Written Contributions to the PTCS In Progress Review Panel from Don Wells and David Parker

Compiled by Richard Prestage, Tuesday 2nd December 2003

1) Email from Don Wells, Thursday 27th November
2) Email from Larry D'Addarion, Monday 24th November
3) Email from David Parker, Tuesday 2nd December

Email from Don Wells, Thursday 27th November:

From dwells@nrao.edu
Date: Thu, 27 Nov 2003 09:29:49 -0500
From: Don Wells <dwells@nrao.edu>
To: Richard Prestage <rprestag@gb.nrao.edu>
Cc: Dave Parker <dparker@nrao.edu>, Don Wells <dwells@nrao.edu>,
Jim Condon <jcondon@nrao.edu>, David Woody <dwoody@ovro.caltech.edu>
Subject: Re: PTCS In-Progress Review

Richard, (etal)

Richard Prestage writes:
> If you would like to make such a presentation, ..

Larry D'Addario asked for my opinion last week, and I gave it to him, in a carefully worded 10_KB response. He then asked for permission to distribute my text to the reviewers, and I gave it. I expect that his action has resulted in my views becoming a part of the official record of the project. There is nothing which I would want to add to that statement, except miscellaneous details which I could supply if questions arise during the session on Wednesday.

A key duty which we (both PTCS and the review panel) have is to assure that the concept of laser rangefinding for large telescope control does not acquire the reputation that "oh, NRAO tried that for the GBT and it didn't work". In this regard, I especially worried that our technical counterparts in the Sardinian project will propose to use rangefinding, and will be met by that response. This would be bad for the progress of the state of the art.

> If you would prefer not to make a formal presentation, you will still of
> course be able to discuss your viewpoints with the panel. They will have
> time set aside on Thursday morning if you wish to discuss any topics with
> them in closed session.

I think that my 10_KB statement speaks for my viewpoint well enough.
I expect to depart GB about 16:15et Wednesday afternoon.

Regards,
Don

Email From Larry D'Addario to Review Panel: Monday 24th November

Subject: GBT PTCS review -- laser rangefinders
Date: Mon, 24 Nov 2003 13:47:46 -0700
From: "Larry D'Addario" <ldaddario@nrao.edu>
To: pnapier@nrao.edu, dwoody@caltech.edu, p.wallace@rl.ac.uk

Dear review panel members,

In connection with the upcoming "progress review" of the GBT PTCS project, you should be aware that the project leadership has decided to cancel all further work on laser rangefinders. Although the official status is that this effort is "on hold" for 6 months, steps have been taken that will prevent it from being revived. These include removing all of the instruments from their monuments and placing them in storage; cancelling a contract to construct weatherproof enclosures; and, especially, disbanding the technical team.

Whether this is justified is certainly arguable. The basis seems to be some calculations given in

K. Constantikes, "Accuracy of trilateration for the NCP experiment." GBT PTCS Project Note 22.1, GPT archive PR034, 2003-Sep-09.

However, please see the appended message explaining why those calculations are not very relevant.

I hope you will take steps to ensure that this decision is carefully considered during the review. It might be helpful to request presentations of views that differ from those of the project leadership, since no such presentations are presently scheduled.

Regards,
Larry

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Don Wells writes on 2003-Nov-27:
> Larry, (and Fred)
>
> Larry D'Addario writes:
> > K. Constantikes, "Accuracy of trilateration for the NCP experiment."
> > GBT PTCS Project Note 22.1, GPT archive PR034, 2003-Sep-09.
> >
> > .., what's your opinion of its (a) correctness and (b) relevance?
>
> Regarding the correctness of the Constantikes memo: as far as I know,
> the actual computations in the memo are correct. Even if the results
> are wrong quantitatively, they are qualitatively correct -- whenever
> you have skinny triangular pyramidal geometry, the trilateration
> produces larger errors in directions orthogonal to the long axis of
> the pyramid.
>
> Regarding your question about relevance: the answer is that simple
> trilateration of sets of retroreflectors is a rather poor way to make
> measurements for calibrating or controlling the GBT. There are two
> main reasons for this:
>
> * the XYZ coordinates are not the actual parameters which one wants
> to determine, and attempting to determine those parameters from the
> set of XYZ coordinates involves a statistical mess, and
>
> * the XYZ coordinates are affected by several sources of systematic
> error. It is hard to compensate the systematic errors when
> determining the parameters from the XYZ coordinates.
>
> The better way to approach the problem is to solve for the desired
> parameters directly, using a nonlinear least-squares model which takes
> the rangefinder measurements to the set of targets as inputs. The
> model implicitly constrains the relative positions of the targets. I
> use the word 'truss' to characterize such models -- one assumes that
> the retroreflectors are connected by a semi-rigid structure. This

