

died Feb 2009

(C.I.)

3/20/84

Notes of telephone interview w/ Fred Haddock - 23+27 Jan 84 ~110<sup>m</sup>

[based on original tapes <sup>which</sup> had terrible hum + have <sup>now</sup> been over-recorded; only these notes and some notes on my copy of H's 5/83 Green Bank talk <sup>now</sup> exist]

- took Army ROTC at MIT, but did not go into military due to taking job as a research physicist at NRL
- in Hagen's group then were Dr. Ryan (?) + Harold Herman
- during the war, NRL group frequently ~~consulted~~ <sup>consulted</sup> w/ Rad Lab, e.g. he + McClain travelled there to consult Pound re mixers
- Ed Dyke (?), an E.E., worked for H. during the war
- Dyke + H did work on whether a horn, a mattress array, or dish was better for cm  $\lambda$  radar
- near end of war, H. learned of a "tool crib" man, who was real bright, had a B.S. in physics, + was extremely self-effacing = Sloanaker
- for ~2 yrs, S. worked for H. doing calc's on effects of rain drops on cm  $\lambda$  radar (full Mie Theory for abs. + scattering); goal was to find an optimum  $\lambda$  (calc's for 0.1  $\rightarrow$  10 cm were done) + avoid the K-band bad choice of  $\lambda$
- during <sup>~1944</sup> war, the submarine ~~people~~ people wanted a radar on their periscopes
- Hagen pushed for a dish, but H. pushed for a slotted waveguide
- H. + Page (later Dir. of NRL) went up to Canada + learned " " tech's from Watson
- H. + Mayer led a group of ~10 which developed these 3cm  $\lambda$  radars for the Mark II periscope (<sup>~100</sup> 200 of 'em were made); ~~was~~ H.'s design was selected over Al Beck's poly-rod one at BTL.
- 2 one-inch waveguides, each 6" long, side-by-side, + w/ 6 slots each (over)



+ 2nd ed. 1975 - 2nd ed. 1975 for counter-subject for 1971

- during the War, Lewis Strauss (of later AEC fame) had son named Lew S, Jr.
- LS, Sr. was an aide to FDR + LS, Jr. was "youngest Ensign in Navy"
- LS, Jr. worked in H's group on the sub radar + this connection gave the project a "lot of visibility" + ~~and~~ top priority
- "The idea was floating around NRL during the War that we should build a big radar + get a reflection from Moon", and that one could also do surveillance on distant  $\oplus$  regions this way
- Trexler + counter-measures people were in this from the start and it eventually led to the "Sugar Grove Coor"; <sup>(later) always</sup> T. <sub>A</sub> wanted to control the project, but they wanted the P.A. as window-dressing; Hagen + H. were thus ~ "fronts"
- H. during War really grabbed the moon radar idea + made extensive calc's, modeling the surface as small facets (H. always liked optics), etc.
- H. calc'd  $\sigma_p$  + at  $\lambda$  10 cm, calc'd that a 30-ft dish would be needed (very big for that day)
- LS, Jr. took this idea to his father + LS, Sr. was very enthusiastic
- so LS, Sr. invited Hagen + H. + wives to his very rich digs in Washington + they were super-impressed w/ meal + ambience

need to get rest of story from H.

- LS, Sr. checked them out + Hagen sold him on the idea + it looked like they would get big \$ for a large dish, but end of War ended that
- proposal for + building of dish + why the long delay
- did DeWitt's success play a role?



50-ft dish ( $b/D = 0.5$ )

- axis converter was not operational at the start + wasn't too great anyway
  - alignments of all the axes, etc. + pointing were a real problem
  - They put a small tr. at top of Wash. Mon. to measure beam pattern
  - used sun to ~~measure~~ <sup>measure</sup> pointing corrections
  - az. scale was in units like "996704"; their technique was to be constantly doing trig. to calc. next source position + then put dish there + let source drift thru
  - "Hagen boarded the 50-ft + ~~was~~ <sup>was</sup> very incompetent (good project salesman, but no scientific insight)" + 1952 (Wharton)
  - 1947 (Brazil), 1950 (Aha), eclipse expeditions + methodology of analysis plus "rain calc's" (w/ Slovenker) occupied all of H.'s time + kept him away from the 50-ft, where he wanted to work
  - he even did the 1954 eclipse analysis of Mayer's data - Mayer was only interested in developing first-rate Rx's, which he did, + he produced beautiful data, but had little interest in the sun
  - "Hagen became obsessed w/ the 50-ft dish + just wouldn't let us on it; finally he gave us 2 weeks on it at end of '53<sup>o</sup> and we immediately were able to calibrate it, + observe sun, moon, Cyg A, Tau A, + Cas A"
- pub's ⑧ + ⑨
- also on p. 118 of Serendip (1983)
- "After that, I just took the Messier list of objects and went down it - 1, 2, 3, 4, 5, ... and picked up the H II regions M8, M20, M16, M17, etc."
  - Back in '47 Obs. review, Greenstein had suggested Ori A as a thermal <sup>Orion</sup> source + they were getting other H II regions, so they went for ~~it~~ + it came in quite strong

~1948-49 H. wrote the justification for the 50-ft dish - not only lunar radar, but also Cyp A + Cas A + planets' thermal em.

