HI Chat-

Actually, we are (for now) relying on the mechanical registration of the ALMA feeds' linearly polarized receptors, which I think we expect to be good to ˜ 1deg or less (at least at the lower-freq bands). See below for more.

I promised Rosita I would work up some explanatory text for these more subtle parts of the guide, but I've been pulled away to other work over the past few weeks. Stay tuned for that over the next few days.

On Fri, Mar 6, 2015 at 3:44 PM, Chat Hull <chat.hull@cfa.harvard.edu> wrote:

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> Hi, Hiroshi--
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>
> Thanks for the quick response.
>
> Be aware that ALMA does not have circular feeds but linear feeds.
>> X-Y phase difference of the reference antenna does not rotate the
>> polarization P.A..
>> We do not use the known P.A. source as a reference, but solve X-Y phase
>> difference
>> by fitting the slope of cross-hand visibilities on the complex plane.
>> For more detail, see George's presentation (p.13-20) in Socorro.
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>
> 1) XY-phase fitting
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> I'll admit that I'm having a hard time achieving a hand-waving conceptual
> understanding from the slides in George's talk. :-)  However, I think the
> answer is, "because we have linear feeds," and linear feeds have an actual
> orientation relative to the ground (or the vertical, or gravity, or
> whatever).  And because a source with linear polarization will generate a
> unique response as a function of time in crossed-linear feeds.  How about
> this for a statement to put into the casaguide? --
>
> "Note that we can solve for the XY-phase without an absolute position
> angle (PA) calibrator like a polarized noise source or a stable
> polarization calibrator.  This is possible because ALMA has crossed-linear
> feeds, as opposed to the crossed-circular feeds at many telescopes
> including CARMA and the VLA.  Crossed-linear feeds respond to a linearly
> polarized source in an understandable way, thus allowing us to fit for both
> the absolute position angle and the source polarization fraction."
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No, I think this statement is conflating too many things. In fact, we _do_ want a stable polarization calibrator (w/ non-zero polarization), and we are _not_ fitting for absolute PA. Circularly polarized feeds also respond "in an understandable way", actually, just differently.

Sorry my slides aren't helping. Here's some more explanation. Some condensed version of this is the sort of text I'll try to add to the guide, I suppose, but my purpose for the moment is to help you understand. This isn't too long...

The 'XYf+QU' solve is doing two things:

1. It is measuring the XY-phase spectrum of the _gain_ (and bandpass) calibration refant. This phase bandpass shows up identically in all cross-hand correlations (conjugated between YX and XY, of course). It is entirely an artifact of the fact that the bandpass calibration applies no constraint to the relationship between the X and Y polarization systems; instead, it just forces the refant's phase in both hands to zero across the band. But actually, there _is_ an interesting bandpass phase relating X and Y on the gain/bandpass refant. The 'XYf' part of 'XYf+QU' is solving for this. We need a polarized astronomical source because we want to detect this net bandpass phase through the whole of the signal path. However, at this stage of the calibration, we don't know Q,U nor the instr. pol., so we look at the slope in the complex plane of the mean (over baselines) cross-hands for observations of the polarized source over a meaningful range of parallactic angle. The average over baselines tends to dampen the net instr. pol effect (but there is still a small complex offset), and the resulting signal should be dominated by the source polarization response evolving with parallactic angle. If the XY phase were zero, the source pol evolution would occur entirely in the real part (at least in the limit of zero instr pol, also)---i.e., a horizontal line. Non-zero XY phase rotates this horizontal line partly into the imag part. The slope _is_ the XY phase, it is different in every channel, and this is what the 'XYf' part is calculating. Note that it need not know what that source Q,U are, just that one or both are non-zero. (Strictly, the net instr. pol residual in the baseline average causes both and complex offset _and_ a parang-dep elliptical perturbation on the parang-evolving source polarization term, but this is typically very small.)

Note that there is _not_ a refant "for" the 'XYf' solve, in the sense of selecting one or applying one. The phase residual is present on all baselines for the gain- and bandpass-calibrated cross-hands, and the relevant "refant" is the one that was used for gain and bandpass calibration. I.e., it is a property of that antenna. However, the phase spectra stored in a cal table for this term is stored for one polarizaiton in _all_ antennas, because this is how to compensate for the refant operation applied in the gain/bandpass calibration. Note that gain and bandpass probably use the same refant, but this is not required. If they don't, then the XY-phase that is calculated as above describes the _shape_ of the bandpass refant with a chan-INdep offset related to the gain refant (or something like that). When you talk of the XY phase refant in the context of CARMA, I think you are referring to use of a calibraiton signal introduced on that antenna by which an XY phase is calculated by looking at that antennas cross-hand auto-correlation? Presumably, then the gain (or bandpass?) phase solution is forced to exhibit that offset for that antenna, one way or another.

