Comments about GISMO Run #2 at Pico Veleta

Feedback on Johannes Staguhn et al. "Report on GISMO Run #2"

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August/September 2009

Introduction

This Document compiles comments from IRAM contributors to Johannes et al report on GISMO Run #2 at the IRAM 30m telescope in October 2008. For easy reading it is organized in paragraphs identical to Johannes report and when it is appropriate the comments begin with a quotation. To avoid useless repetitions the comments referring to "GISMO sensitivity at Pico Veleta" (the feedback to Dominic Benford's "Atmospheric emission at Pico Veleta") are not discussed again here.

Comments on "Summary"

Values of the predicted NEFDs → See "GISMO sensitivity at Pico Veleta".

"Resulting pixel efficiency of about 50%" → In "Atmospheric emission at Pico Veleta" the system efficiency without neutral density filter (ND) is 35%, so what explains the difference between these two values?

"1 mJy in one hour" → NEFD = F·√t = 60 mJy·√s = bad weather map NEFD with ND according to Dominic's calculations; this is compatible with my calculation but there are problems with the method (see "GISMO sensitivity at Pico Veleta").

"With a pixel yield of 90% we can expect less than a quarter of the integration time [compared to run #2]" → Taking the ratio of run#2 vs expected good pixels ratio, and the gain in observing efficiency, I think the gain in observing time should be less than a third but more than a quarter: (8×16)·(1-1/4-0.2[5]·3/4) / (0.9·8×16) / 2.4 = 28[26]%.

"Under typical conditions (20% line of sight opacity)" → According to the ATM model an <u>opacity τ = 0.2 @ 150 GHz seems mediocre since it is obtained for example with 5 mm of water vapor and an airmass of 1.3 (50 degrees elevation)</u> (see "GISMO sensitivity at Pico Veleta"), <u>so when speaking about "weather conditions typical for the 30m site", which water vapor values and telescope elevation did you take?</u>

"Expect a good pixel map sensitivity of 22 mJy·√s" → My calculations are compatible with this value under excellent weather conditions (see "GISMO sensitivity at Pico Veleta"), but again we should agree first on coefficients and formulas used in the calculation.

Comments on "Instrument Configuration for Run #2"

"Designing neutral density filter: [] polarizing grid [or] two cold neutral density filters that can be moved" → What is the status of these studies now? Aren't they complementary? Indeed I guess a polarizing grid could be interesting to study polarization of astronomical sources, whereas cold movable ND sounds best for adapting GISMO dynamic to the weather conditions. Naive question: wouldn't it be even better to use a more powerful fridge without ND? If the use of ND is better, what are the pros and cons to use one filter with two conditions (in and out) or use 2 filters?

"Pixel yield turned out to be about 50%" → This means 64 pixels, but in the introduction it is said 20% to 25% of 3 quadrants were bad, which makes 72 to 77 pixels; why is there about 10 pixels difference?

"Increased noise [] was likely the result of an address line short" → After fixing this problem, what is the result of the quantification of the detector stability and saturation power you were conducting when you sent your report?

Comments on "Mitigation of discrete frequency spikes […]"

"The physical units of pA/√Hz shown in the [spectrum] figure are derived from [] the statistical analysis of flux quantum jumps" → Just curious: can you tell us more about this method?

Comments on "Instrument setup issues"

"[] The optical bench would often oscillate […]" → This means the bench pistons were not adapted to support GISMO weight; I am afraid this is a bad sign for a dual installation of MAMBO + GISMO on the same bench. We have to find a solution to this problem!

It should be noted clearly: At its present position in the Nasmyth cabin, the GISMO cryostat blocks the heterodyne receivers at elevations below about 30deg. This is a major reason to seek an alternative position.

Comments on "Observing modes"

"Lissajou scans produce faster crossing times, reduce the effect of atmospheric and system drift contribution" → OK with that, but like any scan mode at the 30m telescope, the Lissajou patterns are approximations built with pieces of straight lines. For run #2 the coordinate targets were calculated at a frequency of 8 Hz, with 16 points (128 Hz rate) of linear interpolation in between to control the natural drifts (e.g. wind). So what is the effect on the data of the short but possibly strong accelerations due to the changes of target coordinates, and is there a correction for this effect?

