Comments on Leo's draft memo *polconvert_pseudoStokes_comparison.LB_v20240901.pdf*
Brian Corey / 2024 Sept 10

Some of my comments are carryovers from comments on Leo's 2024 Aug 12 draft. Some of my suggestions may be more appropriate for his upcoming memo on a comparison for a standard geodetic session.

1. How well do the LinI and CirI delays close? How about the LinI-CirI differences? Do the differences close better than the undifferenced delays? They should, if source structure is the dominant cause of MBD misclosures.
2. The 'relerr' as defined in equation (7) and tabulated in Tables 1 and 2 is not meaningful for MBD and SBD because the value of $\overline{\mathrm{PRM}}$ in equation (6) depends on the theoretical delay model used in correlation. (The MBD and SBD values in the memo are presumably O-C residuals.) One could, e.g., change $\overline{\mathrm{PRM}}$ by 1000 ps for all scans simply by changing the clock model by 1000 ps. Or one could change $\overline{\mathrm{PRM}}$ by adjusting the channel additive phases. Relerr for SNR is, however, meaningful.
3. In paragraph 3 of Section 4, it is noted that "the average errors...are very small...for MBD." That conclusion is based on comparing LinI-CirI differences with the mean MBD. Per the preceding item, however, the value of the MBD is not a meaningful standard against which to compare the differences.
4. A meaningful standard is instead provided by the theoretical standard error for the undifferenced delay. For the SNRs in this session, the theoretical MBD error $\sigma_{theor}$ is typically <0.2 ps. The values of MBD σ in Table 1 range from 0.45 to 5.74 ps. The scatter in MBD differences is therefore much larger than $\sigma_{theor}$, at least for the high SNRs in this session. At these high SNRs, however, effects other than thermal noise are likely to increase the MBD standard deviation to well above the theoretical value. An often-cited target MBD error is 4 ps. Five of the baselines in Table 1 have σ > 2 ps. If it is only slow drifts over time scales of 10-20 minutes, say, that are responsible for the larger σ values, these drifts will be absorbed into the clock model in the analysis of a geodetic session. If it is scan-to-scan variations in LinI-CirI that cause the large σ values, however, this will pose a problem in the geodetic analysis for either the LinI or CirI data, or both, depending on which data set is "wrong". As seen in Figure 3, often the largest changes in MBD occur from scan to scan. A comparison analysis of a geodetic session should shed light on the time scale of the changes in LinI-CirI.
5. There is no obvious (to me!) correlation between MBD σ (Table 1) and SNR (Table 3). This lack of correlation is consistent with the fact noted in the last item that the MBD σ is much larger than the theoretical standard error for undifferenced MBD.
6. A more useful standard for comparing SNRs than differences may be fractional differences. For instance, an SNR difference of unity at SNR=10, say, is more significant than at SNR=1000.
7. While a bias in MBD or SBD is unlikely to have any effect on the geodetic analysis, provided the bias is constant in time and it closes, a bias in SNR may be significant on many counts. The values of $\overline{\mathrm{SNR}}$ and $\overline{\mathrm{Bias}}$ in Table 3 allow an approximate estimate of the fractional SNR differences. On V baselines, the fractional difference $\overline{\mathrm{Bias}}$ / $\overline{\mathrm{SNR}}$ is 3-5% and positive (LinI SNR > CirI SNR), while on all other baselines, the fractional difference is <3% and negative. Is there something unusual about the V-baseline phase vs. baseband frequency dependence, say, or about differences in the X and Y fringe amplitudes?
8. I remain confused about why, in most cases, the observed residual histogram has a narrower distribution than the normal distribution for the σ calculated from the observed differences, but I suspect it has to do with σ being inflated by outliers. As far as I'm aware, there is no reason to think the LinI-CirI differences should have a normal distribution. I'm in fact surprised by how well a normal distribution fits the observed distribution. There are ways to calculate σ that are

less sensitive to outliers. I suspect that, if one compared the observed distribution to a normal distribution for σ calculated by a less-outlier-sensitive method, they would agree well over the ±2σ or ±3σ range. The conclusion might then be that the LinI-CirI differences are normally distributed except for a small number of outliers.

9. Paragraph 2 in section 4: The LinI-CirI biases cannot necessarily be blamed only on PolConvert. It may be that LinI is wrong. As long as the biases close, a large bias causes no problem. Similarly, in lines 2-3 of Section 3, the phrase “errors introduced by PolConvert” implies the nonzero LinI-CirI differences originate in PolConvert errors. But the differences could just as well originate in errors in the LinI parameters. Maybe CirI is right and LinI is wrong! This memo says nothing about the origin of the differencs.

Quibbles:

10. In lines 5-6 of Section 2, both LinI and CirI data are said to come fom “the original...data set.” That’s a vague statement and should be clarified.
11. The expression for σ in equation (5) should have N-1 in the denominator, not N. So σ is a factor of $[N/(N-10]^{1/2}$ larger than RMSE.
12. Units in Tables 1 and 2 are ps, not μs.
13. Two lines below equation (8), what does “bins with sparse data...were grouped” mean? That they were ignored? That the data in those bins were lumped into one bin?
14. Start of last paragraph in Section 3: I disagree that “the Pearson’s test does not work.” It does work, and it shows that the differences are not normally distributed according to the calculated σ.