

16/24/82

(13)

SHKLOVSKY - notes on 8/18/79 interview

Tape 125B-250

[1933-5]

degen [1938]

- his first 2 yrs were at Vladivostok Univ., then studying physics at Moscow Univ.
- his interest was ~~physics~~ in nuclear physics, but he got bored into astronomy
- Dept of Astronomy of Moscow ^{St.} Univ. = Sternberg Astron Inst
- worked there during WW II → ~ 1970 (then to new Space Research Inst)
- could not fight in Army due to poor eyesight
- ~~after WW II~~ his candidate thesis was in solar physics - solar corona was then an "empty field"
- pub. of high ionization temp; he did work 2 yrs before Biermann's
- the high-exc. lines convinced him that corona was hot - a plasma
- he worked out the equil. between coll. II + radiative recomb.
- he derived the N_e values, $T_e \sim 1-2 \times 10^6$

~~Other~~

- he is one of few persons in early R.A. who did not come out of radio physics or radio ^{eng'ing}

S/p 45 - ^{N.W.} Pariiskii (father of present R.A. 'er) of ^{Lebedev} Phys. Inst. ^{[was also his graduate} ~~advisor]~~

~~advisor]~~ [~~Heger's~~ discovery a lecture being given by ~~him~~ was his motivation to go to Lebedev for a series of lectures; there he heard, from a military man summarizing wartime progress in radio physics, of the British discovery of ^{radio} α bursts

- immediately S. understood that \odot should have 2 radio components (meter λ 's):
 - (1) thermal, at $T \sim 10^6$ K from coronal
 - (2) variable, w/ plasma freq's determining $S(\nu, t)$ - he predicted the later-observed delays [8/86 - I find no mention of delays in his 1946 paper]

- so in 1945 he worked out his Astr. Zh. 1946 paper

- he encountered much prejudice against idea of hot corona

- Astr. Zh. 46 also discussed gal. lg.; S. wonders why Reber used the term "cosmic static"

(OVER)

- S. claims to be the first astronomer in USSR to pay any attention to Jansky + ^{Reber}
- during WW II he read a "fair review" on plasma (a new term) by a Gosman [I can't decipher the name] translated into Russian in Usp. Phys. Nauk
- he learned of plasma osc's there
- scientific exchange between West + USSR was very limited in post-war years; his 1947 Nature article led to much more acceptance of his ideas in West
- he was very interested in R.A. because 'was entirely new branch of science w/ great future prospects'; his character is to prefer new fields (e.g., in 1950 he is still most interested in γ -rays, 55 433, etc.)

- ⑤ - used IS dispersion as an explanation for $v(t)$ in Cyg A fluc's
- used Wolf-Rayet * as explanation for Cyg A fluc's because their optical spectra \rightarrow ^{mass loss}
 - these were very happy times for him in his research; he was a sort of wunderkind - it was as if "he had open eyes + others were blind"
- ↑. E. ch. 5 - This phase ~~of~~ cont'd right into the 1960's, but then the field changed, i.e., computers became of great importance, + "this is bad for deep considerations"
- "I am too old for this; my style is old-fashioned in that I prefer to work by myself, not in teams"

Tape 126A

- he claims credit for the term "relict radiation", which he says is a much better one than "blackbody radiation"
- he also says he ^{first} pointed out that the old CN $T_{exc} \sim 3K$ result could be explained in terms of the new microwave bg.

