AUSTRALIAN NATIONAL UNIVERSITY

DEPARTMENT OF NUCLEAR PHYSICS

14UD TANK OPENING REPORT No.40

20th to 23rd June 1983.

(3½ days open.)

REFERENCES: Earlier Tank Opening Reports are referred to by the notation (12/4) etc, meaning Report No. 12, page 4.

REASON FOR TANK OPENING

The opening was scheduled to coincide with a visit from Robert Rathmell, of N.E.C. Fortunately, the machine had run out of terminal stripper foils; unfortunately, the opening was somewhat preceded by lost charge problems which rendered the machine uncontrollable.

PREAMBLE

The 14UD was last closed on 20th April and there were no problems with conditioning. The machine was used at 5 MV for the first day and the day after ran at 13.7 MV for two or three days, this in spite of the fact that the tube had been let up to atmospheric pressure. On the second day after button-up there was a mishap with the tube vacuum. A thermocouple gauge head on the beam tube below the H.E. ball valve was broken by the gripping device on the shorting rod insertion mechanism. The device had to be held aside as it passed the gauge. Luckily the gauge was not sheared off, only cracked; this allowed time for the ball valves to close fast enough to hold the tube pressure below 1000 microns. One of the authors informed the other that, for a year, he had prophesied such a disaster because the thermocouple was in a stupid place and would inevitably get hit when someone was changing rods in a hurry. The other author said that this was an inaccurate statement because it had been for three years, not one, that he had both endured and withstood the irritation of persistent nagging. Beyond this single, sharp exchange there were neither recriminations nor manifestations of contrition. What mattered was that the foils and conditioning level survived the incident.

On 10th May, while running at 13.5 MV, there was a big tank spark following which the machine would not go back to the same voltage. There were no problems up to about 12.5 MV, at which point there were kicks in the H.E. vacuum indication. With no beam in the machine there were corresponding current kicks on the H.E. cup, characteristic of what is seen with tube conditioning. Progressing through successive units the effect was eliminated when Unit 27 was shorted. We continued with this configuration for 6 days after which we found that the fault had cleared itself. The machine held 13.55 MV without the unit shorted; as there were no longer any problems we left the rod out.

On 26th May, the superconducting buncher was first used in an experiment with a 19F beam on a 30 degree beam line. The pulse was measured to

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be less than 320 picoseconds, which was due to detector resolution. The beam was chopped to give 107 nanosecond pulse separation. Unfortunately, the average intensity was less than the 10 nanoamps threshold of the present phase detector system; therefore it could only hold the beam in phase lock for about five minutes at a time.

Low terminal volts.

On two occasions in May, we bettered our previous low voltage record (39/6) by running at 4.15 MV and early in June our enthusiastic low voltage users went down to 2.94 MV with the same shorting rod configuration. No corona current was drawn, but the 14UD ran so steadily at this voltage, with little reduction in beam intensity, that the controls were not touched for hours on end.

The new vogue of low voltages enticed a different group of experimenters into the sheer peace of accelerating particles this way and the 14UD went cheerfully down to 2.5 MV for $^{7}Li(2^{+})$ with units 4 to 13 (inclusive and 19 to 28 (inclusive) shorted; everyone was pleased.

On 1st June we discovered two things about the L.E. midsection sublimer pump: we were unable to switch to the other sublimer and when the sublimer in use was turned up it put electrons into the tube; this caused the x-rays to rise to 10^{4}. We determined this to be a repetition frequency of 330 c.p.s. which is shaft speed.

Ripple tests.

On 14th June Robert Rathmell arrived in the laboratory as a Visiting Fellow for four weeks. After an enthusiastic first day of general discussion we set about making what tests we could without unduly interrupting the experimental run in progress. Measurements on the capacitive pickoff established to Robert's and our satisfaction that there was no ripple at chain pellet frequencies, (400 cycles) or expected column vibration fequencies, (a few cycles). We saw some small ripple at 1.7 kc/s and planned to locate its source during the tank opening. We had only one c.p.o. installed and this was inadequate for unambiguous ripple measurements; a second c.p.o. was promptly made for fitting at 180° to the first when the tank was open in a day or so.

Lost charge.

We next turned out attention to the mysteries of lost charge which, in recent weeks, have been more elusively and irritatingly mysterious than for a long time. Non-zero lost charge readings can be caused by many phenomena. In most cases they are ascribable to x-ray ionization of the SF_6 because of incomplete electron suppression by the accelerator tube when running beam. There are other cases where the cause of lost charge is less obvious; for example, in Report No. 12, page 2, we related lost charge to voltage conditioning producing breakdown products in the SF_6. On that occasion the lost charge was directly proportional to the triode current; the best Bandaid procedure was to operate with the triode needles withdrawn so that both these currents were minimized.