Addition to the report: It should be noted that GISMO observations using the newly implemented Lissajous curves were partly conducted outside the agreed-upon limits, i.e. the fastest mode at high elevation. For the next run, we have to ensure in the system that this is impossible.

Addition to the report: Besides the advantages you listed, can you clarify the pertinence of the Lissajous scan concerning the gain of time compared to other modes to make an image at a given noise r.m.s. Indeed, R.Zylka did the contour plot below of the distribution of the integration time for the observation of 3C111 with Lissajous observing mode. It was created using the true coordinates (distribution of the integration time on the sky), centered on the image, without knowing what was the orientation of GISMO on sky and what were the actual dimensions of the pixels (neither the number of usable pixels). The details in this plot might change but the major question should stay: isn't it a big loss to spent so much time in the edges?

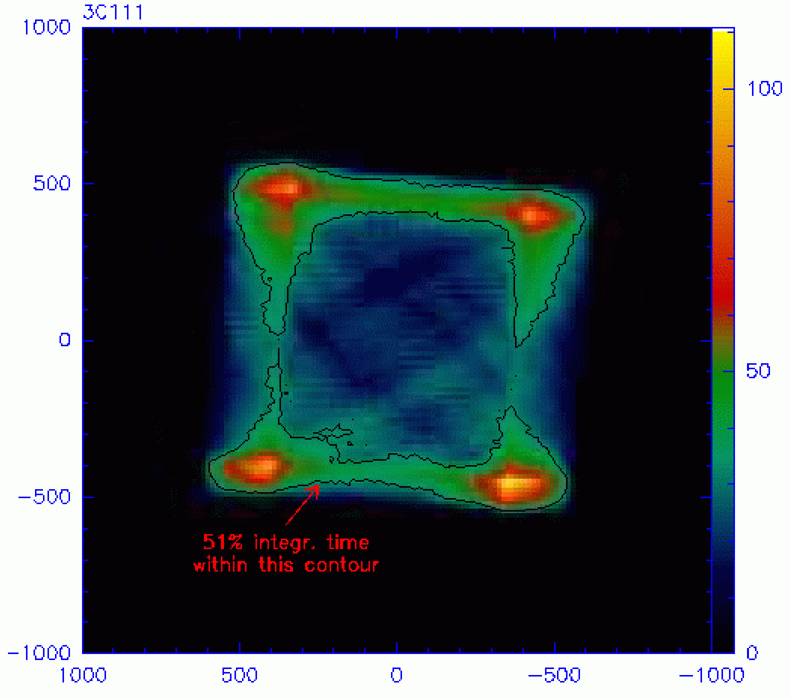

Figure 1: Distribution of the integration time on the source 3C111.

The size of a pixel in the image may be different from the actual size of a GISMO pixel, but this plot is qualitatively representative of the inhomogeneity of the integration time per pixel for GISMO using the Lissajou scan pattern, hence the question of a possible variation of the modulation efficiency γ depending on the position on the map. Are the Lissajou NEFDs presented in the reports homogeneous on the observed maps?

Comments on "Results from observations"

"We have adequate software tools to process observations in near-real time" → Assuming GISMO stays at the telescope, we'll need a user manual for this tools, and we'll need to discuss conditions of use (for example will the source be open? will it be valuable to integrate it into IRAM's own data analysis software? and so on).

"A Gaussian fit yield a beam size of 15.8"x16.2"" → Why isn't it square?

"Figure 7" → We don't see the source J1148, the claim of a detection, even at low significance, is therefore doubtful.

There are a number of nice figures showing GISMO results. However, a consistent set of header information would be very helpful for each of these data sets. Could you please add information e.g. on the integration time, observing mode, peak flux, rms flux to all: Fig. 2 J1849 (labels are very small to read), Fig.3 Cyg-A, Fig.4 Cas-A, Fig.5 IRDC30, Fig.6 Arp220, Fig.7 J1148, Fig.13 OriA, Fig.14 3C454.

Comments on “Instrument noise performance”

“We significantly improved our modeling of the optical performances of GISMO by incorporating measured filter transmission functions in our calculations” → Could you communicate these functions and your modeling? Does this explain the big difference between Dominic and Samuel about AΩ (3.93 vs 2.5 $mm^2 \cdot sr$) and η_{sys} (35% vs 52%)?