> approach uses the covariances between range measurements and desired
> parameters directly and efficiently (in the sense of statistical
> efficiency).
>
> For example, we might want to determine changes of the orientation of
> the backup structure of the big mirror w.r.t. the encoders. We can
> range on retros attached to the edges of the gigantic truss structure.
> We assume that the truss is nearly rigid, specifically that its shape
> changes slowly. This means that ranges from the ground to any of the
> retros contribute to determining the angular offset between the truss
> orientation and the encoder readouts. The rigidity assumption means
> that we can use all of those ranges together to solve for the Az and
> El angular offsets. Changes in those offset angles are probable
> changes in the pointing error of the GBT, of course. The LS solution
> produces other outputs, such as the XYZ translations of the whole
> truss (probably produced by wind and thermal effects) and the
> effective temperature of the steel of the truss. It may be possible
> to solve for the variation of the air refractive index as well, but I
> am not confident about determining that parameter in the presence of
> steel expansion which is 10x larger (it would be better to have air
> thermometers at different heights and make an open-loop refractivity
> correction). However, systematic error due to refractive index
> differing from expectation largely cancels when making differential
> pointing corrections. The parameters produced by such a LS solution
> are exactly the physical quantities which we want to know, whereas the
> XYZ coordinates produced by simple trilateration of those retros would
> be nearly useless. Incidentally, the RMS errors of a backup structure
> truss solution will be independent of the Az/El velocities -- the
> truss technique works just as well for a moving truss as it does for a
> stationary truss.[1] Simple trilateration of a set of targets becomes
> a complete mess if the targets are moving!
>
> The Constantikes memo contains its own refutation, in the form of
> separate error distributions for trilateration to a retro on the left
> side of the feedarm and trilateration to a retro on the right side of
> the feedarm. Note that the long axes of the two error ellipsoids are
> nearly orthogonal (see the two perspective views facing each other on
> pages 8 and 9 of PTCS/PN/22.1). This means that if the range data
> from the left side were to be combined with the range data from the
> right side, and we were to solve for the XYZ position of the point
> which bisects the line connecting the two retros, we would have a much
> more favorable error ellipsoid. The reason for this is really simple:
> with rangefinders from both sides of the ring-of-fire contributing
> data the triangular pyramid will no longer be skinny -- the width of
> its base will be more than 200_m, quite favorable for measuring a
> target about 170_m above the base of the pyramid. This concept of

> solving for XYZ of the bisector point is not just theoretical, it was
> applied to geodetic total station data on the GBT feedarm three years
> ago.[2]
>
> More generally, it is bad practice to range directly between the
> ground and the feedarm. The first problem is that the feedarm is
> generally moving too fast. The second problem is that the geometry of
> the trilateration pyramids is unfavorable, as Constantikes shows in
> his memo. The fix for both of these problems is to introduce an
> intermediate set of targets at the rim of the big mirror, and range on
> them from both above and below. (Constantikes has proposed that we
> invest in acquiring retros which work over much larger solid angles in
> order to improve the geometry; while this is fundamentally a good
> thing, it doesn't solve the problem that the gigantic main mirror
> blocks many of the potential lines-of-sight which would use that solid
> angle.) The triangles for trilateration are much more favorable for
> the two-stage measurement. These intermediate targets are the
> 'triplet' retroreflectors which Michael Goldman designed. Their
> positions relative to the ring-of-fire can be determined by one truss
> solution, and the position of the feedarm can be monitored w.r.t. the
> triplets by the feedarm rangefinders, using a separate truss solution.
> In this concept the set of triplet retros define the reference frame
> for the coordinates of the optics attached to the (moving) tipping
> structure.
>
> In a number of cases 'common-mode' systematic errors can be largely
> cancelled by appropriate choices of measurement technique and least
> squares modelling. Two simple cases are the calibration of the
> subreflector Stewart platform and 'polishing' of patches of primary
> mirror panels. The concept of the latter application is that three or
> more rangefinders would trilaterate the surface retros over an area
> perhaps 5_m across and a paraboloid would be fitted to the ranges,
> probably by orthogonal distance regression. Residuals from the fit
> would be corrections to be applied to the actuator zero points. The
> corrections determined by this technique would be almost completely
> insensitive to refractive index variations.
>
> In summary, Constantike's memo does not provide a proper technical
> basis for the decision to 'defer' (or, more likely, cancel) the GBT
> laser rangefinder project.
>
> -Don
>
> PS#1: You will hear that trilateration from the feedarm to the retros
>   on the primary mirror will have errors larger than the goal for
>   3 mm operation. The measurement errors are roughly 2x the