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To explore the relationship between lost charge and triode current in the present circumstances, we ran the machine with the triode needles at varying distances and, for a series of charging voltages, recorded all relevant parameters, including triode current indicated by a separate meter in series with the triode system itself. During these tests, when triode current was increased by inserting the needles further, the lost charge fell with negligible change in terminal volts. For example, an increase in triode current of 10 microamps made the lost charge fall by the same amount. This is exactly opposite to the behaviour mentioned above. A large, confusing table of values was then studied by all participants, each of whom had his own unique ideas as to how the triode system worked and where, in the circumstances, lost charge would have vanished to, and could draw a convincing diagram to prove it.

There were at least three schools of thought; they required, respectively, corona-producing deposits on the triode mushroom, on the wall near the mushroom, or on the terminal. No-one was confident enough to bet the usual darkroom quantum of conviction. (This was just as well, since not one of the predictions was convincingly supported by subsequent inspection inside the machine.)

Suspicions of a leak in the L.E. tube in the vicinity of the midsection, led us to carry out some tests with the residual gas analyzer. The results indicated weak evidence for some SF_6 in the tube and we decided to carry out a thorough leak chase when we got in the tank.

OPERATIONAL TIME

During the 60 days since the last closure, the 14UD operated for 1193 hours. This was 86% of elapsed time, excluding the days for gas transfer.

THE TANK OPENING

Smell in the tank.

We had visitors from the Tata and Bhabha Institutes, Bombay, who were most anxious to be taught how to sniff a newly opened tank. Robert Rathmell wished to calibrate his personal sensor against the local talent, too. Immediately the lower door was opened the most experienced olfactory system in the department took the first sample and its owner pronounced that there was a perceptible smell, but it was very slight and not one to cause great concern. The younger author and Robert Rathmell agreed with the assessment and then the visitors were encouraged to sniff freely and commit the sensation well to memory. The atmosphere in a newly opened tank is not to be taken lightly and we have found it to be a significant indicator of gas condition, though it would be very difficult to make reliable assessments by this means. We drew to the attention of our visitors' noses the marked worsening of smell from the tank for the first 15 minutes, or so, of forced fresh air ventilation. This is ascribed to the possible hydrolysis of adsorbed breakdown products, producing SOF_2, which smells.

Exploratory tour.

The first thing we did in the tank was to go to the stabilizing triode system and look for the dirt, flakes or other fault which we had determined

to be so obviously the cause of the lost charge. We found nothing wrong with the needles, the mushroom or the insulators. There was, however, a collection of innocent-looking dust and particles on the terminal, especially at the gap between adjacent spinnings, (see photo). Remembering the conjectures of a day or so earlier, we all felt not a little deflated. Rathmell was closest to the mark, but he claimed no bets. From outside the tank, lost charge has often been explained; inside the tank there is equally often nothing to explain the explanations.

The composition of the innocent material was difficult to establish. Some was successfully transferred to a self-supporting carbon foil for Rutherford back scattering analysis. Such material is a thick target and so only provides ill-defined edges in the spectrum. The bottom spectrum in the figure is taken with the dust source. Above it is a spectrum obtained from a carbon foil which was left in the machine since the last tank opening. It was placed on top of casting No. 1, still on glass and so exposed to the rain of breakdown products. Keith Fifield floated off the carbon foil and carried out the RBS analysis shown in the centre of the figure. The top spectrum is from an unexposed blank carbon foil. (The Ca is from the release agent.) These spectra demonstrate that FeF is probably being deposited in the machine and that the terminal 'dust' is also of this composition. The fibre-like appearance may be due to the electric field collecting the FeF into strands.

On the terminal, opposite the triode, there was a brown patch and there were brown patches on the corona assemblies. Tests of the terminal patch showed sulphur, with no metal content. Again we report that there was very little of the grey powder which was so often noticed on shiny surfaces.

Leak chasing.

The L.E. tube, from the top of the column to the terminal, was helium leak tested twice, with ancillary flanges taped and the tube bagged. No leaks were found, though the detector (helium mass spectrometer), was tested by introducing minute amounts of helium into the tube at the sputter source.

A spectrum from the residual gas analysis at the tube entrance is enclosed. The peak due to SF_5^+ is marked. The other peaks are due to hydrocarbons which have always been present. (See RGA plots enclosed with Report No. 14.)

And so to work!

Foils.

The terminal foils were renewed where necessary. In all, about 240 were replaced.

Shaft bearings.

The upper bearing in casting 10 was replaced. Bearing failure in the upper rotating shaft motor led us to put in another motor.

Points.