We are not sure that we understand your calibration scheme. The bolometers we know all use skydips to determine the atmospheric opacity. How are sky opacities to be determined with GISMO? During the last run, you also obtained skydips with GISMO, but we can't find any discussion of the results and conclusions. If GISMO has insufficient dynamic range to handle skydips, this should be said so clearly. Note that the use of quasars, whose 2mm fluxes were determined by heterodyne receivers with EMIR, adds uncertainty to the GISMO data. Quoting Johannes email from 12.1.09: "Wir werden unsere skydip Messungen gut unter die Lupe nehmen und hoffentlich zukuenftig haeufig implementieren...". With skydips, we would have the opacity in the direction of the source, and would not depend on knowledge of the current 2mm fluxes of variable quasars.

“IRAM advised us not to rely on their tau meter to determine the atmospheric opacity” → Yes, is not meant to be used for calibration purposes. It anyway points towards a fixed azimuth direction only. The plot below shows the weather conditions during the last GISMO run, taken by the taumeter. GISMO had been on-sky between 21.10. and 28.10.2008. Weather conditions were indeed poor. This is especially clear when looking at the correlation coefficients of the skydip fits.

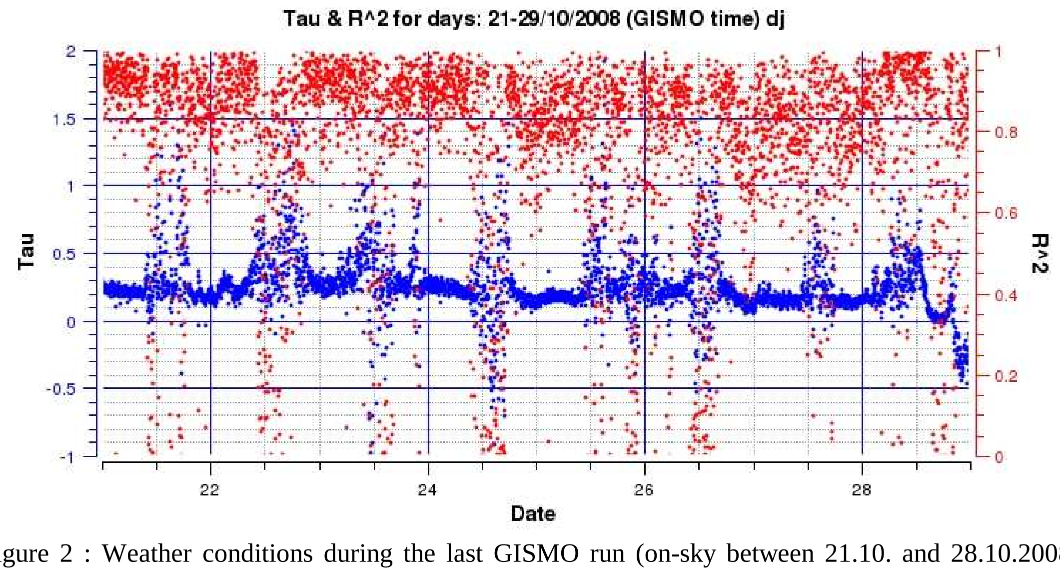

Figure 2 : Weather conditions during the last GISMO run (on-sky between 21.10. and 28.10.2008), taken by the taumeter. The blue dots represent the opacity τ at 225Ghz measured at zenith by a Schottky receiver (Tau=0.2 @ 225GHz is close to 7mm of water vapor, see formula on the weather pages of the 30m webpage), and the red dots represent the correlation coefficient R^2.

“The calibration method will involve improved version of the internal calibration system with the addition of a total power calibration measurement” → This may be indeed useful for performances diagnostics. Is this method fully implemented now?

In the plots including a variation of the atmospheric emissivity (Figure 8, 9, 10, and 12), the airmass is varied between 1 and 2 → An airmass of 2, means 30deg elevation, excluding important sources like the Galactic Center. For this an airmass variation of up to 3 (20deg Elevation) would need to be taken into account.

To improve the analysis, the plots of the atmospheric emissivity against frequency could be convolved with the 22GHz bandwidth of GISMO. This would allow obtaining a more realistic view of the conditions for GISMO.

Values of transmission, sky NEP, and so on, also presented with more details in Dominic's document → see "GISMO sensitivity at Pico Veleta".