> desired RMS. This 2x discrepancy was known from early in the
> project, more than ten years ago.[3] However, what you usually
> do not hear in such discussions is that there is a simple cure:
> increase the integration time from 128_ms (the standard default
> integration for the GBG rangefinders) to 512_ms, probably by
> averaging four measurements. All evidence to date is that the
> errors will improve by averaging in the usual normal
> distribution fashion. This 2x discrepancy has been cited as
> another justification for suspending work on the laser
> rangefinders; but this justification is without technical merit.
>
> PS#2: It has been customary to quote 100_um as the RMS precision of
> the GBT rangefinders. However, in practice many rangefinders are
> found to have considerably smaller RMS noise, often 40-60_um,
> and some rangefinders do even better. The best noise levels ever
> seen on open-air slant-range paths were below 20_um RMS! If one
> wants to present a pessimistic view of the potential for the
> technology, a way to do that is to always quote 100_um RMS as
> the nominal performance specification and never mention the more
> favorable numbers which have been seen in field trials.
>
> [1] 'Fitting Models to Simulated Rangefinder Data', Don Wells, GBT
> Memo 196, 1999-04-06. Abstract: "Simulated rangefinder data is
> fitted to estimate rangefinder coordinates, zero points and
> backprism offsets, and to estimate coordinates of target
> retroreflectors. The translation and tilt of trusses with
> retroreflectors attached are estimated from rangefinder data, for
> the cases of differential backup-structure pointing corrections
> and subreflector pose determination. The differential pointing
> technique is advocated for Phase-I implementation early in
> 2000. The simulation code shown here executes faster than the data
> acquisition process being simulated, so this code is a candidate
> for production use."
>
> [2] 'Analysis of cardinal point survey data', Donald C. Wells, GBT
> Memo 207, 2000-10-16. Abstract: "A least-squares model has been
> fitted to the data from a survey of retrospheres attached to
> ``cardinal point" nodes of the GBT feedarm and box structure and
> to the elevation bearings. The LS fit includes gravitational
> deflections from the as-built finite-element model of the GBT. The
> azimuth zero point (a traditional pointing model parameter) and
> the elevation of the elevation axle can be estimated from the
> data, and the vector offsets of the retrospheres from nearby nodes
> can be determined. The trajectory of the tip of the feedarm is
> discussed, including the deflection out of the
> plane-of-symmetry. The lateral position of the azimuth axis

> w.r.t. the ring-of-fire" is estimated from measurements of the
> four elevation bearing retrospheres."
>
> [3] 'Error Sensitivity of GBT Laser Metrology', Fred Schwab, GBT Memo
> 37, 1990-02-27. This memo discusses the precision with which
> feedarm rangefinders can measure the GBT primary mirror. The final
> sentence of the first paragraph of Section IV (Conclusions) is:
> 'The 4-rangefinder configuration.. would apparently achieve our
> goal of 100_um accuracy for surface setting, given range
> measurements good to 50_um.' I.e., Schwab is saying that there is
> a dilution of precision of about 2x for measuring the surface.

Email from David Parker Monday 1st December

From dparker@nrao.edu Tue Dec 2 19:57:00 2003
Date: Mon, 01 Dec 2003 18:44:02 -0500
From: David H. Parker <dparker@nrao.edu>
To: rprestage@prospero.gb.nrao.edu
Subject: Views on suspension of work on the laser ranging system

I have been ask to comment on the decision to suspend work on the GBT laser metrology system. Having worked on the GBT antenna metrology since joining NRAO in 1990, I probably have a little different perspective than most. While I have heard a lot of people express surprise that the laser metrology work has been suspended, I think that this decision should be viewed in a larger perspective. In order to see where the laser metrology system fits into the scheme of things we need to review some of the history of the GBT project.