There were several cases of tube points at irregular angles. Robert Rathmell was so intrigued that we had been running like this that he took some "I wouldn't have believed it" photographs to show them back home.

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Since we have had the 14UD we have set the points by eye, paying more attention to gap distance than uniformity of the assembly planes. Robert told us that N.E.C. now set strings of point assemblies by means of a special alignment tool.

We reported, to our shame (38/2) that a column point assembly had been taken out at the previous tank opening and we had closed without noticing its absence. Because of this, both authors then inspected the points with exquisite care at the end of the tank opening last reported. We now report, with twice the shame, that two adjacent column points were found to be absent in Unit 6 during this tank opening. These were almost certainly a pair which had been removed because of damage when one fell onto the other. They were taken out, and not replaced, at the very time we found the first missing one. These things teach us humility; history teaches us that humility has a short half-life.

The accelerator had exhibited no evidence of complaining about the two missing column points during the last running period. The corona had bridged the two open gaps forming large, brown stains on the next available corona point plane. Machines work when they shouldn't and don't work when they should.

Chains.

The two chains were examined with extreme care using the light pipe candling technique; once again, no defects were discovered in any links. Robert Rathmell complimented us on our set-up for examining chains and the thoroughness with which we used it. He examined lengths of chain from past breaks and was shown the fine cracks which we have reported on various occasions. He was surprised that there were not more notably larger cracks in chains that had been in bad enough condition to break.

Idlers.

About four stabilizing idlers were serviced because of bearing problems. Two had seized completely. Some contact points on d.c. idlers were renewed. A sufficient period of operation with A.N.U. design has elapsed to judge them against the current N.E.C. version. We conclude that N.E.C. have overcome the defects in their original design better than we have. We shall soon scrap our version and revert to N.E.C.'s.

Mid-section sublimer pump.

A broken microswitch explained why we could not change sublimers; we renewed it. The fact that both sides of the transformer secondary feeding these sublimers are floating from local ground enabled us, with a diode, resistor and capacitor, to use the electron emission to build up its own suppression voltage.

Insulating gas.

The first pipework for our additional SF_6 purifier was fitted to one of the ports opposite Unit 21 which had hitherto been used as a viewing window. The new pipe was terminated with a valve for the time being.

Breakdown products.

The gas from the output of the recirculator has been monitored at frequent

intervals with the conductivity cell and has shown no response as yet. The present load of VIVALYME and alumina has been in use for 2240 hours of operations, corresponding to an integrated total charge of more than 150 mA.hours. (The load was changed about 250 hours later. See page 8.)

The first load failed to provide 100% purification after 1232 hours for an estimated integrated charge of 60 mA.hr. The difference is attributed to the fact that the first load was also removing breakdown products present on the surfaces within the pressure vessel. The alumina has not been reactivated since loading, but the moisture level has remained below 10 parts per million.

We enclose a preprint of the A.N.U. report entitled Aspects of Breakdown Product Contamination of Sulphur Hexafluoride in Electrostatic Accelerators by T.R. Ophel et al. (ANU-P/854; June 1983).

Miscellaneous.

An additional capacitive pick-off unit (page 2) was installed and calibrated by applying 240 VAC, 50 cycles/sec, to the terminal.

Cleaning.

We had noticed, after the last closure, an undoubted return to eerie stability, a characteristic which has been absent for some time. After deliberating the matter we remembered that the last tank opening was for only half a day, and at the button-up (39/6) the column was tacragged by old, slow hands; not those of our oft-praised students, swift in their youth. We decided that perhaps we had been more persistent in pushing the tacrags well between the rings, removing more thoroughly the gritty particles which always accumulate there.

Button-up.

When we began the charging tests, immediately prior to buttoning up, there was a disconcerting noise associated with Chain 3. We opened idler castings again and found an idler with a bearing that we were not very happy about. We replaced the bearing and the noise continued unabated. We spent some time trying to pinpoint the origin but were quite unable to. We have fallen foul of this sort of problem before, right at the point of button-up. It is our own fault for not putting into operation the remedy of always performing, as soon as we get in the tank, the same tests that we perform just before we get out. By making such tests we would find out if anything was wrong when there was still plenty of time to do something about it.

The tank was roughed all night and in the morning the vacuum was 400 microns. This was by no means as good as the 150 microns normally achieved. The poor base roughing pressure was due to a leak which we discovered at a flaw in the weld on the new c.p.o. flange. The flange was rewelded in situ, the vacuum improved and SF_6 was put into the machine.