"Detector NEP is $4 \cdot 10^{-17}$ W/√Hz" → How was it measured?

Comments on "Instrument sensitivity derived from the data time stream"

"A point source sensitivity of 30mJy/rt(s) in time stream data corresponds to a map point source sensitivity of 42mJy/rt(Hz) for our 0.9 λ/D sampled pixels" → Typo: one should read "30mJy·rt(s)". To reconstruct your result one has to do the following calculation: $30 \cdot (\gamma_L/\gamma_s)/\sqrt{2} = 30 \cdot (4.07/2.06)/\sqrt{2} = 42$, where I guess (based on Dominic's document) √2 represent the conversion from bandwidth to time, but I think the correct conversion is $X/\sqrt{Hz} = \sqrt{2} \cdot X \cdot \sqrt{s}$ (see "GISMO sensitivity at Pico Veleta"), so the result of your calculation should be 84 mJy/√Hz! Furthermore, what is the default element size you use for your NEFD? Indeed if you indicate the pixel size in one case, one can suppose you may use a different size in the other case; according to Dominic formula you use the beam efficiency η_{MB} in the calculation of the NEFD, which implies it is calculated for the main beam of a diffraction pattern hence a size $\theta = 2\ \lambda/D$. When the bunching (radiometric) noise dominates, the NEP is proportional to θ^2, so that the NEFD is proportional to θ^2/η_θ, where η_θ is the relative power in a square of size θ centered on the beam. Hence 30mJy·√s in the main beam ⇔ $30 \cdot (\gamma_L/\gamma_s) \cdot \sqrt{2} \cdot (\theta_G^2/\eta_G)/(\theta_{MB}^2/\eta_{MB}) = [30 \cdot (4.07/2.06) \cdot \sqrt{2}] \cdot [(0.9^2/0.4)/(2^2/0.6)] = 83.8 \cdot 0.3 = 25$ mJy/√Hz for a 0.9 λ/D pixel. Three different interpretations are possible: (1) you measured 42 mJy/√Hz in a 0.9 λ/D pixel, which means you have more than 30 mJy·√s and you are less sensitive than you think, (2) you actually measured the 30 mJy·√s so the sensitivity of your pixel is better than you think, (3) my calculation is wrong and I'm eager to learn the correct one.

"This selection results in an the apparent […]" → Typo, choose either "an" or "the".

"The centroid of the histogram is at 35mJy/sqrt(s)" → Does that mean that the 30mJy·√s quoted before correspond to the best pixel whereas the 35mJy·√s is the mean over the best 32 pixels? If not, can you explain better?

Remark: the source is 12.5 Jy, this makes 0.4 pW on the pixel, which is small compared to the 17 pW from the background. So the additional noise due to the source itself is negligible.

"Figure 12 [] NEFD [] degraded due to the fact that the equivalent number of pixels illuminated by the source in the time stream is 2.06, whereas it is 4.07 pixels in our maps" → The lower the NEFD the better the sensitivity, so sounds strange to use the word "degraded" when the sensitivity is better. In "GISMO sensitivity at Pico Veleta" the number of equivalent pixel illuminated is used as factor of time loss per pixel, do you agree that both interpretations of this quantity are equivalent?

"Figure 13 [] shown in Figure 10" → Typo, I think it's 11, not 10.

Comments on "Data reduction with Crush-2"

It would have been interesting to see images of the same objects reduced with the procedures you used in previous figures and with Crush-2. And maybe a word about the differences between the 2 methods in terms of rms obtained.

Subsidiary question about data processing: GISMO pixels sizes are 0.9λ/D but one need 0.5λ/D to sample the telescopes resolution at Half Power Beam Width, so is there a plan to use several time samples and deconvolution to reach the telescope resolution (and in that case how will be treated the additional noise introduced) or is it worthless?

Comments on "Magnetic field"

"95.5 counts per microTesla […] 0.3 counts per arcminute of azimuthal slew" → The relation between the 2 numbers seems compatible with the value of the earth magnetic field in this region of Spain, but just by curiosity I would like to know how you do your calculations.

Comments on "Internal calibration source"

"LED with fiber optics [] misalignment […] difficulty controlling the shutter" → Are these elements working correctly now? This internal calibration seems absolutely valuable. By the way, how do you know precisely the power detected from the LED?