Finally, note that if instr pol were zero _or_ if we could assume the calibrator polarization signal in the cross-hands was large enough to hugely dominate the instr pol (i.e., not ˜zero by bad luck of the specific parang we happen to
observe), then a single parang would be sufficient to solve for XYf phase---and the conventional 'cross-hand phase'
solution in polcal that is used for circulars (poltype='Xf', AKA pos angle calibration for circulars) would suffice. For linears, this would only get the cross-hand phase. To also calibrated the absolute PA, you would need to know the PA of calibrator a priori.

2. Once the XY phase spectrum is determined, the baseline-averaged cross-hand data are corrected for it (which moves the source pol entirely into the real part of the cross-hands) and averaged in frequency (which boosts SNR).
Then, the real part (as a function of parang) is solve for the _apparent_ Q and U. Note that this Q,U is not formally calibrated for _absolute_ PA.

But since it is solved from data for all baselines, it is only as bad as something like the _mean_ orientation error in the ensemble of feeds. This Q,U is recorded in the XY-phase caltable so that it can be compared (separately for now, but this will be streamlined soon) with the qufromgain result and the XY-phase ambiguity resolved (should the rotation back to horizontal be CW or CCW, essentially).

Now, after resolving the ambiguity and revising the gain calibration to account for the apparent source polarization, this Q and U is used in the source model for instr pol calibration, and so the orientation portion of the D-term solution (the real parts to a good approximation) are effectively relative to this assumption (0.5*atan2(U,Q)) about the position angle of the calibrator.   If there is a large systematic position angle offset, we are _not_ detecting it. All the instr pol calibration manages thus to do is align all feeds to the same position angle.

Note that a similar argument about the imag part---the ellipticity---applies w.r.t assumptions about the calibrator’s actual Stokes V (nominally assumed to be zero, for lack of anything better), and this is why continuum Stokes V is so hard.   If a systematic offset in ellipticity at a level comparable to the apparent dispersion in feed ellipticities (typically a few %), then we don’t know the absolute ellipticity accurately enough to measure astronomical continuum V at the levels at which is seems to occur: few 0.1% (in quasars, at least).

> 2) Absolute PA accuracy
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> *Critical question:* at CARMA we found that the PA accuracy was only as
> good as the reference antenna we used for the XYphase calibration. See
> column (9) of Table 2 in the CARMA polarization memo
> <https://www.mmarray.org/memos/carma_memo64.pdf>, where position angles
> for 3C286 varied from 39-49Âº. We’re not sure where those differences come
> from -- possibly the angles of the wire grids we used, but maybe leakages,
> etc. Only when we calibrated using the radial polarization of Mars were we
> able to get the correct answer for 3C286: column (9) -- column (5) = column
> (10).
>
> Is this also the case for ALMA? We’re fitting for the XYphase on *one*
> refant, which means that our PA answers are only as good as the positioning
> of the feed -- and any other strange optical effects that might be
> affecting that particular antenna. Has anyone tried looping over a number
> of refants to see if the PA values are the same?
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>

No, we are _not_ "fitting for the XYphase on *one* refant", at least not in the sense you seem to imply here. As described above, we are a solving for a property of the gain/bandpass refant using _all_ of the data, because the cross-hand residual phase shows up on all baselines.

And, the details of the XYf solution, specifically, have no bearing on the accuracy of the PA calibration whatsoever. To say this is to conflate with the circulars case (it has taken me many years to wean myself of this!). No, the PA accuracy is set by how well the feeds are constructed and mounted on the antennas according to the prescribed nominal position angles for them.   We can’t know this without some external reference.   Maybe 3C286 (and similar sources) can be used for this. Or the limb poln of Mars (not sure how accurate that can be). In the present case, I think we are effectively reassured that the mechanical registration is, in fact, pretty good, since we are getting results for 3C286 consistent with prior measurements---though we are no better than whatever systematic error they might have been subject to, of course!

In short, while the value for our derived PA is very precise (+- 0.03Âº)
> because of errors averaging out across antennas, it may not actually be
> that accurate. Let me know if I’m off base here.
>
>

You are correct here.   The precision is a net sensitivity issue.
Accuracy is limited by systematic errors, as yet not completely understood.

Cheers,
-George