify still open questions.

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