Comments on "Major Post Observing Efforts"

"Detector package fixes" → Good
"Design of alternative density filters" → Is the polarizing grid interesting also for astronomical purposes?
"New Position for GISMO in the receiver cabin" → The "Dragone" proposition from Cathy looks interesting from an optical point of view, but with this proposition both MAMBO and GISMO are on the same anti-vib table, which was not designed for such a weight. I (Samuel) had to put the work on this subject in standby during the summer, but I'll resume it and give you some input in the following weeks.
"Software tool improvements […] Lissajous frequency filtering" → How do you deal with the acceleration bumps at the segments edges due to the approximation of the Lissajous curves done by the 30m telescope control system?

Comments on "Conclusion"

"The results from this observing run […] GISMO achieved a sensitivity clodeley travce" → Typo: "close to" seems better.

“Noise integrates down radiometrically over thousands of observing seconds” → just to make sure we agree on the signification of this sentence: this means r.m.s ~ $T_{noise}/\sqrt{(\Delta\nu \cdot t_{int})}$, and this is equivalent to say that the photons bunching dominates the noise with a non gaussian space coherence ($A\Omega/\lambda^2$ close to unity) and a gaussian time coherence ($\Delta\nu \cdot t_{int}$ large enough), which is expected from theoretical calculations.

“The results are consistent with our models” → See “GISMO sensitivity at Pico Veleta”.

<u>The integration times of all images shown in the report are rather short. Does that mean that longer integrations do not show the expected reduction of noise?</u>

Conclusion

The comments, questions and remarks above may look severe, but their goals are to make sure we understand how GISMO behaved at the 30m telescope, and to help optimizing its use for the future. So we are globally pleased with the improvements made between the two runs, and we think that until a new multicolor instrument is ready it could be valuable to have GISMO as a 2mm bolometer array in addition to MAMBO 2. However this decision can’t be taken before the five most important remarks listed below are addressed.

1. The skydips are ignored in Johannes document, although they are known to be a standard reliable method to characterize the atmospheric opacity and deduce the instrument sensitivity. We insist that skydips must be reduced and present in the report, showing clearly the atmospheric correction they provide (as the GISMO team has experienced, the calibration of data on quasars is not reliable enough in practice). If the skydips can’t be reduced because GISMO can’t deal with the variation of atmospheric emission for different elevations it must be said clearly (the instrument should be able to observe the Galactic Center at 20deg, and also Cygnus sources rising high in Elevation up to 80deg).
2. We remind that GISMO was meant to be a summer instrument, hence with a big dynamic range allowing handling big variations of atmospheric opacities. It’s a shame that a neutral density filter had to be used with the conditions encountered during run #2. This is not only a pity that there’s about 20% instantaneous loss of sensitivity when reaching the limit where the background load makes it necessary to use the neutral density filter, but this raises questions about the capacities of the instrument to perform correct background rejection (see point 1 about skydips). Is there no way to avoid using a “discontinuous” solution like the neutral density filter? For example a more powerful fridge could handle a bigger dynamic of background power, couldn’t it?
3. How do we perform a dual installation of MAMBO 2 and GISMO without affecting the quality of the observations? Catherine Marx proposed an elegant optical design with a dual installation on the anti-vibration table. However there are still some problems of dimensions and room available to be fixed before endorsing this design. Additionally a serious issue concerns the capacity the anti-vibration table to work efficiently with more than twice the weight it was designed to support. It is possible that appropriate shock absorbers could do the job, but this must be studied before taking a decision. IRAM is working on it.

4. Several advantages of the Lissajou scan mode were listed. However we would appreciate clarifications answering two possible critics. First, the repartition of the integration time on maps is very inhomogeneous (see R.Zylka plot), hence looking detrimental. Second, it would be interesting to know whether variations of accelerations create excess noise in GISMO data, whether this can be identified and corrected, and whether this doesn't ruin the advantages of the Lissajou scans compared to other standard OTF modes. Indeed as said above, for the Lissajou scans used in run #2 were built with pieces of straight lines refreshed at a rate of 8Hz, and including 16 interpolation points (128Hz rate).
5. We would appreciate more details about the determinations of the filters transmission, the instrument throughput ($A\Omega$), optical efficiency (η_{sys}), the instrument intrinsic NEP, and the calculation of the modulations efficiencies (γ).