Double peaking in beam-off asymmetries: a fishing expedition

Rob Mahurin
MTSU

Analysis teleconference 2017-07-07 Fri
(updated July 7, 2017)

Outline

- Motivation for single-run analysis
  - Restate the problem: double-peaking in beam-off signal differences
- Run 38300 and neighbors: double-peaked M1 signal differences
  - Unphysical signal variations in one early time bin
  - Slow “sawtooth” signal in m1
- Summer-runs analysis
  - Reproduce general shape of Kabir’s double-peaking plots
  - Detector could look happier
- Spin flipper synchronization doesn’t seem to be correlated
- Summary remarks

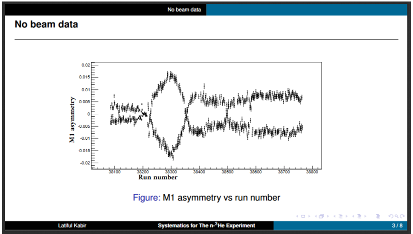
Does double-peaking in beam-off asymmetries affect beam-on measurement?

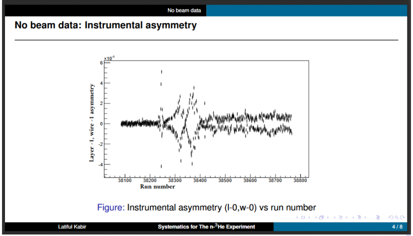
My understanding

- “Asymmetry” here means “signal difference in volts” since there’s no denominator to divide out — a one volt “typical signal” has been assumed
- These are “summer runs” with no beam and who-knows-what going on in the hall
- Run history

date	runs
2015-06-25	38081*–38124
2015-06-26	38125–38215
2015-08-03	38216–38301
2015-08-04	38302–38416
2015-08-10	38417–38493
2015-08-11	38494–38657
2015-08-12	38658–38769*

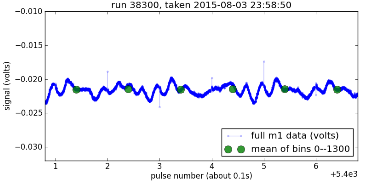
Kabir presentation 2017-05-19

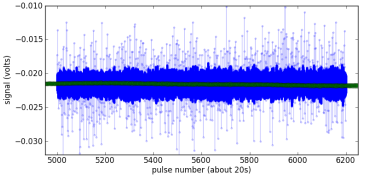


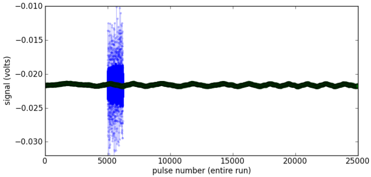


Run 38300, double-peaking m1 asymmetry in Kabir's analysis

Raw m1 signals look a little fishy





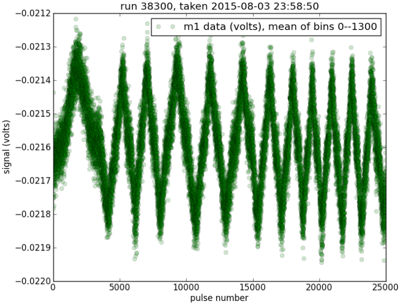


All plots this slide: same data

- blue points: raw M1 signal, 1624 time bins per pulse
- green points: mean M1 signal, one datum per pulse

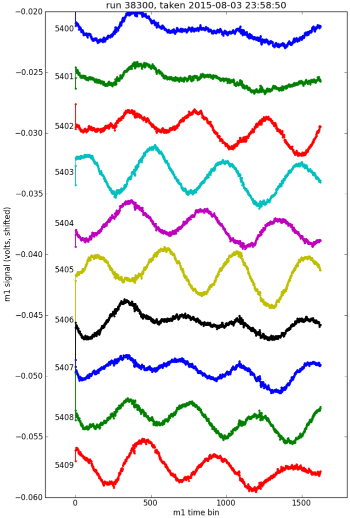
Observations

1. Outlying point is second sample (not first) in each pulse. Most samples have noise width 0.76 mV, but second sample in each pulse has noise width 3.8 mV.
2. Pulse-averaged data have slow sawtooth behavior, with slopes of order 0.3 μV/pulse



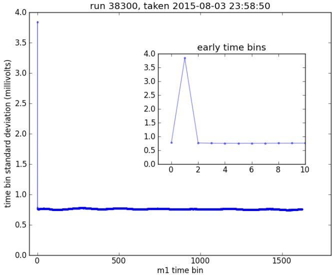
Which time bin is the outlying point?

