Dear reviewers,

Please, check my answers on your comments.

Most of them have been accepted and incorporated into the revised version of this manuscript. However, having additional results some of them should be considered carefully. Please, read my answers and advice whether a further revision is pertinent, and, consider some alternative options for specific comments. I attached one extra file DOC1 with some plots and tables for your consideration.

I would please to hear your recommendations for further improvement of this manuscript.

> Reviewer #1: Abstract:

> - 2nd sentence: ".mean number of observations of the reference radio

> sources." it would be good to clarify what "observation" means (baseline-

> delay or delay or scan per telescope), and what the meaning of "reference"

> radio sources is

Accepted and revised

> - Last sentence:

> o "While the previous method indicated." which previous method?

Accepted and revised

> o ". vertical . 4-5 times larger that the horizontal." today, this is a

> very bad guess - this number seems to be taken from Ryan et al. 1993 as

> stated by MacMillan & Ma 1994; today the formal errors of single session

> results as well as WRMS of long time position series reveal factors like

> 2-3.

To be honest I have never seen any paper where the factor like 2-3 was rigorously revealed. The 'old' factor 4-5 is commonly used so far. I would please to revise this part of paper and add all the references, if they are available.

>

> Chapter 2:

> - Formula (5) appears to be wrong if derived from (4) and (6), the factors

> b and a should not be squared; this might have consequences also in the

> last part of chapter 4.2 and the conclusions the author draws from this.

It's a misprint, and I am sorry about that. Should be 'b' rather then 'b squared'. However, it does not cause any consequences afterwards, only in formula (16). Revised.

> - Last but one sentence: ".better visualization." has an unclear meaning.

>

Accepted and revised

> Chapter 3:

> - 2nd sentence: "This assumption is valid if all VLBI sites produce an

> equal number of observations." can only be a very rough and raw guess,

> many other factors (from equation system point of view and technical

> aspects) are at least as important.

Indeed "this assumption" referred in the sentence is outdated and not currently appropriate. That's why I have decided to develop more advanced estimator. However, I believe the number of group delays is a main factor to be incorporated with the baseline length (see more explanation to the comments of the reviewer #3 and Fig 1 and Fig 2 in the revised manuscript).

> - Last but one sentence: "A sum of these two slew rates." why the sum?

> Does this mean an antenna drives the 2 axis to the pointing in a row?

Accepted and revised

> - Formulas (8) and (9) are a strange representation of "baseline

> dispersion" because of two reasons

> o VLBI provides observations directly per baseline, the concept with the

> number of observations per station is improper,

> o A single session baseline residual can easily be computed from the

> baseline length directly: dl = l\_estimated - l\_apriori, together with a

> formal error using error propagation sl (which reflects, besides other

> properties, the number of observations for the baseline); The

> repeatability of a baseline length for several sessions shall be derived

> with wrms = sqrt( sum(dl'\*(1/sl^2)\*dl) / sum(1/sl^2) ).

>

Accepted and revised

> Chapter 4:

> - 2nd sentence after figure 2: ".repeatability." please state how it was

> computed, with the formulas (8) or (9)?

Not. It is referred on (7) in the text

> - Next sentence: how would it look like with the quadratic model? Maybe

> the noise-dots are related to one or two special stations with special

> properties only?

More new post-fit plots are attached to the DOC1 file. I am not sure which of them to be added to the paper.

> - Next section:

> o The concept to derive N from the observations of one of the station

> remains unclear - N is directly given by the observations of the baseline

> (N <= N1)!

Accepted and revised

> o The reference to fig 3 here is very critical, as the legend of fig 3

> is very vague - maybe better specify like "Fig 3 (upper left)"?

Accepted and will be revised after consideration of suggestion for the graph improvement

> o ".mean number N of observations of the reference radio sources." is as

> unclear here as in the abstract,

Accepted and revised

> - Last sentence before figure 4: Please specify the post fit RMS of the

> "old" and "new" approach to support your statement.

>

Unfortunately, there is no way to compare the RMS from the "old" and "new" approach straightforwardly. However, some alternative statistics can be calculated (see the DOC1 file attached). I would pleased to get advice on the best selection of the parameters and/or plots).

> Chapter 4.2:

> - First part of the formula (16) might be wrong according to (5). Please

> check. Interesting enough, the second part of (16) seems to be in

> accordance with the definition (14).

>

It's a misprint (I am sorry again). Should be 'b' rather then 'b squared'. Revised.