Tape 126A (contd)

— by chance he read the very ^[one 9] short mention of ν H₂ prediction in ~ 1947

Obs review by Reber & Greenstein

— immediately, he saw how important this line was for gal. structure + ISM physics

(4) — The $\tau_{21cm} \sim 10^7$ yr was fantastic — no one believed it

— he calc'd that line should be easily detectable

— he tried to persuade [↑] Vikhitein, chief of Crimea R.A. station, to do

this [↑] (excellent radio physics experimenter — very free from astronomy)

— Vikhitein [↑] was the main person actually at the Crimea station

— after several weeks, V. said that the experiment was very simple + told him about

his idea of freq.-swing, etc; several months later, S. was astonished to learn that all work on this had stopped (no antenna was built, no Rx, etc); S. says ^{Academician} Lendov [!] stopped this project

— "People call me a theoretician, but I am not really one; I do not like to work out

detailed models, etc.; rather I use my brain + make predictions; I like to stay close to the data" ("the real business" of astronomy)

~~I am not an interpreter~~

— he cites (17) vs (35) = his prediction of detectability of H₂ regions, w/ long delay before it got done in USSR (+ meantime they were "scooped" by NRA), as another example of what he has a good idea, but the Soviet observers don't follow thru

(4) + (15) — molecular lines considered; he was also working on upper atm spectroscopy ^{at that time}

— he had no ν in ml spectroscopy; started from zero

— despite lack of optical OH lines in ISM, he thought it a better possibility than CH; but instead Soviet R.A. was looked for CH, not OH [when?]

~ 1966 — he was first to link star formation to OH emission regions

(6) — "This work is absolutely a mistake"; based on the spherical shell of radio stars; first use of term "radio halo"

(over)

Tape 126A (contd)

M400

- with a "mushy" for strong RC's
- (11) — Kiepenhauer's idea of synchrotron rad. for gal. was much better than Alfven's
 — Ginzburg ^{got Ginzburg} + he quickly developed this idea + were sure they were "seeing" most of world ~~the~~ "was blind"
 — G. was interested in the physics-side of the phenomenon (e.g., spectral index), while S. applied it to sources + gal. bz.; he quickly dropped the idea of the gal. bz. being the \int radio stars

"I was the first one who understood the general relation between origin of CR's + R.A."

with 2 comp. model

- (17) — "I first realized the connection between SNR's + CR's + synch."

- (20) ^{with} 1953 — inspiration for Crab ~~optical~~ optical + radio ~~em~~ being synch. came to him all at once in a train station [he told me off tape that this happened because of the tremendous sense of relief (joy) he had, esp. as a Jew, at news of Stalin's death] (but he had obviously been thinking about the Crab for several months)

see (C)
on card →

- (21) 1954 — Oort came to dedication of newly re-constructed Pulkova Observ.

— S. explained to him his ideas of synch. rad. in Crab, + Oort became convinced

- (34) — convinced that optical jet was synch. due to (1) high s.b., + (2) no optical lines

— Soviets looked for opt. p/z, but found none due to ^{their} low resolution; Baade found it

— he did not go to 1955 Jodrell meeting; first visit to West was in 1966

for Jansky Lecture at NRAO, Charlottesville, Va. [which WTS heard]

--- except for 1947 Soviet expedition to Brazil for Eclipse

— he worked on optical measurements on land (~400-500 km inland) w/ Pericelli

↑ end 126A

(40" map exists on 126B)

WJD
8/14/86

Tape 126B

8/18/79 Shklovsky interview (contd)

re ⑫ + ⑬ — world history is a parttime of S's

③⑦ — Baade + ^{B & M}Minkowski '54 were not certain that the Cas A filaments were a SNR,

but S. was, although he was pushing for an AD 369 SN.

— (gives his current views on why Cas A was not seen in 17th cent as a SN)

— a guiding principle: "I don't believe in miracles"

①⑦ — he opposed B & M's "colliding galaxy" idea for Cas A.

— he learned of B & M's redshift of Cas A from a visit to U.S. by Ambartsumian.

— he did not correspond w/ any Western astronomers at this time (early 1950s)

②② — "popular book, but content is very dense" (translated into Chinese)

③⑧ — advanced book; many American (young) radio astronomers of early 1960s told

him that the 1960 English translation was very useful to them

late '50s — S's interest shifted to planetary ^{PN} nebulae; lots had been done on the

astrophysics of the emitting gas, but little on the main question: where does a PN

fit into overall stellar evolution?