When the first leak tests during gassing up were made at 2 p.s.i.a. a serious SF_6 leak was found at the rewelded c.p.o. flange. The re-welding had sealed the first leak, but so stressed the weld as to cause it to crack over 200 deg. of arc. The SF_6 pressure was reduced to atmospheric, as measured by a mercury manometer at the level of the flange. The defective flange was whipped off and a blanking flange whipped on at remarkable speed.

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We then continued gassing up.

After gassing up we repeated the measurements with the residual gas analyzer mentioned at the end of the preamble. The results were the same, suggesting that the leak on the tube is pressure sensitive. In regard to the leak, Robert Rathmell remarked, after the laborious leak chasing, that he had just been reading one of our old annual reports which was lying in the Control Room and had noticed that we wrote, in 1976, "The puzzle of the anomalous pressure at the midsection triode pump of about 10^6 still exists". Plus ça change, plus c'est la même chose; a free translation of which is: People read what we write, even if we don't.

Chain survey:

We said in an earlier report (37/10) that there appeared to be little point in publishing figures from the survey before we were satisfied about the effect of breakdown products. Since results from our conductivity measurements continue to correlate to fault-free nylon links in our new chains, we shall wait until we have better statistics on improved chain lifetimes before expressing great relief too soon.

Initial performance.

The machine was given to the experimenters at 5 p.m. on Friday 24th June. At 9 a.m. on Monday the bearings on the generating voltmeter had failed. Emboldened by the earlier success of quick flange changing with SF_6 in the tank, we took the gas down to atmospheric pressure again and replaced the GVM with a blank flange. The GVM motor was replaced and the grounding brush for the rotor much improved. While we were at it, a new, non-leaky capacitive pickoff was installed at 180°. The tank pressure was then restored to normal 90 p.s.i.a.

We carried out some vibration tests, using the new capacitive pickoff unit in conjunction with the original one. No logical explanation was found for the 1.7 kc/s ripple seen on the c.p.o. before the tank opening (page 2). Not surprisingly, this ripple was still observed after the machine was closed and running again. It was not cancelled by adding the signals from the two c.p.o.'s but, because of the frequency, it was still assumed to be associated with vibration rather than terminal voltage ripple.

Turning off the rotating shafts had little effect on the signal. With the chains off, the terminal voltage dropped to the corona current threshold of about 7 MV, and, of course, the c.p.o. displayed no ripple. We discovered that thumping the tank wall with the wooden handle of a mallet excited a 1.7 kc/s signal on the c.p.o. Not knowing when to leave well alone we then thumped the tank with the rubber end of the same mallet and found a 400 c/s signal. Ignoring the confusion of the evidence we stick by the assumption that the 1.7 kc/s c.p.o. signal is tank wall vibration associated rather than actual voltage ripple; even if it were voltage ripple the c.p.o. calibration would predict it corresponded to 55 volts peak to peak terminal voltage ripple.

The calibration allows us to put an upper limit on the terminal ripple of about 500 v.p.p. at all frequencies greater than 10 cycles/sec, but unfortunately no limit near the column frequency of about 2 c.p.s.

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Tests of the L.E. midsection were moot because it appears that there is no power there. In our haste to repair and re-install the electron suppressor and sublimer switch we neglected to test the apparatus in place. We repent at leisure.

It was not long before we found, during the early voltage tests, that some kind of lost charge was back with us. There was little to do but sit round the same table in the control room and debate the matter, as before. Robert Rathmell was the first to come up with a possible solution: fuzzballs! Fuzzballs are small conglomerates of some type of matter which adhere to a 14UD terminal and, because of their very nature, cause corona to the walls of the tank, losing charge; (plotting our lost charge as a function of terminal voltage yielded a curve with the characteristics of a corona point). The fuzzball theory was hotly disputed by some, though not by others, who held it to be as sensible as anything else said about lost charge in the past week or so.

New conductivity measurements on the gas produced satisfactory readings; likewise, the hygrometer continued to indicate the satisfactory value of about 8 parts per million. While there were no measurements to indicate that the purifier materials, VIVALYME and alumina, were at fault they were replaced with new load on 12th July (see page 6). The load removed had been in use for 2511 hours. The gas was then recirculated for 8 hours.

Lost charge.

The lost charge now observed was independent of triode current and only a function of terminal voltage. At 14.1 MV, 37 microamps showed on the lost charge meter. In order to see if this problem was associated with a sub-section of the machine and also because we wished to operate above 14MV, the machine was conditioned in subsections for about 10 hours. Very vigorous tube conditioning occurred near 1 MV/unit in units each side of the H.E. foil stripper. Presumably there are bits of carbon foil which need conditioning away; this was, in large measure, accomplished.

When all the shorting rods were removed, there were 100 microamps at 13 MV. This rapid increase in lost charge following conditioning mirrors an experience reported earlier (12/2). We applied the same solution: home to bed.