Individual pulses (as previous)



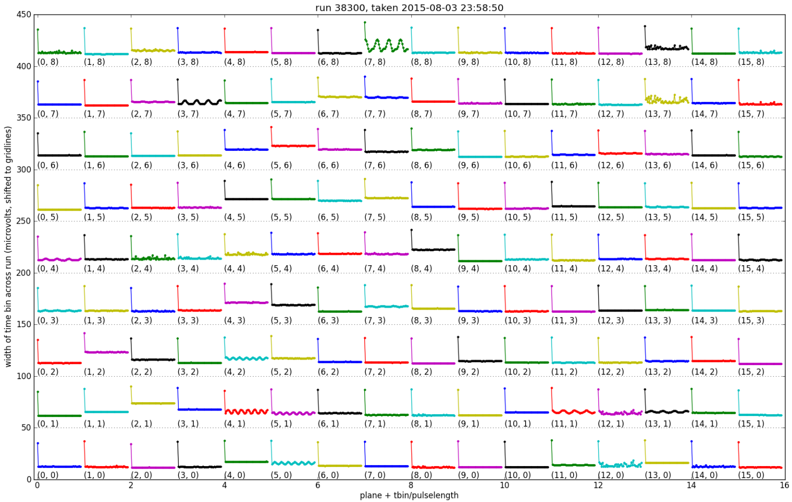
Pulse-to-pulse standard deviation

Analysis: Treat m1 raw signal data as a 25000×1624 matrix, take standard deviation along the long axis.

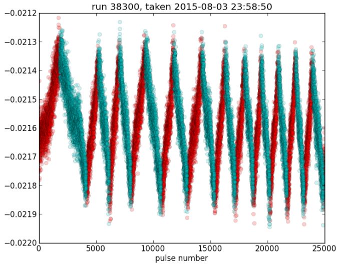


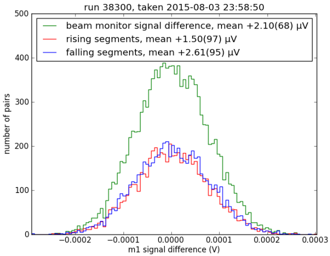
Is the bad time bin also present in the detector mean signals?

Yes. It's the *first* time bin rather than the second, but since the detector time bins are wider than the monitor that's not surprising. Some channels seem to be in phase with noise sources. Most detector time bins stable pulse-to-pulse to about 10 μV. (Most M1 time bins stable pulse-to-pulse to about 760 μV.)



Sawtooth consistent with Kabir's observed M1 asymmetry?