~ 1956 — he argued that PN came from a red giant, but nobody believed him

until late '60s

④⑨ — his basic theory on SNR expansion and decrease in radio flux with time
grew out of his work on PN

— 1960 — he wrote to Cambridge group + asked if they could measure current

Cas A flux with same set-up as in early '50s [and they did]

— his interest in SETI (search for extraterrestrial intelligence) began with the

1959 Morrison & Cocconi article

1962 — S. was invited to contribute to a collection celebrating the fifth anniversary

of Sputnik I — he ended up producing his book Intelligent Life in the Universe

(now ~ 1979 in its fifth edition)

[OVER]

- S. was/is greatly amused by Sagan's English edition of his book - S. told Sagan he could update it, but Sagan ended up doubling the size!

- the symbols indicating "who wrote what" are also amusing to S.

- in recent years, S. has become more pessimistic about frequency of extraterrestrial life

- he rails against the idea of interstellar travel ^{+ colonization} - very far into the future

(discussion of TV radiation leaking from earth)

- S. says ^{detailed} method of VLBI proposed by Kardashev + Matveyenko in 1962 in a Soviet radiophysical journal ^{+ Shklovsky}

- but idea could not be followed up in USSR due to "lack of production"

- shortly after this, Lovell visited USSR + S. told him about VLBI, but

Lovell passed it off as if music device: "all radio sources are extended"

Kardashev - "brilliant - my best student" - has recently achieved a VLBI

between ground + a satellite

Q (WISS): why ^{was} Soviet theoretical work (in the <1960 era) ~~so~~ so much stronger than observational in radio astronomy?

A [Shklovsky] = he agrees this was so, but never gives an answer; rather he gives several examples

(Vittorich - O occultation of Tau A, ~~the~~ Dravshikh² - recombination lines)

to emphasize that there were obstacul successes

550

650

690=end

S&K 10/1/79

prints which SHKLOUSKII was not willing to record ^{on tape} in interview of 8/18/79

SECRET

Ⓟ Re trying to get Soviet observers to go after 21-cm line after his '49 paper
 Vitevitch showed him plans for a Rx, etc., using frequency switching, but then nothing
 happened for a long time despite S's "begging"
 - only in 1960's did V. tell S. the reason: V. had been told by London that this
 was a hopeless experiment (since A is small, etc.) + a waste of time

Ⓟ Re detection of Crab Nebula
 - ~1948 Kheikin had a sea-cliff inter. operating in the Crimea (Black Sea) Φ
 + S. wanted him to use it to look for side-lobes from Crab
 - re S. calculated the azimuth of rise + set for obs., but found that the 42's were
 not over the water, but obscured by hills
 - he tried to persuade Kheikin to move the λ 1.5m set-up but V. had little interest in
 non-solar work + that was the end of it
 - shortly came Belton's work on Tau A + its ID

(OVER)

© [Tape turned off at ~500 on (126A)]

Hand of transcript 1982 (1000)

re Reason for burst of creativity + ideas etc in 1953

- shortly before Stalin's death in 1953, he felt the "Jewish doctors" were out to do him in and there was all sorts of purges, anti-Semitic feelings, etc. in the country
- This weighed very heavily on S. + other Jews (reference to the article on "discussing of Jewish doctors, etc.")
- while waiting for a train, he first heard of Stalin's death
- The emotional "left" and ~~right~~ joy was such that in the next 30", while standing on the very crowded train, he worked out all the basic essentials in his '53 Acta 24 paper on the Crab
- he had been bothered for some time about various aspects of the Crab + trying to figure it out, but all of a sudden it all fell into place when he saw that the optical continuum also must be synchrotron radiation
- The mass of nebula became much lower + more reasonable, etc. (S 2:15 1952 papers + 7 1953 ...)
- [it was not clear to me how much more of ~~the~~, if any, of his "1953 spirit" he ascribed to the passing of Stalin + the resultant release of tensions - 4/7/83]