Reply to "Comment on 'Quasiperiodic spin-orbit motion and spin tunes in storage rings'"

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We reply to Lee and Mane's foregoing Comment [Phys. Rev. ST Accel. Beams **8**, 089001 (2005)]. In particular, we discuss how an adherence to certain notions of spin-orbit resonance and spin tune can limit the analysis and understanding of phenomena. Since the Comment has very little to do with the main thrust of our paper we take the opportunity to point out the main features of the "proper uniform precession rate," a concept introduced in our paper and based on the concept of quasiperiodicity. We also respond to other material in the Comment.

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We welcome the opportunity to discuss again, and to further illuminate, issues surrounding the precise definition of spin-orbit resonance described in our paper [1] which develops the earlier work in [2–4].

Since the foregoing Comment by Lee and Mane [5] relates to just a few sentences in the summary (Sec. X) in [1] and has little to do with the bulk of the paper, we will begin by reminding the reader that the main aim of our paper is "to provide a rigorous discussion of the concept of spin precession frequency on synchrobetatron orbits and thereby consolidate a framework for systematizing and classifying spin motion in storage rings . . . ."

In particular we focus on the frequency spectrum of the spin motion on a fixed torus for integrable orbital motion and execute our program using the concept of quasiperiodic functions. Our spin tune, as defined in Sec. V, is just a specialization of the concept of proper uniform precession rate and the definition of the latter is made possible by the quasiperiodicity of the orbital motion. We say that a spin tune exists if and only if the set of proper uniform precession rates is the same for each value of the orbital phase and is nonempty. If a spin tune exists, the torus has at most countably many proper uniform precession rates. Although it was not emphasized in [1], the definition of the proper uniform precession rate is so simple that it neither involves an invariant spin field nor the invariant frame fields introduced in Sec. VI. Our spin tune is arguably the most natural translation of the concept from the Hamiltonian language of the works of Derbenev and Kondratenko [2,3] and of Yokoya [4] into the non-Hamiltonian language and finding such a translation was our main motivation. Note that the proper uniform precession rate has a considerably wider range of application than the spin tune because in general it even exists on orbital resonance. Thus it can be applied to the case of snake "resonances" discussed below although we do not actively treat these in [1].

It has become convenient common practice among workers who are not familiar with the material in [1–4] to define spin-orbit resonance in terms of the spin tune on the closed orbit. However, those who are familiar with that literature realize that such a definition has the potential to limit analysis of phenomena. Nevertheless, such a definition can be useful if an amplitude dependent spin tune [1] exists and if the closed orbit spin tune is a good approximation to it. Moreover, it is the closed orbit spin tune that often emerges in perturbative calculations of the invariant spin field. See, e.g., [6]. At low values of $G\gamma$ this procedure often suffices.

However, in situations, such as in the models involving the single resonance approximation and two thin lens Siberian snakes [7], and for the snake-"resonance" tunes, there is no amplitude dependent spin tune. On the contrary, for orbit amplitudes large enough to be important, the eigentune for the appropriate multiturn spin map, which can be shown to be proportional to a proper uniform precession rate, has significant dependence on orbital phase so that no unique spin precession frequency can be assigned to a particle trajectory. Thus the relation in Eq. (4) in the Comment does not normally express spin-orbit resonance even in the approximate sense mentioned above. Of course, although the spin tune on the design orbit is not the spin tune required for the spin-orbit resonance condition—which does not exist in this case, it *is* the case that for small enough orbital amplitudes, the dependence of the proper uniform precession rate on the orbital phase will be weak. In that case it could perhaps be argued that for the orbital tune of a snake "resonance," the system is near to a spin-orbit resonance. The possibility that proper uniform precession rates with weak dependence on orbital phase can be useful parameters for characterizing the system was implied in [7] and in Sec. X of [1]. Of course, even a weak dependence on orbital phase usually implies that there are uncountably many proper uniform precession rates. It was also pointed out in Remark 4 in Sec. IX in [8] that proper uniform precession rates with weak phase dependence might be useful for characterizing an approximate resonance condition.

Note, with reference to the second paragraph in the Comment, that in none of the works by the authors of [1,7] is there a denial of the existence of snake "resonances'' or of the pioneering work by Lee and Tepikian on the subject. If snake ''resonances'' were not important, no one would discuss them and we would not have looked at the subject. The comments in [1,7] refer solely to interpretation.

In our opinion the subject is best served if, as in other branches of physics which are reaching maturity, one adopts rigorous definitions and then, where necessary, relaxes the definitions in a well defined way to cover other cases that do not fit. This is surely the best way to make progress, avoid confusion, promote meaningful discussion and illuminate phenomena. See, for example, [9,10]. In the latter it is shown how, with the our careful definition of spin tune, the orders of higher order spin-orbit resonances can be correctly assigned. Thus a ninth order resonance is identified. If naive definitions of spin tune and of spin-orbit resonance had been used, that would not have been possible. Those papers also show that with our careful definition of spin tune, there is a tendency for exact spin-orbit resonance to be avoided.

It is stated in the Comment that snake ''resonances'' were rederived by Mane in [11]. In fact Mane had simply found that his chosen parametrization of the invariant spin field, namely, one in terms of continuous functions of the orbital phase, broke down at snake-''resonance'' tunes. Nevertheless, in Figs. 7 and 8 in [12], he presents continuous curves and they are accompanied by a commentary emphasizing their continuity. We therefore invite the reader to consult [13] where the crux of the matter is explained and some correct curves are shown.

With respect to the commentary about snake ''resonances'' being hidden: note that nothing is being hidden in [7]. From the context and statements in that paper it is clear that it is being pointed out that contrary to the tendency of practitioners to think of snake ''resonances'' as effects that appear during acceleration, they are already troublesome at fixed energy and one does not need to do simulations of acceleration in order to find exotic behavior. See [13] for a discussion about just how exotic the behavior is.

We now come to the suggestion in the Comment and in [14] that the oscillations at nonzero betatron amplitude of the eigentune associated with the one turn spin map (called the ''perturbed spin tune'' by Lee and Mane) can point to the instability of spin motion. Note that the corresponding eigenvectors usually do not even satisfy the T-BMT equation. We refer the reader to [1,7] for comments that we have made in the past. The Comment mentions three examples.

We begin with the material appertaining to the variation of the spin precession rate due to energy oscillations. This has no connection with the dependence of the ''perturbed spin tune'' on the betatron phases at nonzero betatron amplitudes and it therefore has no relevance in the current context. However, for a coherent explanation of how to include energy oscillations, we direct the reader to the paragraphs about energy oscillations in Secs. I and X of [1].

We now deal with the material relating to Fig. 2 in the Comment. In this case the resonance strength from the orbital motion is very small but a significant and dominating imperfection is included. Since, by design, the dependence of the ''perturbed spin tune'' on the orbital phase almost vanishes, the ''perturbed spin tune'' in this example essentially reduces to the closed orbit spin tune discussed in Sec. III of [1]. Thus this example is trivial and it is not a topic for a discussion within the current context.

The material relating to Fig. 1 on the case of no imperfections, is not backed up by the kind of high quality mathematical analysis that we think would be appropriate. To be of value, such an analysis should demonstrate the relevance, i.e., the ''predictive power,'' of the ''perturbed spin tune'' for a variety of different spin-orbit systems with purely vertical orbital motion. Such an analysis should include a study of the effect of the rate of crossing, due to betatron motion, of any resonance which it is claimed is being crossed. We therefore feel that no useful purpose would be served by commenting on the analysis presented by Lee and Mane.

With reference to the last sentence of the Comment: we invite the reader to inspect the plots in [10].

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