From root Mon Apr 6 17:00:53 1992
From: SWAI@db1.cc.rochester.edu
To: abridle@polaris.cv.nrao.edu
Subject: Re: How's it going?
Date: Mon, 6 Apr 1992 16:55 EST

We got the SUN finally; I have yet to install AIPS on it. I have been trying to estimate the flux from FeIVX in the lobes. The FeIVX line is at 530.3 nm; numbers for that transition are HARD to find. I'm still chasing numbers for the collision strength and relative abundence of FeIVX to Fe at 10^6 deg. Just today, I got some refferences form a solar astronomer at NOAO (or something like that). We'll see if they pan out.

I presented the Perly et al. paper on the spectral aging of the lobes of Cygnus A and learned a lot. I guess there is no chance we could preform such an anlysis on 3C353; it looked like they had LOTS of observations. I think they had 15 different frequencies for thier 4.5 arc sec resolution immages. With the rich structure exibited in 3C 353 we might find that the\ spectral aging contours are not as uniform. It would be interesting to see if the break frequency of the filiments is different from that of the lobes or if the injection indices match.

It's ironic that you sent me a message when you did. I sent a message to you Friday March 27. I must have made a mistake in the address because today I found lots of "undelivered mail" messages from mailer demons. It turns out that I was in Charlottesville Tues->Fri of last week. I wanted to drop by to chat and do some free copying of thesis related materials. Since I didn't get a reply from you, I assumed you were out of town, not that I had made a mistake with the email address. (I have a script set up so that I type your name and it automatically fills in your address - this has worked just fine in the past) I intended to try to telephone you while I was in C'ville but I was swamped with the closing on our house. In addition to the closing, I gutted the basement appartment clear down to the stud walls - this included removing the ceeling. Sorry we missed each other.

Melanie was acceptd by UVA and decided to transfer. It will certaintly make me happier to have her in Charlottesville than in Rochester for the summer and fall semester (the summer and fall semeter are when she would have stayed in Rochester to finish class work).

I start work on a presentation soon for my astrophysical MHD class soon. Naturally, it will be on MHD models for jets. Jets have been completely neglected in our course. Currently I know how to write down the induction equation and a few other equations and solve them for a few, simple, contrived systems. Hopefully I'll learn a little about how to apply them to jets. Got any favorite papers to suggest?

Grading and homework continue as usual.

Mark

NATIONAL RADIO ASTRONOMY OBSERVATORY

520 EDGEMONT ROAD CHARLOTTESVILLE, VIRGINIA 22903-2475
TELEPHONE 804 296-0211 TWX 910 997-0174 FAX 804 296-0278

June 22, 1992

Dr. Dan Watson
Dept. of Physics and Astronomy
University of Rochester
Rochester, NY 14627-0011

Dear Dan:

The NRAO has offered one of your graduate students, Mark Swain, an appointment as a Junior Research Associate. Mark will work on his Ph.D. thesis research with Dr. Alan Bridle on a comprehensive investigation of the physics of nonthermal radio jets.

Junior Research Associates are graduate students in residence at the NRAO who are conducting their thesis research in collaboration with, and under the supervision of, one of the NRAO staff scientists. The student has access to all the facilities of the NRAO. He or she is paid a stipend of $1250/month by the NRAO, but the student has no assigned task other than the (considerable) effort needed to complete his or her thesis research. An appointment as a Junior Research Associate may have a duration of no longer than 24 months.

The goal of the Junior Research Associate program is to assist students in their pursuit of professional training in radio astronomy. However, at all times we recognize that the student's first obligation is to his or her university. So this is not forgotten neither by the student nor by the university, we ask that the university provide the student with a continuing stipend of at least $100/month in addition to the NRAO stipend and that the university appoint a faculty thesis advisor whose responsibility is to verify that the thesis research program is of a quality suitable for the degree and that all university requirements are met. In Mark's case, I understand that either you or Hugh Van Horn will serve as faculty advisor.

We look forward to Mark's participation in the scientific life at the NRAO and hope that his research experience is productive and rewarding.