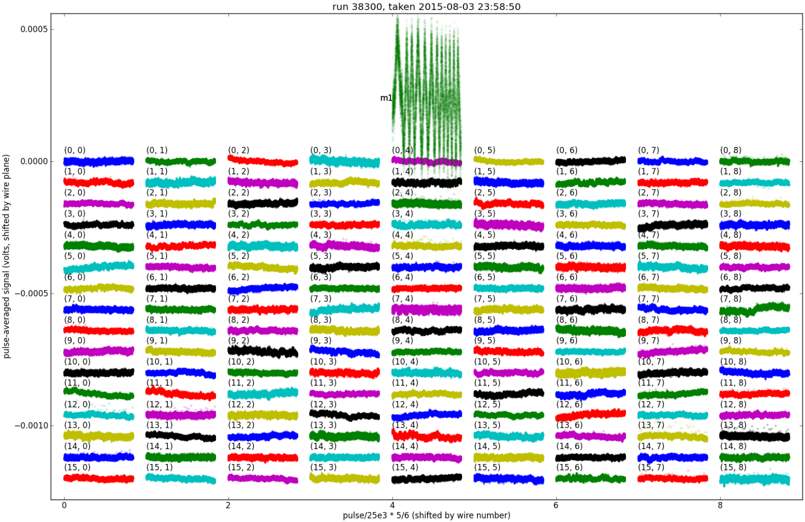




- Pencil fit to rising/falling sections gives slopes of order $\pm 0.4\,\mu$V/pulse.
- Computation of differences has second pulse in pair with negative sign, so predict "rising data" differences more negative and "falling data" differences more positive.
- That's what's shown here, but statistical power is weak for a single run.
- Also possible: spin flipper contamination of M1 signal in dirty DAQ.
- Identifying sawtooth waveforms by hand isn't scalable.
- These results disagree with Kabir's "M1 asymmetry" of ± 0.015 for runs in this neighborhood, or else I have misunderstood his units.

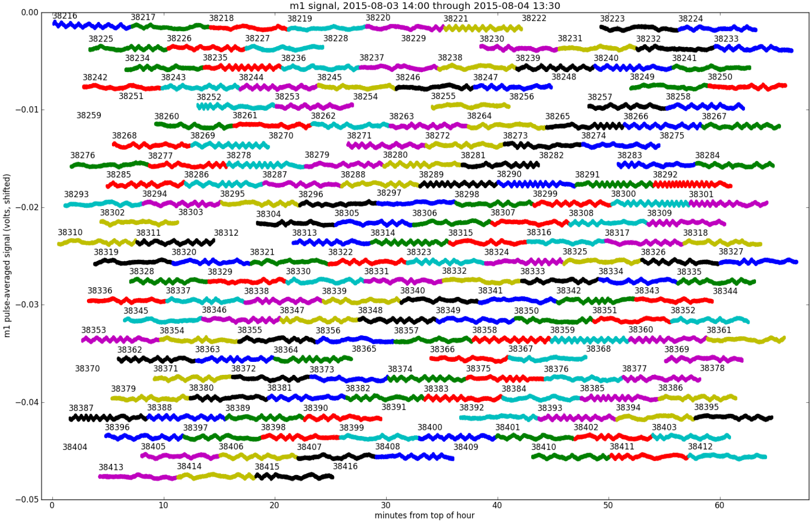
Is the sawtooth signal also present in the detector mean signals?

Not obviously. In this run, the detector means are much more stable than M1 means. They vary much less around their mean values (typical means $0 \pm 2\,\mathrm{mV}$, typical signal width $5\,\mu\mathrm{V}$). Even after zooming in, they don't show the slow sawtooth structure.



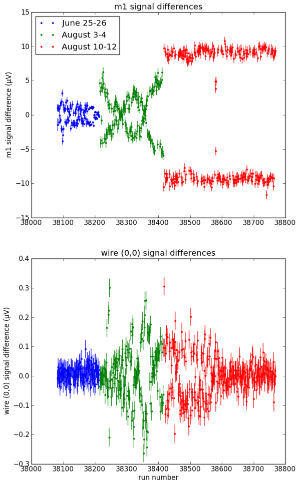
Is the sawtooth signal present in M1 in other runs?

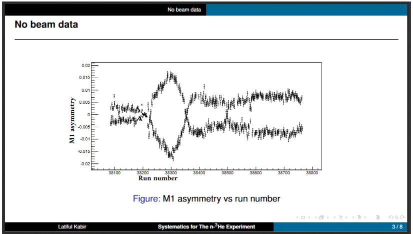
Yes: over the 24-hour test period containing this run, the m1 sawtooth in 38300 is pretty typical. (Runs with a number at the start time but no signal trace have a data-quality issue and are also missing from Kabir's run list.)

