Magn. Reson. Discuss.,
https://doi.org/10.5194/mr-2020-18-AC2, 2020

MAGNETIC RESONANCE
Open Access
Discussions

Interactive comment on "Using nutation-frequency-selective pulses to reduce radio-frequency field inhomogeneity in solid-state NMR" *by* Kathrin Aebischer et al.

Kathrin Aebischer et al.

maer@ethz.ch

Received and published: 13 August 2020

*The introduction gives an effective overview of prior work. I would add the method proposed by Odedra and Wimperis (DOI: 10.1016/j.jmr.2013.04.002) for quick-and-dirty imaging of the RF inhomogeneity using the z shim (i.e. not needing a magic-angle gradient). I feel it should be clearer that this work builds, conceptually at least, on the 2002 Charmont paper cited. This can be done by moving the sentence beginning "Alternatively" to the start of the final paragraph.

We have added the reference to the work by Odedra and Wimperis on line 21: " ... but also simpler methods using the z shim have been proposed {Odedra:2013dw}". In

addition, we have changed the breaking of the paragraphs to emphasize the Charmont paper more clearly.

*The theory is mostly clearly explained, although some wording is a bit unclear which meant I was reading some sentences a couple of times to understand the point being made. For example, we don't really "apply RF orthogonal to the static magnetic field" (at least not in MAS probes). The truncation of the Hamiltonian at high field could be more carefully expressed, particularly as this is important for the following step. I similarly didn't understand the term "Larmor-frequency axis" (a frequency can't have an axis).

There are many textbooks that discuss in detail the Bloch-Siegert shift, wo we decided to only show the static part and neglect the counter rotating part in Eq. (2). However, we have made this clearer by changing the sentence before Eq. (2) to: " Neglecting the counter-rotating part, we obtain a ...". We have rephrased the (admittedly awkward expression) "Larmor-frequency axis" to: "Using an appropriate phase or amplitude modulation will allow us to implement frequency band-selective pulses in the normal rotating frame {Emsley:2007ej}."

*I wasn't entirely convinced by the analogy between the spin-lock frame and the (Larmor) rotating frame; it is clear that $\omega 1$ is much smaller than the Larmor frequency, but much less obvious, e.g. Fig. 1(c), that $\omega 2$ is much less than $\omega 1$. The breakdown of this approximation is referred to, but it is not clear how this would manifest itself in practice. Wouldn't it show in Fig. 2 as deviations between the simulated profile with and without the spin-lock component?

Originally, we expected that the Bloch-Siegert effect would be very important in this situation where the amplitude of the IBURP pulse is less than an order of magnitude lower than the spin-lock amplitude. However, numerical simulations (as in Fig. 2) using a circular-polarized field show almost identical profiles as in Fig. 2 and zero inversion if the counter-rotating circular-polarized field is used. We have added a figure to the

SI (Fig. S05) that shows these simulations and refer to these simulations in the figure caption of Fig. 2. We believe that the main reason for not seeing any significant Bloch-Siegert effects is the fact that the second-order Bloch-Siegert terms will point in the direction of the spin-lock axis and lead only to a change in the magnitude of the spin lock. Since the pulses have an almost square excitation profile, such additional Bloch-Siegert fields will only have a significant effect at the edges. We have added a sentence to the figure caption of Fig. 2: " Simulations using circular-polarized radio-frequency fields that address the role of possible Bloch-Siegert effects can be found in Fig. S05 of the SI."... while the blue line uses $\nu2$ = ... a s it would be the case in a real experiment with rf-field inhomogeneity."

*In terms of the simulations, I didn't really understand the significance of the blue line. Is this trying to approximate the effects of RF inhomogeneity without assuming a particular RF profile?

For the black line, the IBURP pulse has always the ideal rf-field amplitude and only the spin-lock changes. For the blue line, the amplitude of the IBURP pulse is assumed to be proportional to the spin-lock amplitude as it would happen in a real experiment with inhomogeneity. This is explained in the figure caption. We have extended the last sentence in the figure caption of Fig. 2 to read: "

*The experiments are all clearly described. The disappearance of the modulation side-bands when using the selective excitation pulse (Fig. 5) is interesting. Could this simply be explained by the breaking the rotor cycle periodicity by the time-dependent (and unsynchronised) shaped pulse?

The modulation side bands also disappear under a spin lock without a pulse or they would be present in the difference spectra. We do not fully understand this yet as it is also stated in the main text and would prefer not to speculate more about this.

*The results obtained from the two different probes (Figs 7 and 8) were strikingly different and I feel deserve more comment. In particular, the response of the zero-frequency

artifacts is very different. The artifact is much larger in Fig. 7, but vanishes when the shaped pulse is used, whereas the artifact is much smaller in Fig. 8 and the changes are much more modest. Is the behaviour in Fig. 7 reproducible? It is not obvious why the selective excitation would strongly reduce zero-frequency artifacts, and given that this is highlighted in the conclusions, it would good to understand this point a little better. There is some repetition in this section, e.g. Hellwagner 2020 doesn't need to be cited twice to make the same point. (Similarly for Bloch-Siegert earlier.)