Sincerely,

Robert L. Brown

c: A. Bridle
H. Van Horn

From mswain Thu Aug 12 14:47:16 1993
From: mswain (Mark Swain)
To: abridle
Subject: Socorro report
Date: Thu, 12 Aug 1993 14:47:16 –0400

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Alan,
   I thought I would give you a brief summary of this
trip while it is still fresh in my mind.  On the hole,
it has been a very useflul trip due to the advice of
Mark Holdaway and Frazer Owen.
   The computing situation has been a continued frustration.
It is my impression that mx cleans run consideribly slower
on this machine (Aztec) than on rhesus.  Further, opon
finishing a large mx clean, the machine must be rebooted.
I have talked extensively with the personel here and sent
mail to Pat Murphy but no solutions have materialized.
According to Gustave, this problem has been seen
by other people; the source of the problems is a
subject of much contention presently.  The basic
problem is that boid processes seem to get created
under certain quasi-repeatable conditions.  These  processes
start consuming a significant fraction of the cup as measrued
by monitor -top.  However, them impzct of the biod processes
on the excution speed of a task seems to be out of proportion
to how much of the cup they take up.  Thus if biod
processes take up %50 of the cpu, a given task might take
4 or 5 times as long.  This problem is localized to the
IBM machines.
    My stratagy for the image reduction was to put the
arrays together as fast as possible.  I used a B model
(after carefully checking for evidence of fringing) to
cross calibrate the C uvdata set.  As soon as possible,
I cross calibrated the D array daya on the BC model.
The combine BCD data set has been selfcalled onec and imaged
twice (for the east  pointing).  Problems in the clean image
are apparent; problems in the vtess image are sevear.  The clean
image has several problem fringes localized to specific regions of
constant surface brightness - the classic clean instability.
The vtess image is dominated by a high frequency fringe whcih
covers the entire image.  Back transforming the vtess image
revealed a spike at the uv distance corresponding to the
spatical frequency fo the image-wide fringe.  I removed
the uv data associated with the bright feature in the uv
domain and reimaged the data set.  Again, an almost indentical
fringe was present; back transforming showed a new peak in
the uv plane.  Back transforming the dirty map shows a family of
peaks in the same area.  For some reason, vtess makes a map
suffering the affects of on ly one of this family at a time.
However, kill off one of the peaks and vtess suffers from
another.  I am currently experimenting with restricting the
uv range to exclude this family of  peaks  in the transform
of a dirty image.  My intent is not to proceed with further
calibration untill I can get decent looking vtess images.
   I suspect the above problem is identical to the fringe
problem we were having with the C band A array image; at least
the problems look very similar in the image domain.  By the way,
in neither the C band A array fring case nor in the X band
BCD array fringe case do the uvdata responsible for the fringe
show up in a radially averaged uvplot.
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Thesis Progress Report

Mark Swain
August, 1993

I am joining the AAS and I intend to present a poster at the January meeting in Washington. To facilitate this, I need you to sign the form inclosed form in the places indicated by post-it notes (front and back) and mail the form in the attached, pre-addressed and stamped envelope. The deadline for poster abstracts is Oct. 1 so I need to get the application in as soon as possible.

Progress on making a C band high resolution image continues to be stymied due to artifacts in the A array data. The primary artifact is a .4 arc second fringe present in the entire image at about 4 times the thermal noise. Also present are relatively deep negatives North and South of the core which are part of the side lobe structure. Since the side lobe problems are localized to a very small area around the core, we have decided to "live with them"; the draw back is that we will not be able to say anything about a core jet but that is not central to this thesis. I removed a similar fringe from the X band BCD data set which I partially reduced during a trip to Socorro during the first two weeks of August. The trick in that case was to back transform mem images of 353 which revealed the source of the fringe in the uv domain. A similar look at the dirty data revealed a whole family of such points. Perhaps there is an instability in the vtess algorithm which allows 1 high amplitude point in these families to "leak" through. It could also be that there is some problems with the calibration on the long East-West baselines in the data. What ever the case, a quick solution turned out to be restricting the uv range so as not to include the family of high amplitude points. This cause us to lose a very small amount of resolution (beam FWHM went from .87 to .92 arcsec); not a big deal. Naturally the expectation is to try this technique to the C band A array data ASAP.

