Hydrogen impurities in mCap

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1 Introduction

In view of the statistics achieved in the recent MuCap run, we estimate the systematic errors introduced by hydrogen impurities and investigate possibilities to measure them.

Notations: All rates in this paper are given in units of μs^{-1}, capture yields Y, change of decay rate δr and concentrations c in ppm.

2 Z>1 Impurities

2.1 Capture yield and lifetime effect

Table 1: Main characteristics of common impurities. r_tr transfer rate in 10^5 μs^{-1} at LH_2 density, r_cap capture rate, x=r_tr*ϕ c/r0 (ppm), y=r_cap /(r0+r_cap), Y=x*y capture yield in ppm, δr relative change of observed decay rate and c impurity concentration.

element	r_tr	r_cap	x	y	Y @ c=1	dr	del_r/Y	c @ Y=10
C	0.4	0.0376	879.12	0.076	67.103	120.27	1.792	0.149
N	0.34	0.0693	747.25	0.132	98.769	164.29	1.663	0.101
O	0.85	0.102	1868.13	0.183	342.100	533.54	1.560	0.029
Ne	0.08	0.235	175.82	0.341	59.882	77.99	1.302	0.167

Note the following:

- References: capture [1], transfer[2] [3] [4]
- δr was calculated according to my study e-log 384. In particular the approximation d**r= x y (2+y)/(1+y)**^{2} was used here. As mentioned there this expression depends on the fit range and should be confirmed by direct Monte Carlo studies.
- According to Ref.3 there might be an additional epithermal component to transfer on O_2. According to my simple estimate from the above mentioned paper this would increase Y by ~4.6! This means that the effect of impurities could be ~5 times larger and have a different time dependence than estimated from a steady state thermal model. We don't know if that is happening for N_2 as well. Correction: Fortunately epithermal only relevant for high c, see purity_nb.pdf.
- Without the epithermal enhancement $Y \sim 10^{-5}$ corresponds to $c_O \sim 0.03$ or $c_N \sim 0.1$. The effect on the lifetime is ~16 ppm as compared to a statistical uncertainty of 30 ppm (for 10^9 statistics).
- In order to have $Y \sim 10^{-6}$ $c_O \sim 0.003$ and $c_N \sim 0.01$ is required.

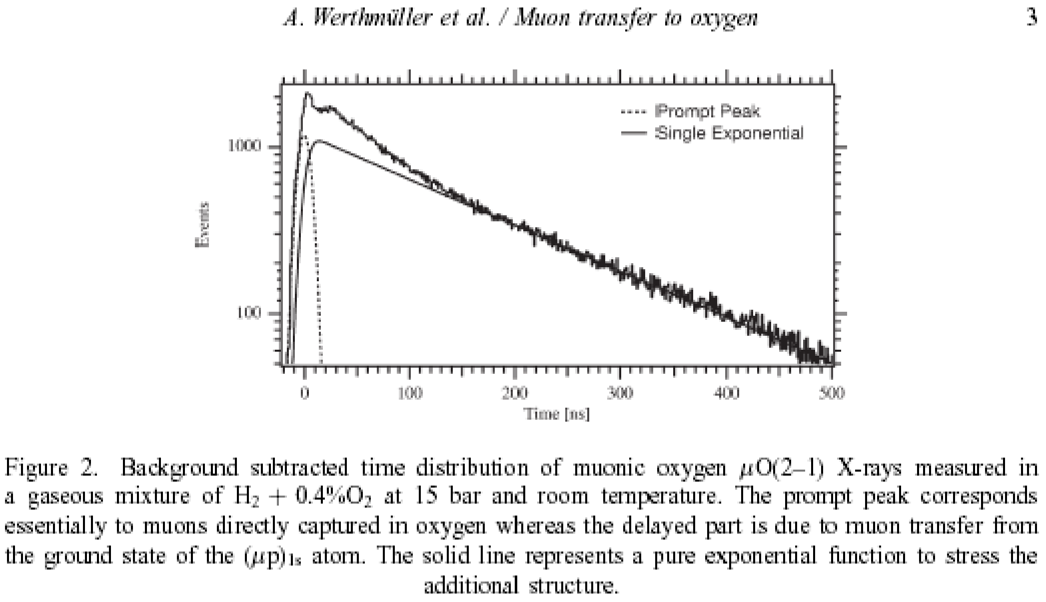

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Figure 2. Background subtracted time distribution of muonic oxygen μO(2–1) X-rays measured in a gaseous mixture of $H_2 + 0.4\% O_2$ at 15 bar and room temperature. The prompt peak corresponds essentially to muons directly captured in oxygen whereas the delayed part is due to muon transfer from the ground state of the $(\mu p)_{1s}$ atom. The solid line represents a pure exponential function to stress the additional structure.