The GBT metrology system was unique in as much as it was an opportunity to build the systems into the telescope construction and design. It was well funded and what should have been a classic demonstration of how to do things right. The idea was to use an engineering approach to do the pointing, surface corrections, confirmation of the finite element model, and provide structural health analysis. While the laser rangers were at the heart of the concept, they were only one element of the larger philosophical approach. Throughout the construction we tried to model, measure, and calibrate as many critical parameters as possible. Sebastian von Hoerner made what I thought was an excellent point in his GBT Memo 110 (11/17/1993) in which he said;

"Parameters, to be solved for, must first be defined. Sometimes this is done (or at least suggested) by just solving for a series of spherical Fourier terms up to a given order. But this I would call "Fishing in the Dark". Results may be used but won't tell you anything."

In that spirit, we tried to define parameters and measure as many as we could. JPL, Sri, Wells, and Goldman all worked out optical models of the GBT. I fought hard to calibrate the entire subreflector mechanism--but lost. We were allowed to calibrated the subreflector actuators, however. Goldman measured the receiver turret and made provisions to dowel the receivers to the flanges in order to maintain traceability to the receiver. Anderson measured the phase centers of the receiver on the test range. Lee provided the best estimates of the antenna deflections from the finite element model. We surveyed the as-built deflections of the feed arm which clearly showed the side deflections that the FEM did not model. We established permanent bench marks, referenced to the center of the elevation bearings, before the encoder shaft was installed and that reference line was lost forever. We used this line to establish the initial azimuth encoder offset, from a survey to the laser monuments, and had provisions to do a more accurate measurement of the zero by doing Polaris surveys to establish the reference line. We surveyed the center of the pintle room and tied it to permanent monuments before that point was lost. We measured the drift of the azimuth encoder and made people aware that it is temperature sensitive. We made hydrostatic level measurements of the foundation in order to monitor long term drifts and measured the deflections under the wheel load. And could have done more.

I bring all of this up because people think that suspension of work on the laser system was a major turning point. I look at it as only one element in the larger scheme of things. As I understand it, with the exception of the FEM being used for the surface correction as a function of elevation, and the surveyed location being used to find fringes; not a single engineering measurement or optical model has been used in the software to date. If the lasers were perfect, the software infrastructure does not exist to use the data.

Some will argue that the conventional 8 pointing equations are well rooted in a physical model. I disagree. For example: in addition to the azimuth and elevation, the subreflector has 6 degrees of freedom, i.e., there are 8 degrees of freedom available to point the antenna. It would seem that the combination of the 8 parameters is not unique, and surely the 8 conventional pointing equations coefficients are coupled together. For example, as I understand the present model, the subreflector tilts are constrained to be 0. I find it hard to believe it is really so simple. This most probably introduces changes in the eight coefficients. Are we to believe the encoder zeros change, or the difference in the elevation bearing heights change based on whether or not the subreflector is tilted?

I maintain that a proper model of the repeatable pointing will require approximately 50 terms to get it down to sub arc second errors. The subreflector actuators are presently using the wrong linearity scale factor and make no corrections for non-linearity. This has to be introducing 6 errors. We know that the azimuth encoder has a temperature dependence. In addition to the zeros there is probably a sinusoidal cyclic error in both encoders which accounts for 7 sources of error. We know the left elevation bearing is higher than the right bearing (not to mention the temperature dependence). This must introduce an error. The reflector, feed arm, and receiver room were set based on the gravity vector, so the antenna was built at an angle with respect to the shaft. The feed arm deflects in all three directions for 3 more terms. The reflector distorts as a function of elevation for 3 more terms. This adds up to 21 independent parameters, without the track---just to cover the obvious repeatable errors.

As far as I can tell, the only attempt to reconcile the engineering measurements with the pointing model was a 4/2/2001 memo by Ghigo and Maddalena, Comparison of feed arm metrology with focus tracking measurements. After talking with them I found that they had not taken into account the fact that both the subreflector and receiver move together on the feed arm, so the report would seem to be in error.

In my opinion, the opportunity to use an engineering approach was lost once we started down the wrong road three years ago and I don't see much hope (or enthusiasm) for setting it right. The astronomers seem pretty happy with the GBT as it is. I am sure that given enough telescope time, the astronomers will get something to work. The problem with the GBT is that we have already used 15% of the design life of the telescope and have only achieved mediocre performance. Moreover, the track is deteriorating, and the clock may run out before it even gets a chance to reach its design goal.

In conclusion, I don't think that suspension of work on the laser system is a major departure in the program. The departure quietly took place years ago and this announcement is simply an acknowledgement that NRAO is going to use the traditional approach, and the issue of structural health monitoring has been ignored.

I am sure others will disagree with my views--and they may be right--but this is how I see it.