On the above occasion, the lost charge was eliminated by reactivating the alumina. At that time it was demonstrated that the SF_6 was dry, but nevertheless, reactivation of the alumina banished the lost charge for a few weeks. We then took this as evidence that the freshly reactivated alumina removed breakdown products, and that these products were rapidly produced during conditioning. In the current case, there were 5 bits of information:

1. The lost charge increased rapidly during conditioning.

2. The soluble fluorides breakdown product monitor (Conductivity cell) said that the SF_6 was still clean and that the output of the VIVALYME/ alumina purifier was free of breakdown products.

3. The b.d.p. monitor had never responded to sparking and conditioning, thus going against our expectation for breakdown product production

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and N.E.C.'s experience on small machines.

4. The Shaw hygrometer said the moisture level was 7 p.p.m.

5. The VIVALYME had outlasted its liftime, based on our first experience with its use.

It was decided to postulate that the b.d.p. monitor was insensitive to a b.d.p. causing lost charge and that the VIVALYME had lost its purifying ability; therefore, the load of VIVALYME and alumina was removed from the dryer and a fresh, pre-dried load put in. During this procedure, the calibration of the hygrometer was checked in a different way to that employed hitherto: it was simply left exposed to room air, as the manufacturer recommends. Under such circumstances, the meter should read greater than 1000 p.p.m. It only read 800 p.p.m. and adjusting the calibration knob could not achieve the correct reading. We concluded that the hygrometer calibration had substantially shifted. We expected that, because the SF_6 coming through the purifier was pure, the integrity of the hygrometer head could be trusted. Resetting the calibration at maximum, 800 p.p.m. instead of greater than 1000 p.p.m., and re-installing the head after the purifier, resulted in a reading near 60 p.p.m. for the new pre-dried VIVALYME alumina mixture.

When the SF_6 was allowed to recirculate through the fresh purifier, the lost charge at 13 MV reduced from 100 microamps to less than 23 microamps in 7 hours. It reduced to 4 microamps 16 hours later. It is hard to believe these phenomena could be explained solely by wet SF_6. The evidence against moisture being the only culprit is that there was no corrosion seen in the tank at the last opening and that the charging efficiency did not fall. Both these have been diagnostic of excess moisture. As well, why should 10 hours of conditioning suddenly worsen the moisture level?

The alternative explanation is likewise unpalatable. It is that there are b.d.p.'s to which the soluble fluoride monitor is insensitive which causes lost charge. These poltergeist b.d.p.'s could break Marley's chains too.

Another hygrometer head, never having seen SF_6 or b.d.p.'s was used offline to check the SF_6. Its calibration was checked at greater than 1000 p.p.m. and its zero at less than 2 p.p.m. in a vacuum system. The SF_6 reading for the 14UD was 4 p.p.m. Unfortunately, one can't use these readings to confirm or contradict the moisture levels in the 14UD before the alumina was changed.

Robert Rathmell.

Robert's visit ended on 7th July. During his stay he used the machine, whenever it was idle for an hour, to make his own observations about voltage performance, stability, conditioning etc.

Our many discussions were punctuated, where feasible, with tests and measurements; where tests were not feasible, we simply argued. There is no doubt that, as well as being thoroughly enjoyable, Robert's visit was very successful. It gave rise to ideas which we intend to try out on both the source and the machine.

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The E.N. Tandem.

We have just heard that, late in June, the former A.N.U. Tandem emitted its first beam since arriving in New Zealand, 7 MeV protons.

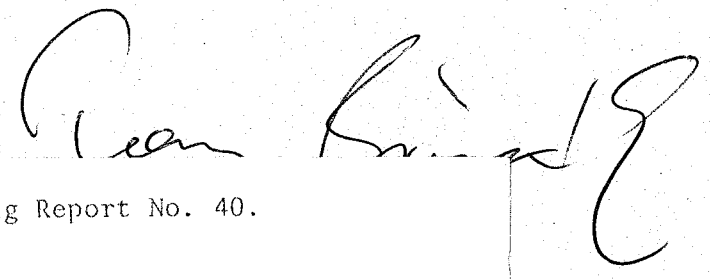
D.C. Weisser
T.A. Brinkley
20 July, 1983.

An expression of thanks from the older author.

Early in May I received, out of the blue, a kind communication about my coming retirement. The message was signed by more than 50 delegates who attended the 6th Tandem Conference which had just concluded in England. I was impressed that so many leading names in the world of accelerators were on the list and it was good to see, among the signatures, identical copies of some which we preserve on a white door in the darkroom.