This must be a misunderstanding. In Fig. 7, we compare PMLG spectra with and without rf-amplitude selection and the zero-frequency artefact is strong in PMLG without selection of the amplitude. In Fig. 8 all spectra are with rf-amplitude selection and show very small artefacts. The artefacts are slightly bigger if we put the carrier in the center. We believe that the zero-frequency artefact comes from parts of the rotor where the rf field is much lower than the desired amplitude and these parts are suppressed by the selection. We hope this clarifies this question.

*Figure 8 is quite hard to read. It is not clear why different modulation frequencies were used for different shaped pulse lengths.

The different modulation frequencies were chosen to make sure that the pulses of different band width are centered in the region of maximum intensity of the nutation spectrum. This requires a lower modulation frequency for pulses with larger bandwidth. We have added the following sentence to Fig. 8: " The modulation frequencies were selected such that the band widths of the different pulses cover the region of maximum intensity in the nutation spectrum."

I'm not convinced that the linewidth data in Tables 1 and 2 is that useful in the main text, since I doubt that the quantitative results will be very reproducible - certainly not to 1 Hz! For example, there is a large reduction in FWHM specifically for the 16.8 ppm peak in Fig. 7, while the peaks behave much more uniformly in Fig. 8. The width of the 16.8 ppm in Fig. 7 looks more like a phase artifact (perhaps associated with the homonuclear decoupling?) rather than a tail due to RF inhomogeneity. The interacting nature of effects is a major headache for these experiments, but this is why a more detailed discussion of the weight that can be put on the quantitative results would be helpful.

We believe that the line width data illustrate the line narrowing by restricting the rf-field inhomogeneity quite nicely but we agree that a three-digit precision is not warranted. We have rounded the line width data to 10 Hz and we have also corrected the phasing problems in 7c that lead to the too large line width. Thanks for catching this. We have also rechecked the rest of the line width data and updated the table. Figure 8 shows only data with rf-field selection and illustrates the narrowing associated with longer selection pulses (narrower rf-field distribution).

*It's good that the data sets are available on request. It would be even better if they were available via an open data repository!

We will consider making the data publicly available in a repository.

Minor typographical issues:

*Not all symbols are defined, e.g. is τp in Fig 3 the duration of the shaped pulse or the combined duration of spin-lock + shaped pulse prior to nutation measurement?

The duration τp is the length of the IBURP pulse and at the same time the length of the spin lock since the spin lock was always adjusted to the length of the pulse.

*Axes are conventionally italic, e.g. x.

We have tried to change all axes labels in figures and in the text to italic. I hope we did not miss any of them.

*In line 18, there should probably be a comma after "NMR". As currently worded, the sentence implies that there are some MAS probes in which the coil is not close to the sample. Adding the comma creates a separate clarifying clause which applies to all

small MAS probes (probably the intended meaning).

This is indeed correct. We added the comma in the revised text.

We would like to thank Paul Hodgkinson for carefully reading the manuscript and for his suggestions which, we believe, improved the paper. We hope that these changes address all the issues raised.

Interactive comment on Magn. Reson. Discuss., https://doi.org/10.5194/mr-2020-18, 2020.

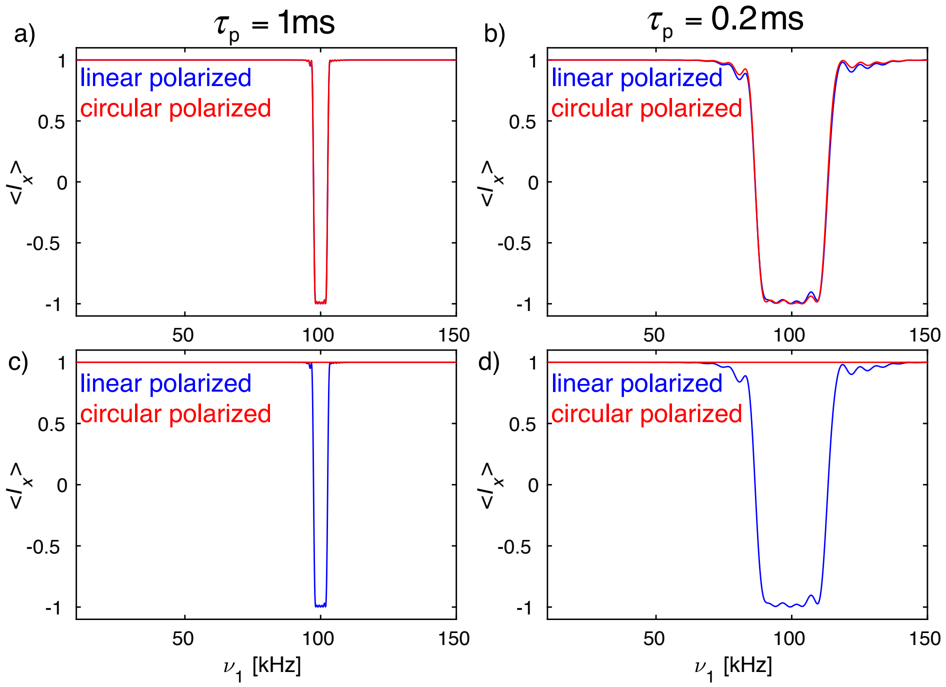

Fig. 1. New SI Figure S05 top row is linear vs. circular (+) and bottom row is linear vs. circular (-) polarized rf.