> Thus, it is unclear if the conclusions, the author draws in the rest of

> the article are disproportionate because of this possible error, e.g.:

> \* ". with the traditional approach the vertical uncertainty exceeds the

> horizontal on by a factor of five." These values still are very

> unrealistic (today), it should be a factor 2-3.

Again, to be honest I have never seen any paper where the factor like 2-3 was rigorously revealed. The 'old' factor 4-5 is commonly used so far. I would please to revise this part of paper and add all the references, if they are available.

> \* ".new model." The factor here (as given in table 3) is maybe 1.3, which

> is also unrealistic - from the physical meaning of the observations and

> the VLBI observation equation's point of view, this is not possible.

>

The only natural limit that sigma(v) is equal or greater than sigma(h).

So, any factor more than 1 is permitted. I do not understand why the factor 1.3 is not possible.

> Figures:

> - Figure 3: these pictures should either be separated from each others

> (1st one is used early in the text, the 2nd and 3rd late), or the legend

> must be more detailed (e.g. using hints like "upper left", "lower left"

> and "right". Additionally, the asymmetric design is not nice (Figure 3

> also).

>

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Accepted and will be revised after consideration of suggestion for the graph improvement.

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> Reviewer #3: I have strong reservations about recommending this paper for

> publication. However, I'm prepared to hear a rebuttal from the author.

>

> There are many detailed comments I could make relating to communication in

> general, labelling of figures, etc, but I will restrict myself to a few

> conceptual difficulties - and proceed with detailed comments at a later

> time if the conceptual difficulties are answered adequately by the author.

>

> At the root of the problem is the transition from equation (2) to equation

> (9), where a factor of 1/N is added to the right hand side. This is at

> the crux of the paper and yet, to me, it is argued very poorly, and

> probably incorrect. This has important implications for how the

> repeatability vs baseline length plots are interpretted with respect to

> the ratio of sigma(h) to sigma(v), so it is important to get it right.

>

> To me, the basic premise of the paper is well taken. The standard

> treatment of the problem assumes that sigma(h) and sigma(v) are identical

> at each station. This is, of course, a simplification, and under

> circumstances where this inequity leads to a baseline length dependence,

> will bias the interpreted ratio of sigma(h) to sigma(v). Furthermore, the

> author's argument that sigma(h) and sigma(v) depend on 1/sqrt(N(i)) (start

> of section 3) has some truth to it, as does the argument that variations

> of N(i) may lead to some baseline length dependence. But beyond this, I

> have problems with the treatment.

>

> Before dealing further with the transition from equation (2) to equation

> (9), I would like to comment on figs 1 and 2. In my opinion, fig.2 should

> be plotted R vs L instead of R\*\*2 vs L\*\*2.

> Otherwise, it is very

> difficult to compare it to fig.1. Furthermore, in the usual practice of

> VLBI, the standard plots are R vs L, so it is confusing to me why fig.2

> was not plotted in the usual way. In addition, R vs L has less of a

> tendency to emphasize outliers compared to R\*\*2 vs L\*\*2, and hence it will

> better pick out the general trends. I would be very interested to see

> fig.2 plotted as R vs L, as I think the motivation for the paper would

> also be much less compelling since the fit would be improved. Fig.4

> should also be ploted R vs L instead of R\*\*2 vs L\*\*2. Certainly, changing

> figs. 2 and 4 to R vs L would greatly enhance the ability to intercompare

> the figs 1, 2, and 4. It would also be good to see a more numberical

> measure of the improvement from fig. 1 to 2 and 4 such

> as the weighted rms of residuals, although this would not include the

> impact of systematics, i.e. plots of the residuals would still be useful.

> Further, I would suggest the author consider weighting the repeatabilities

> (based on confidence) when doing the least squares adjustments.

>

In fact, all plots were plotted R\*\*2 vs L (rather than R\*\*2 vs L\*\*2).

In general, there is no way to present the post-fit residuals in a form R versus L, saving their statistical sense – just because I have to get a square root from negative values. I have prepared more plots and alternative statistics in the attached file DOC1. I would please to catch some advice what plot and statistics are pertinent for the revised version of this paper.

