2005 April 13 Skience Forum

Friedhelm Ames, Andrei Andreyev, Rick Baartman, Gordon Ball (GCB), Pierre Bricault (PB), Lothar Buchmann, Jac Caggiano (JAC), Thomas Coccolios, John D'Auria (JDA), Barry Davids, Paul Delheij, Jens Dilling, Marik Dombsky (MD), Andy Hurst, Mark Huyse, Dave Hutcheon (DH), Peter Jackson, Rob Keifl (RK), Jens Lassen, Bob Laxdal (REL), Phil Levy, Andrew McFarlane (AMcF), Colin Morton (ACM), Chris Oullette, Chris Payne, Chris Pearson, Matt Pearson, Jean-Michel Poutissou (JMP), Chakrawarthy Ravuri, David Rosner, Mike Trinczek, Violetta Toma, Christof Vockenhuber (CV), Jim Waddington (JCW)

Notes transcribed by ACM and GH

Yield Measurements -- Colin Morton

The current target is new Ta Target #11, with 21.8 g/cm^2 Ta foils. Unlike previous designs, the Re ionizer foil in the transfer tube was replaced with Ir, a change that was expected to improve yields, esp. of Al. This was also the first use of the new yield station. Its new scintillators are designed to span three orders of magnitude of efficiencies, to provide the dynamic range for both the weakest and strongest ISAC beams. As this device only came together just in time for the real on-line experiment, the efficiencies of the scintillators were not fully understood, and this complicated the yield measurements.

The measurements focused on Li and Na. On Saturday, tracking from 10 to 40 μA of proton current, the ^{11}Li yield actually decreased at and beyond 35 μA. However, when the measurement was repeated on Sunday, the yields increased as had been seen on previous experiments. Nevertheless the yield was ~1/3 that of the best ^{11}Li production ever. Yields of Na were also low compared to previous targets. On Sunday night, 2x10^7 ^{8}Li/s was produced with 20 μA proton current and delivered to βNMR.

Ensuing discussion: ACM reported that in order to compensate for the beam profiles, the target heaters were appropriately adjusted. MD mentioned that there were no records available for previous targets, making direct comparisons difficult. ACM reported estimates Al yields. With 40 μA proton current, the gamma ray yield-station data indicated yields of 2x10^6, 2.9x10^4, 3.8x10^3 ^{29,30,31}Al/s, respectively. MD reported no previous Al data for comparison. Unusually small beam emittance was measured, but rumours of a smaller hole in the transfer tube were quickly and thoroughly quashed. It was noted that the proton beam halo was moving continuously, although the profile monitor showed that the central beam was stable. MD concluded by pointing out that Ir is *in principle* better than Re by a factor of two for ionizing Li, Na and Al, but that he really can't tell that for sure now. Furthermore, he pointed out that there may be nasty surface effects.

βNMR – Rob Kiefl

RK showed that the new β-NMR/NQR switching electrostatics were working properly, and asymmetry measurements were taken "simultaneously" at the β-NQR and low-field β-NMR stations. Furthermore, a new pulse-shaped RF system helped mitigate beam-rate instability problems by allowing the measurements to be made more quickly. AMcF pointed out that this RF system had been designed and built at TRIUMF.

E1024, ^{40}Ca(α,γ)^{44}Ti -- Christoff Vockenhuber

This experiment is planned in two phases. The first part was to focus on beam development, contamination, leaky beam suppression, and measuring strong resonances at E_x=9.22 MeV. One item to explore was which of the two existing ion sources would provide both the yield and cleanliness needed. This is especially challenging as the RFQ's M/q<30 limitiation means one

must accelerate doubly-charged $^{40}Ca^{2+}$. CV pointed out that absolute energy measurements after the RFQ at MEBT would be helpful in identifying beam components. The DTL's 3<M/q<6 specification limited that part of the chain to 6+ to 9+ charge states. Contamination from O, N, Ne at 1% level was reported. Because ED1 of DRAGON was limited to 200 kV, a foil was needed after the gas target to boost the charge state and get the recoils through. The two foils tried were 20 μg/cm^2 C and 17 μg/cm^2 SiN. At the end of the day, the experimenters were able to demonstrate leaky beam rejection from expected recoils at the 10^8 to 10^{10} level, depending on slit settings.

Heated discussion ensued. REL asked that experimenters provide detailed specifications of the size and divergence of the beam, although DH said that detailed specifications depended on too many things. Minimum intensities were discussed; 10^8 ^{40}Ca/s would be enough for the experiment. It also turned out that of the A=40 beam delivered, only 5% was ^{40}Ca. CV reported that DRAGON was ready to take beam for Phase II, in which the yields below E_x=9.22 MeV will be measured.

<u>^{23}Na implantation, Jac Caggiano</u>

In phase 1 of Experiment 1027, 23Na was implanted into various substrates and various depths using a rastered beam down the 8π beamline. The next step, Phase II, is to use the the 309 keV resonance to measure the implantation profile, either at DRAGON in late April, or at Seattle.

<u>Preparation for TIGRESS in-beam test: Jim Waddington</u>

In July, an in-beam test with the ^{23}Na(p,γ) reaction is planned for an "end-to-end" test of a TIGRESS module. This weeks beamtime will be used to commission the new HEGPS station and to test TiH_x targets and reactions. The targets for this test are 11 mg/cm^2 Ti foil loaded with H and a thin (100 μg/cm^2) TiH on Cu; the latter will only populate one state. The original 8π Chalk River target chamber, with thin Al wals and a four-position target wheel. JMP asked about target cooling. For this test there is none in place, and in the worst case H cooks out. REL asked about tuning; JCW described plan to tune onto an ISAC-standard Faraday cup, remove it, replace it with the target wheel and target chamber endcap, and observe the current on the wheel. RK asked if (how?) needs to know H concentration in target; JCW bases concentration on weight gained in the hydration process, but cannot confirm its presence ab initio.

<u>Other Business</u>

As Roger Miller did not receive the e-mail about the forum, he did not attend and could not present. RK noted that decision re future experiments was to be made and RM could comment at a future date.

REL referred to the technical review of TUDA experiment and asked if plans are afoot for tech reviews for stable beam experiments. He would like to see meetings ~ 1 month before experiment to address needs. PB supported this. JMP asked if the beam development group should chair this. PB indicated he is willing, and noted that this will allow coordination of development needs and allow early identification of dead ends. JDA requests such a meeting for Ca experiment at DRAGON.

Next Forum in two weeks. JMP will report on planning meeting.

GCB asked about draft schedule beyond July; JMP aiming for end of month.