Nothing could have pleased me more than to learn that the Tank Opening Reports successfully convey the spirit in which we write them: that amalgam of bewilderment and optimism, modulated by a perpetual conviction that something is about to go wrong - yet elevated at times to lyrical happiness when she, to whom we are slaves, performs at full volts for days on end. Only another accelerator man can ever understand.

Quite unable to think how to express my pleasure, I asked the younger author to suggest what I should say. He replied at once: "We make a point of not varnishing the truth in the reports, so just say it gave you a very swelled head and you pinned the letter up in your room where it would be seen by anyone who you hadn't already shown it to." I shall leave it to his wisdom and simply add: Thank you all, sincerely.



Post-script to Tank Opening Report No. 40.

terminal voltages.

The poltergeist (page 9) lives!

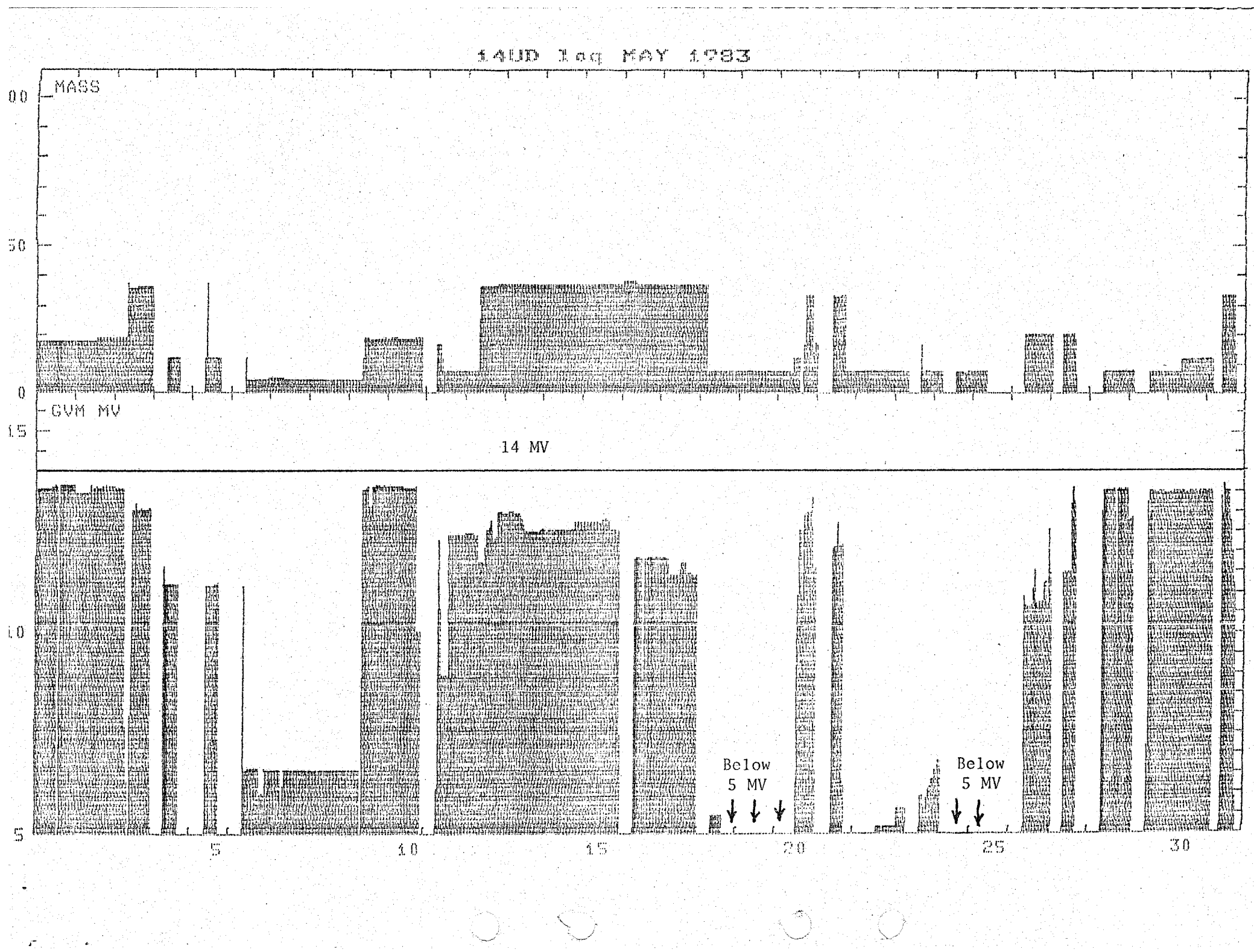
prizontal line at
voltage limit of

The alumina removed from the purifier was checked for water content by putting in a reactivation vessel and pumping on it. There was much less moisture liberated from this used alumina than from virgin alumina at room temp. From this we conclude that the SF_6 had been dry and therefore that the lost charge could have been due to the b.d.p. not detectable in the conductivity cell.

s below 5 MV; when

al spinnings.