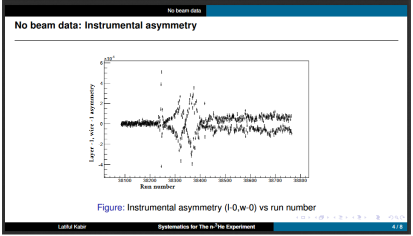


Do I reproduce Kabir's double-peaking plots?

In general shape, but with different sizes and different relative sizes of error bar.



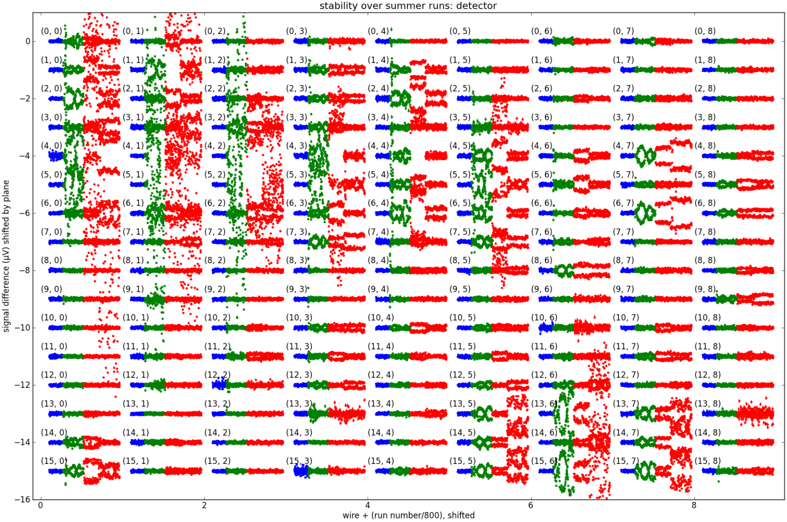




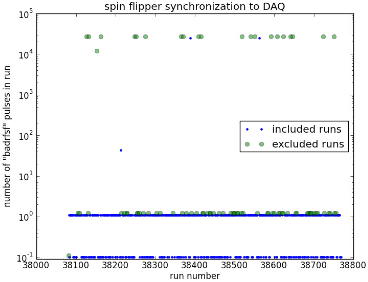
Note that, for the wire signals, $0.1\,\mu\mathrm{V} \approx 3\,\mathrm{ADC\ channel}$ thanks to summing over 36 time bins. Don't expect to see visible signal differences in this channel.

Not all detector channels look so promising

Here's the same analysis for all detector channels; note that wire (0,0) from the previous page is duplicated top left. Trouble spots show up as missing lines as their points invade others' territory, for example (4,3), (4,4), (4,5). Seems unlikely that (4,2) and (4,6) are experiencing common-mode noise that will cancel on subtraction.



Spin flipper synchronization doesn't seem to be correlated



- Most runs seem to have some funkiness with the spin flipper signal in the second pulse, then stabilize.
- "Excluded runs" have the data-quality issue alluded to earlier
- Two runs (38152, 38213) lose sync with the spin flipper partway through
- "Included runs" with all 25k pulses out of sync didn't contaminate the preceding analysis

Summary remarks

- This is less actual data time than I had realized — about 3.9 days.
- M1 sucks.
- In general, M1 signal pulse-to-pulse average signal differences (including bad second time bin) are $\lesssim 10\,\mu\mathrm{V}$.
- ☐ Is the $\sim 12\%$ of the data that I discarded fatally flawed, or is there a trick that this analyzer needs to learn?
- ☐ Might repeat the M1 raw signal analysis for the 2015-08-11 data, where differences seem to be larger (and larger than error bars)?
- When they're well-behaved, detector signals over one run differ pulse-to-pulse by $0 \pm 1\,\mathrm{ADC\,channels} = 0 \pm 31\,\mathrm{nV}$ over the 36 time bins, $[6, 42)$, included in my average.
- ☐ Might adapt the M1 raw signal analysis to a detector channel with troubling (e.g. (6,7)) or deeply troubling (e.g. (5,2)) noise behavior in summer data?
- ☐ This misbehavior might explain something confusing my spring student showed me in beam-on data — need to turn those notes into a proper presentation.