Progress on making the C band BCD image at 1.4 arc second resolution, for use in conjunction with L and U band image for a scaled array analysis, is currently held up by fringes which appear in the vtess images but not the clean images. This means we are not dealing with the classic clean instability. I have made a "diagnostic" C to L band spectral index map and a percent polarization map at L band. The diagnoses is that total intensity maps for C band need more work, total intensity maps for L band are in good shape, and polarized intensity maps in both bands are questionable. Getting science grade C and L band images at 1.4 arc seconds and getting a high quality, high resolution C band image are my top priorities.

The L band percent polarization map had some regions of rather high polarization, which were as high as 50 to 60 percent in some cases. Since the source is mapped here at only 1.4 arc second resolution, it is likely that there is some beam depolarization taking place; a percent polarization map at C band should provide a truer measure of the percentage polarization and the peak values may well be higher.

Progress Report
February 4, 1994
Mark Swain

The poster presentation at the AAS meeting generated quite a lot of interest. The high resolution (0.4") C band image was a crowd pleaser. I also presented interesting findings about the jet, counter jet, and filaments as well as outlining what question we want to answer with multi-frequency data set. On the whole, I think the presentation went quite well.

We have found several interesting things about the jet. First, the jet seems to have a constant, but low, expansion rate out to about 60% of its length where it then begins contracting and continues to contract all the way into the hot-spot. The contraction rate is the same as the expansion rate. A reasonable interpretation is that the jet is not a freely expanding jet its entire length and that the contraction represents some sort of interaction with the surrounding medium.

We also found an excess of emission surrounding the jet. When slices are taken transverse to the jet symmetry axis and added, this excess appears as a relatively broad gaussian on which the jet sits. The emission excess is found even in regions where the jet is not detected. Naturally the first interpretation which springs to mind is that the emission excess represents some sort of cocoon built by the jet. This attractive idea is inconsistent with our failure find *no comparable emission excess feature associated with the counter jet.* Interestingly, the counter jet has the same expansion rate as the jet but at the same angular distance from the core where the jet contracts, we loose the ability to clearly identify counter jet features. There are faint features further out in the counter-jetted lobe which we identify as candidate counter-jet features. The FWHM of gaussians fitted to these candidate counter-jet features give a expansion rate identical to that of the inner counter-jet.

The filaments show evidence of pairing and a range of transverse scales. There are bright, poorly resolved (at 0.4" FWHM resolution) features in the brightest filaments. The emissivity enhancement in these features, assuming a spherical lobe, is about 25 times the lobe emissivity. Filaments near the edge of the lobe have an emissivity enhancement of about 2 time using the same assumptions. The question of what constitutes a filament is still open. Specifically, do two bright regions which are both much longer than their width and which appear to be paired constitute one edge brightened filament or two paired bright filaments? Hopefully the polarization and spectral data will help answer this question.

My intent was to display percentage polarization maps for C band at the AAS meeting. This did not happen because the maps contained non-physical values (greater than unity) even after very aggressive clipping. The problem is that our total intensity image has "pits" near the edge of the source. When the polarized intensity image (Sqrt[Qimage^2 + Uimage^2]) is devided by the total intensity image, the pits in it are low enough to produce values greater than unity. Because the "pits" are very close to the edge of the source, their effect actually extends into the source some. Better calibration has helped but not yet solved the problem.

I am also calibrating the X band data and the total intensity is looking quite good. I have done some imaging of the L band data but I am currently in a highly CPU limited state; I am running almost continuously on <u>both</u> of the NRAO high performance work stations.