July 22nd, 1983.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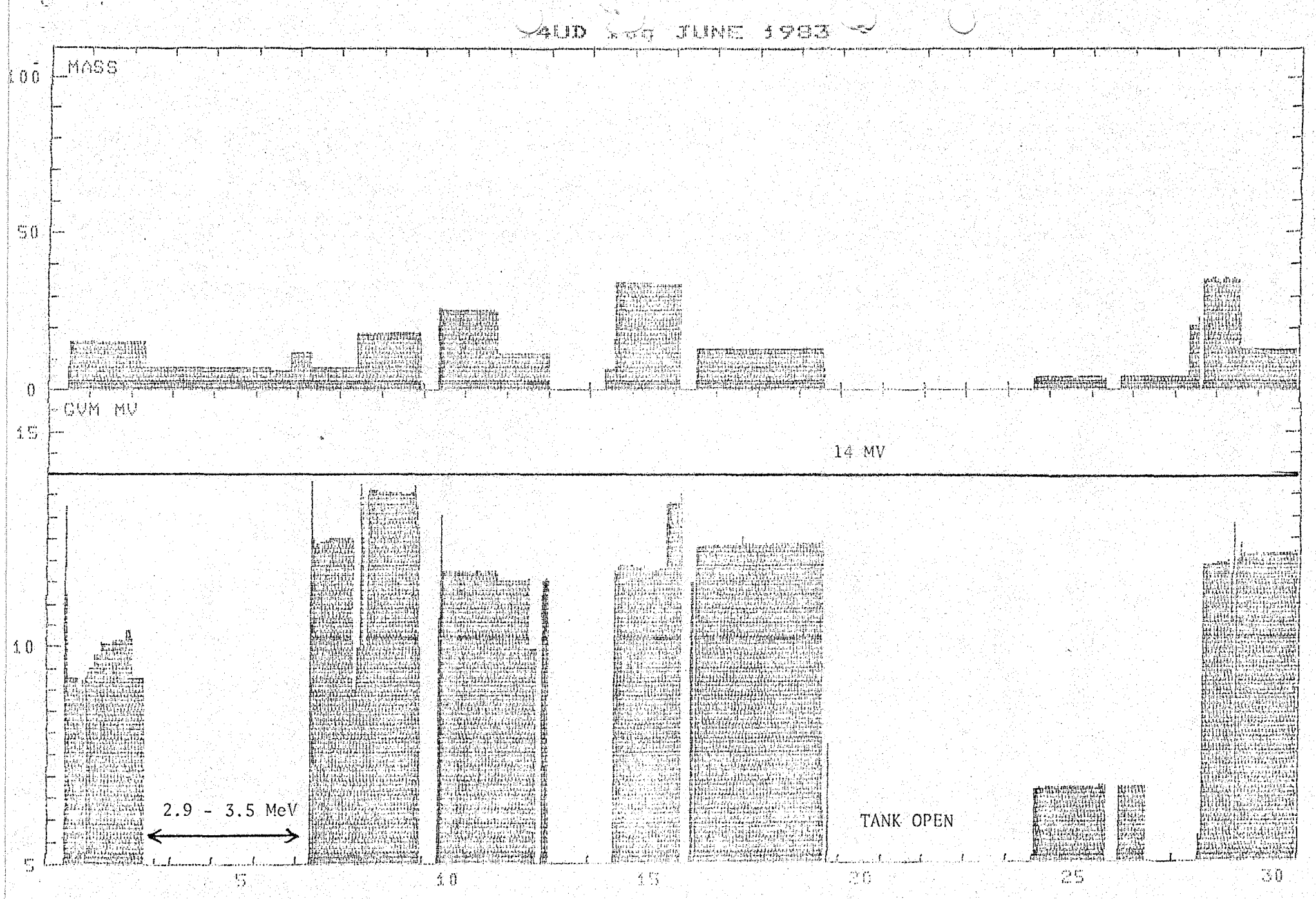
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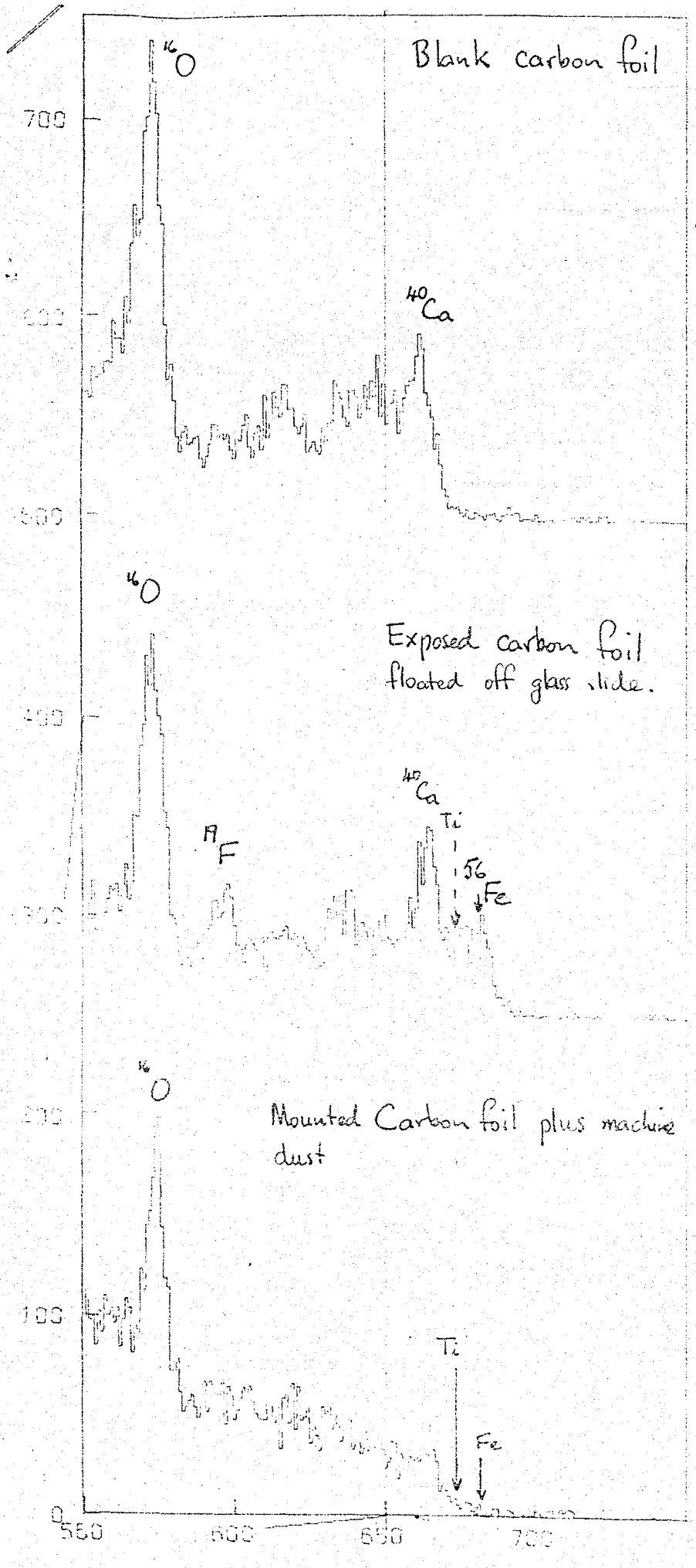