(over)



- Hagen was smart, could raise lots of \$, etc., but his dealing w/ people was terrible; he never gave praise + often criticized + took too much credit for things he shouldn't have

[ H. is quite bitter re Hagen + how he handled the group ]

- 50-ft cost ~ \$100,000

~1954 - Graham Smith + Joe Lawrey's attitude (as they looked at this huge white elephant): "The most expensive dish in the world + all it can see is the Sun + Moon"

- Cambridge + CSIRO were never sympathetic to U.S. com. efforts, "but that's natural - they worked w/ their expertise + we w/ ours" - we simply sat down w/ our equipment + asked "Now what will this do?"

- The available <sup>one's expertise in techniques</sup> equipment dictated the programs in all countries - This was also true outside of P.A. - look at milk beans, NMR, etc. of Rabi, Purcell, <sup>Townes</sup> Dicke, etc.

~1946-47 - H. discussed Mic Theory, etc. w/ vd Harkat during his U.S. post-doc + vd H told him about 21-cm line, but he didn't know whether in cm + abs.

- H. was so frustrated because he was the initiator of the 50-ft dish, but then couldn't get his hands on it

- Ewen made his first 21-cm Rx's for Tuve, Bole, + Haddock, but even tho' H. initiated the whole idea of 21-cm at NRL, Hagen pushed him out of it + put McClain on it

- George Field wrote Hagen + asked for 50-ft time to look for H F abs in front of strong RS's; + then McClain did it

1947 Eclipse - H. didn't go, but he analyzed the data

1954 " - H. " since he wanted to do first of 3 cm work on

the 50 ft dish + indeed he + McCullough built a Rx + got many sources (Hagen + Hagen went to Sweden + H. showed 'em his next new detectors when they returned)



4 NRL eclipses — main scientific result was good evidence for limb-brightening +  
 $n_e + T$  of chromosphere as  $f(r)$

— role of spicules + mechanism of coronal heating were much debated then

— "E.O. Hulburt was a damned good, craftsman-type scientist who liked  
 to do exper's in the Cavendish tradition" (he was head of Atm Sci's Div)

+ encouraged eclipse work + got Friedman into X-ray astronomy, ~~etc~~

— eclipse expedition requirements meant that the NRL group developed ~~very~~ excellent  
 techs for achieving stability + sensitivity in the face of adverse conditions

— This served ~~the~~ <sup>group</sup> well later on w/ 50-ft + 84-ft work

H II region work — one of the first R.A. results which was immediately integrated into  
 optical astronomy

1/54 Washington conf — set up + run by Hagen + Tuve

— Aller was there + was very excited about the new H II radio results

+ had his student Boggess work on it

— Bok had a real love of astronomy, but did not have the physical insight of  
 someone like Menzel; he + Hagen hit it off (e.g., for founding of  
 NRAO), perhaps because of their common shallowness

[27 Jan 84]

— he agrees that lack of NRL (open-lit.) articles  $\approx 1951$  was due to their different  
 orientation ~~on~~ to R & D rather than open science

— one wrote reports, etc. rather than Ap J articles; there was not the scientific leader-  
 ship there to change that

— H. was a real "literature buff" + kept a  $\approx 100\%$  complete collection of R.A.  
 reprints, incl. a lot of NRL Translations of Soviet lit., etc.

(over)



- 1954-55 European meeting and/or on Crab optical p<sub>z</sub>  
~~1958~~ - ~~Paris~~ - discussion on p<sub>z</sub> of gal by rad<sub>n</sub> + H. went back to NRL w/  
 The idea that NRL should measure p<sub>z</sub> in R.S.'s  
 - he initiated for this the rotating feed system; he was working on  
 The radiometer, etc. for this when in 3/55 he got offer from  
 Goldberg to go to Michigan - accepted in 8/55 + went 2/56  
 - so H. walked away from both this polarimeter + from planet work  
 (which he could see coming + wanted to do) + Mayer took them over  
 - Mayer developed finite Rx, but at first it was too variable w/ ambient T,  
 so he went back to Haddock's design  
 - M. did not acknowledge H. in his first p<sub>z</sub> paper, but did in a later  
 review paper on p<sub>z</sub>  
 - M. did not have the scientific insight, but could do the obs + build well  
 - H. did not want to leave NRL, but Goldberg gave him much inducement  
 - After the 1/54 conf., G. wanted to get into R.A. + Helen Dodson also was  
 enthusiastic about @ R.A.  
 ~1950-52? - at one stage, H. brought up <sup>to Mich</sup> lots of his 3cm @ data <sup>from NRL</sup> + he + Dodson +  
 Muller looked for radio burst-optical flare correlations - There was  
 a very tight correlation (< 1") + she was excited; H. suggested that Corvington probab<sup>\*</sup>  
 - G. <sup>applied for</sup> \$50,000 from ONR for a Wild-type dynamic spectrograph  
 but couldn't get the \$ since he had no R.A. on staff  
 - G. tried to get Wild himself, but then he + Attwood (EE Dept) went after it  
 - H. refused G.'s first offer, saying he wasn't an astronomer, didn't have a  
 Ph.D., wanted to work w/ the biggest dish, etc.