I have recently looked into the issue of relative weightings of uv data in combined array configuration data sets. Some observers have suggested that it is important, when combining uv data sets, to adjust the relative weights to minimize the "beam skirt" produced by adding lower resolution data to an existing uv data base. A problem with the "beam skirt" arises when clean components are restored to a residual image. Clean components are restored with an idealized gaussian (typically circular gaussian with a FWHM equal to that of the dirty beam) beam and the units of an image are Jy/beam. However, the residual map has the original ("skirted") beam so the units of the residual image and the restored image do not match. The suggestion of reweighting minimizes this problem by making the dirty beam more gaussian.

I found the reweighting approach tends to effectively convert a multi-configuration data set into a single configuration data set because the data from the lower resolution configurations are down weighted. Since that defeats the purpose of making a multi-configuration image in the first place, it is not acceptable. A better way to deal with the problem is to clean very lightly or very deeply. Light cleaning is what I use as a precursor to deconvolution with vtess, a technique which consistently makes the best images of 3C353. My view is that the whole problem is over rated and that

3C353. My view is that the whole problem is over rated and that proper deconvolution techniques for producing final images (deep cleaning or composite light clean/vtess deconvolution) will not suffer from this problem.

I am writing the data chapter of my thesis. It is essentially a stand alone "how to" guide for making high-quality, multi-configuration VLA images. Special problems (large angular size and dec = -.9) related to 3C353 will be discusses "along the way" as well. The large angular size is the biggest problem because even the foreshortened D array can not sample short enough uv spacing to recover all the flux at some frequencies. Also, the large angular size and high spatial resolution combination require huge images. I now work primarily with 4k by 4k images which are the largest AIPS allows and I *still can't grid at 3.5 points per beam!* I am gridding the C band data at 2.5 points per beam which creates all sorts of problems of its own.

The X band 0.2" resolution images are really going to be nasty. I am planning to go to NCSA to use one of their super computers to run a specially modified version of AIPS which will allow making 8k by 8k images. Each 8K by 8k image is 256 megabytes and the typical clean will contain close to million clean components. A final "deep" cleaning could contain several million.

~~Inter~~office

National Radio Astronomy Observatory

Charlottesville, Virginia

18 Oct '95

To: Stefi

From: Alan & Mark

Subject: 3C353 Alabama Draft

Here's the current draft, sized for our 6-page limit. The figures are still in production, and most particularly need to have axes and angular scales added, and the drawspec gobbledygook subtracted. What is included here should give you the basic picture, however and we'll E-mail .PS files when they are done.

Fig. 1 will be a glossy b/w photo, the image is file #67 on the enclosed DAT (which has all of the basic images from the project).

On fig. 2, the FWHM is 2.75 slice pixels.
3 .. .. .. 4.0 .. ..
5 .. .. .. 2.75 slice pixels

and on all 3 the vertical scale is ~~m~~Jy per beam.

From: "Mark R. Swain" <swain@astrosun.tn.cornell.edu>
To: Alan Bridle <abridle@nrao.edu>
Subject: 3C 353 paper
Date: Sun, 2 Jun 1996 18:32:26 -0400 (EDT)

Hi Alan,

There are two files (apjltr.tex and apjltr.ps) in the ~mswain/Papers directory. These files represent a very rough draft at this stage. I am unhappy with a number of aspects of the current draft, particularly in how it fails to adequately present some of the total intensity results and the jet and counter jet and the ambient magnetic field configuration. However, I am simply out of time to work on the 3C 353 paper at this point; between an observing run (I leave for that tomorrow) and a series of bolometer tests the following week, I will be unlikely to make substantial changes to this draft for two weeks. If you wish to put off working on this paper for a couple of weeks until I can participate again, that is fine with me. Alternatively, if you wish to forge ahead on your own with the draft I have provided, that is fine too.

However, if you are going to start working on the draft at this point, we need to be more closely coupled. I am unsure exactly what your agenda for the paper is or how what I have written relates to it. I tried to summarize what I thought were the important points --- and ended up with a paper much larger than an ApJ letter. My feeling at this point is that maybe we should consider an ApJ paper instead. I don't think we can put in the figures we need to illustrate our main points and discuss all the connections to other work in sufficient detail for some people to get the point in a ApJ letter length document. However, I am by no means hell-bent on a regular ApJ paper for the sake of writing something longer. If you convince me that an ApJ letter will do the job, then I am all for the letter.