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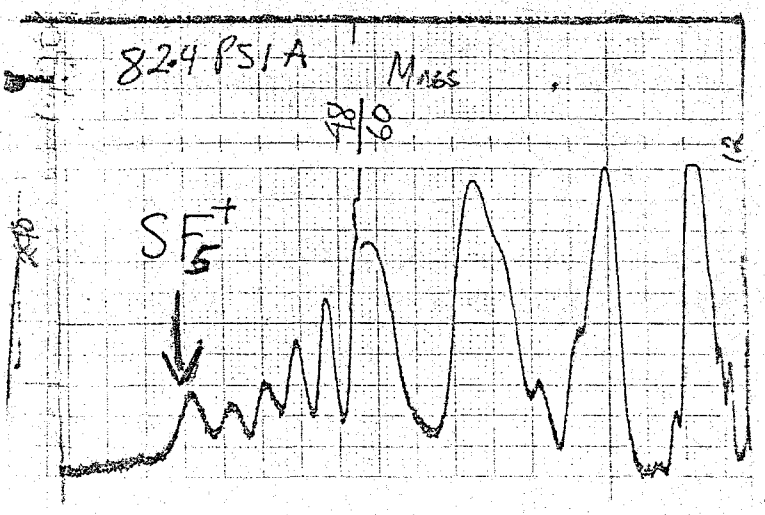
See Analysis 2

Level 4



Residual Gas Analysis

Low Energy Accelerator Tube

19th June 1983

The peak marked SF_5^+ disappeared as the SF_6 was pumped from the tank. It reappeared when the tank was gassed up. The other peaks, at somewhat lower masses, are historic evidence of hydrocarbons.