> Next, back to the statement near the start of section 3, "In general, the

> errors sigma(h) and sigma(v) are different for all VLBI sites and are

> proportional to 1/sqrt(N(i))." I agree that they are generally different

> and there should be some dependence on N(i), but its not clear to me that

> it is properly 1/sqrt(N(i)). I would like to see a theoretical

> justification for that exact form. In addition, there are many other

> factors that impact sigma(h) and sigma(v), some of which might be much

> larger than the 1/sqrt(N(i)) dependance, e.g. typical atmosphere

> conditions at a site, clock stability, antenna diameter, Tsys, stability

> of the electronics, thermal stability of the antenna, barometer stability,

> parameter correlations, etc, etc. If any of these dominate the N(i)

> dependence, then the N(i) dependence won't be apparent. Some of these may

> be baseline dependent it, for example, they impact the southern hemisphere

> sites more than those in the north. I think it is up to the

> author to demonstrate that the primary dependence is on N(i), as opposed

> to the other factors. In fact, it is my intuitive feeling that a large

> part of the additional baseline length dependence is related to size of

> the antenna and quality of the receiving system. The highest quality

> antennas are those of the VLBA. Since the VLBA is a continental array,

> its baselines tend to be comparatively short, improving the short baseline

> performance. In contrast, TIGO is small and has had trouble with its

> electronics. It is in the southern hemisphere, so it's baselines are

> typically long degrading the long baseline performance. I think it is

> likely that this causal factor outweighs the 1/sqrt(N(i)) factor.

>

Each daily estimate of the vertical (v) and horizontal (h) component can be considered as a random value with formal error sigma(v) and sigma(h), respectively. These both formal errors are estimated from the N(i) group delays for each i-th session. From the theory of statistics we learnt the formal errors are expected to be proportional to 1/sqrt(N(i)). I am not quite sure what level of a theoretical justification is required for this paper.

As to the practical point of view, I considered the plots sigma(v) and sigma(h) versus the N(i) for different VLBI sites. All of them shown very apparent dependence on the N(i), including the VLBA and TIGOCONC antenna. The approximation

Sigma = K\*N(i)\*\*(alfa)

looks suitable. I believe the most of technical factors (like thermal stability, troposphere, etc) down to the coeffient K, individual for each site, whereas other factors (slewing rate, sensitivity, etc) affect the N(i) dependence directly. At this stage only N(i) and slewing rate are consider as the most important parameters suitable for quantification in the basic formulae.

Of course, for the dependence N(i)\*\*(alfa) the factor alfa can vary for different sites and their position components. However, the mean factor is close to the alfa =-1/2, therefore I drew conclusion that it would be a good assumption for this paper. At least, it is much better then the mean factor alfa = 0, as it follows from the «old» assumption.

More details (plots and tables) can be found in the revised manuscript.

> Next (through equ's 8 and 9, some discussion of the plots, and other what

> seem to me imprecise arguments) a connection is made between using N, the

> baseline-based number of observations instead of N(i), the station-based

> number of observations. This might be valid if the data were analyzed

> directly on a baseline-by-baseline basis. More precisely, the dependence

> would be on the number of degrees of freedom for each baseline

> (disregarding correlations, etc) as opposed to N directly. However, this

> is not, to my understanding, the way the data is actually processed. All

> baselines are processed simultaneously, and to gain advantage of closure

> properties, station positions and clocks are determined instead of those

> for baselines. Baseline lengths are then determined by applying the

> pythagorean relation to the differenced coordinates. Under this mode of

> analysis, N probably far underestimates the effective number of

> observations on the longer baselines. I would need to see a

> much more rigorous analysis before I would believe that the dependence on

> N is correct.

>

Accepted and revised

> Finally, to the discussion of slew rate. Equn 12 was developed

> theoretically, but I can't see where it was applied to the data.

As mentioned in the text the Eqn (12) can not be applied for the data immediately. Practically, it is used to obtain the Eqns (17) and (18) those can be used for simulation, etc. Everything what can be done - to use the coefficients from Table 2 and approximate the data using the Eqn (12). I have prepared plot of approximation of (12) and residuals in DOC1 file (Fig 3). However, a statistic can not be properly calculated. I am not sure that it is essential to add these plots to this paper. However, they can be added.

> Before

> making any statements of the validity of equn 12 and applying it to the

> rest of the paper, I think it should be verified with real data to see

> that it results in a real reduction of residuals. Intuitively, I think

> looking at the real data may lead to a surprise, but am not sure. In

> addition, I have some reservations about how equn 12 is applied to

> determining sigma(h) and sigma(v). I think all observations binned near

> N=25 at all baseline lengths should be used to determine a value of A and

> B and compare them with the overall values and values binned at 50, 75,

> 100 and 200. This would help assess the reliability of the A and B

> values. I am little worried by the way N is used as a proxy for L.

>

Fig 3 demonstrates that the dependence N(L) is valid. So, the parameter N can be used to proxy for L. Unfortunately, A and B can’t be estimated directly from Eqn (12) so more detailed fitting with binned values is not possible for this case.

Sincerely yours

Oleg Titov