There are a few formatting bugs that I did not have time to work out. Also, the apjltr.ps file does not print on our system at Cornell. But we have a super messed up system. The file appears in ghostview just fine. If you have installed my thesis figure directory, then suitable changes in the path name should enable you to compile the apjltr.tex file under latex. To make the size of .ps file smaller, I commented out two figures at the end of the .tex file; those figures are meant to appear. Much of the text should look familiar to you as I stole it from my thesis whenever possible.

The trees finally got their leaves here about two weeks ago.

Please give my regards to Mary.

Mark

Jets (Jet & Counterjet; usual definition)

Properties:

- distinguishable by brightness gradients (Sobel)
- similar integrated flux (differs by 2 or 3)
- flat topped (demonstrate this for CJ?) (have J profile)
- J expands little if at all from 1st knot (have J plot) 3σ
- J center bright in PI (have plot)
- J B field mostly parallel at edges, perp. in cntr. (cartoon & plots)
- initial J/CJ misalignment of approx 6 degrees.
- only north edge of straight J segment points back to core
- initial J opening angle (to first knot) approx 2.5 degrees.

Differences:

- J appears to brighten more rapidly at 1st knot than CJ (measure, plot)
- J straight until near HS, CJ bends soon after detection
- can trace J into HS, can't trace CJ into HS

Questions:

- is the isophotal col. prop. of CJ differ from J? (measure CJ)
- is there an asymmetry in how quickly J & CJ brighten? (measure)
- how different is spix of J & CJ bright knots? (tomography)
- how different is spix of J knots and rest of J? (tomography)

Rails

Properties:

- detected in PI and %pol
- detected at 1.3 and 0.4 arcsec (marginally at 3.0 arcsec!)
- integrated coherently along J (may be regional exceptions)
- rails occur at steepest brightness gradient of jet (2 profile plot)
- each rail probably has broad and narrow component (int. 0.4 profile)
- narrow component only marginally resolved at 0.4 arcsec (single 0.4 profile)
- rail depth correlates w/ jet flux at rail position (have plot)
- average fractional rail depth same (w/ 1 sigma) at 1.4 and 8.4 GHz
- fractional raid depth appears to correlate w/ nothing (get cor. coef.)
- absolute rail depth correlates w/ POLA (have proto-plot)
- rails made by cancelation (ambient B pos. vec. plot, Q profile)

Differences:

- NR ambient POLA dist. differs from SR ambient POLA dist. (show dist.)
- possible difference in rail depth vs J flux between N & S rails.

Questions:

- how much do NR & SR ambient POLA distributions differ? (t-test)
- do NR & SR differ in width or depth? (go look)
- do NR & SR rail depth vs J flux cor. differ? (make plot)
- is there a cor. between rail depth and rail transverse position? (make plot)

Sheath

Properties:

- 3 to 5 jet widths (int. 1.3 arcsec profile)
- trans. int. sheath flux. cor. w/ trans. int. jet flux. (plot)
- North Sheath has systematically higher surface brightness (rotation plots)
- South Sheath has systematically higher %pol (rotation plots)
- Sheath differs little from lobe (<0.1) in spectral index (tomo. seq.)
- PA mostly perp. to jet axis (show plot)
- sheath brightens before jet but more slowly than jet (plot or cntr.)
- asymmetric Q profile along 3/4 of sheath (show profile)
- inflection points of Q profile at inflections in I profile (show profiles)

Differences:

- sheath candidate only founded on J side (so far!)

Questions:

- is there a CJ sheath around bright CJ knots? (go look)
- correlation of region around CJ w/ CJ? (make plot)
- first 2 max in sheath transverse pos. cor.? (???)
- does sheath center brighten where jet is brighter? (measure & plot)
- what is trans. cor. length for J/Sheath int. flux cor.? (measure & plot)
- how often would similar feature be found at other rotations? (make images)