\* had lots more such data + that's how Dodson - Corv. collaboration began; Dodson never acknowledged this, but H. admits it's also his fault for publishing so little.



— But G. was aggressive + w/in a few months responded to each of H.'s wants: little teaching, tenure, 60-ft dish, ability to do research on RS's + planets very soon after initial solar R.A.

— Robertson, Chief Scientist at ONR, ~guaranteed that if H. came, ONR would fund dish

— so H. went to Mich in 2/56 as Assoc. Prof of E.E./Astronomy

— G. arranged for Wild to be there in Sp 56 + Wild gave H. the same advice he gave Alan Maxwell: get one of the A.I.L. swept-freq. Rx's (for counter-measure military use) off the shelf

— so this was an easy way for H. to fulfill his obligation to ~~do~~ set up a solar RT + then get on to his real interests

[H. says Maxwell over the years always berated H. over "noising" into his field, yet M. in fact copied most of H.'s set-up]

①⑨ + ①⑦ 10/57 — first dynamic spectrum of a Type IV burst <sup>100-600 MHz</sup>; Denise was visiting Mich + recognized it [Wild had never been able to record a Type IV burst + "didn't believe in them"]

— 28-ft dish w/ a multiple feed for 100-600, + ~1000 <sup>2000?</sup> → 4000 MHz was used for solar dynamic spectra

— H. hired Takakura (from Hatayaka's group) + T. delved into synchrotron theory

— Dodson was forbidden by McMuth to look at the radio solar data or to have anything to do w/ H.

— McM. was a real prima donna [domino?]? + dictator, although charming

— " had good connections w/ Board of Regents at U Mich + had "given" Goldberg to be director (at young age)

(over)



- Goldbey was a flunkie / chauffeur for McR. - always under his thumb
- ~1955 " finally got over some <sup>chronic</sup> health problems ("gout") + only then did he "blossom" as a chairman + McR. got angry + kept Podson away
- his original 60' proposal grew to 85' + he got the first Blacow-Knox 85', designed by Howard Tatel (DTM)
- '58 - \$250,000 from ONR for 85'
- Goldbey was never very interested in the science or day-to-day operation of the radio observ. - he was just being a good chairman + having a diversified dept.
- 85' did pr work + then first detection of Mercury
- maser Rx's were developed at Willow Run Research Lab., U. of Mich (an E.E. Dept. off-shoot) + such a Rx was used on 85' for Mercury; but Turner beat them out w/ first R.A. maser Rx at NRL
- H. felt he wasted a whole yr w/ the care + feeding of that first maser Rx - <sup>(9-1)</sup> liq. helium had to be run 30' up to the feed + ran out quickly (open system)
- That Rx also allowed them to get first radio det. of a plan. nebula + of Saturn [despite Drake's claim to Saturn in Pure IRE (2/58)]
- Mike Klein + Bob Hobbs were grad students at Mich.
- H. first thought about low freq R.A. from space in Σ Vanguard days; ~~he went to Newell~~ a McDonnell-Douglas group, <sup>also</sup> wanted to do it + went to Newell at (new) NASA for \$, but Newell favored the science being done in a Univ.
- ~1961 - Michigan astronomy really suffered when Goldbey + Liller went to Howard + Allen → UCLA



— cont. 2158 Proc IRE which H. edited (but original idea was not his) = "R.A. was dragging astronomy around by the nose"

- ⑪ = model for Cas A's NT spectrum — 3 Cambridge data pts in spectrum did not line up w/ 3 NRL pts — due to poor " cal. of noise diodes  
— H's basic idea was for free-free abt surrounding synch. source

### NRAO/AUI early days

- The great battle was Tuve vs Berkner (administrator of AUI)  
" little science vs 'big
- R.A.'ers were generally cool towards idea of NRAO, but Goldberg + Bok<sup>his students</sup>, e.g., liked it
- Bok's students were a different breed from the old-time R.A.'ers
- when \$ came from the govt, Berkner took over + put Emberson in charge, but E. was "too sweet" a man for the demanding task