Figure 1: From ref.3

2.2 How to measure and correct this effect

Fig. 2 shows the recent analysis of Tom and Claude, Oleg finds consistent results.

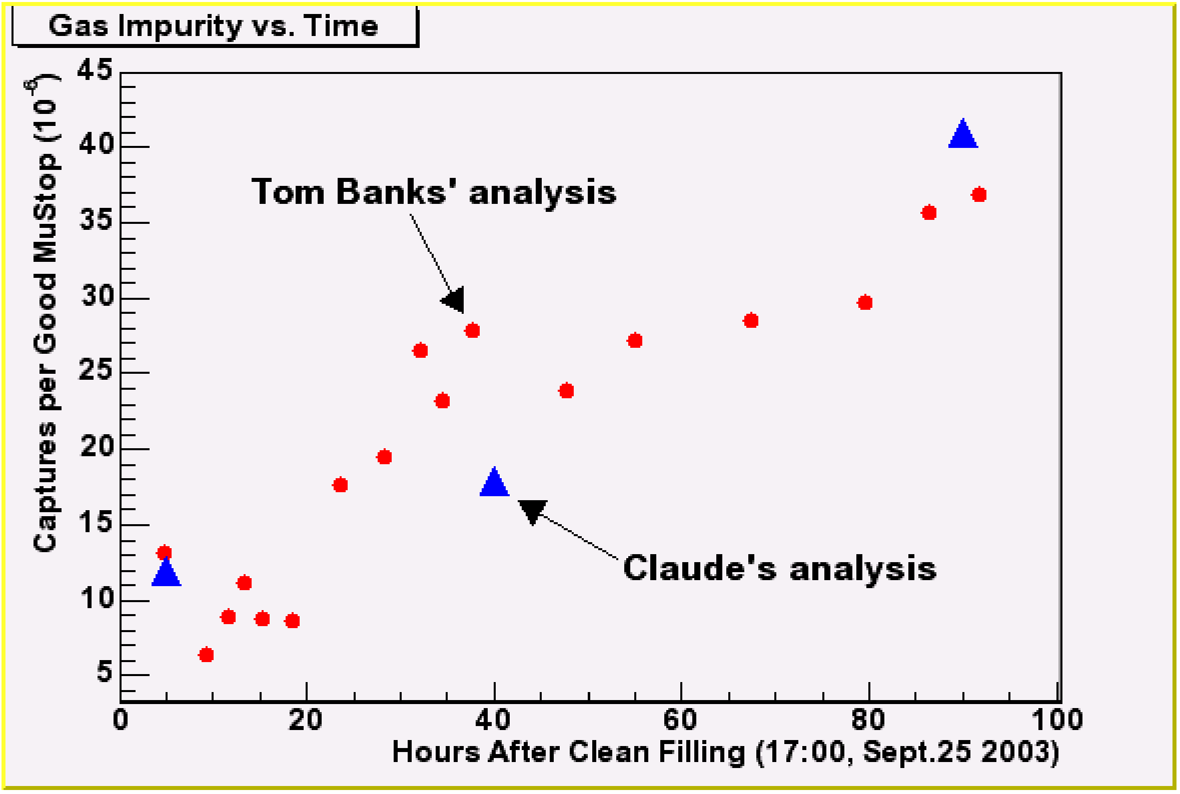

Figure 2: MuCap capture data

The experimental yield Ye=ε Y, where ε is the detection efficiency. Assuming ε~0.5 we observe Y rising from ~20 to 100 ppm within a week.

For the future better baking and gas recirculation is necessary. But what can we do now and in the future to correct for these impurity effects.

Correction strategies:

1. Without correction. Assume we achieve c_N<0.01 ppm in the future, as determined by chromatography. That would be only sufficient for a 10 ppm capture measurement, if we can demonstrate c_O<0.003. Difficult, but not impossible requirements. In particular, the H_2O sensitivity will be a challenge. We need to study whether the TPC signals allow us to discriminate between the contamination elements. At first sight that will be difficult, but maybe N break-up stars might help.

	C	**N**	**O**	**Ne**	**Si**	**Ar**
r_cap	0.0379(5)	0.066(4)	0.1025(10)	0.231(10)	0.8712(18)	1.270(80)
y(%)	7.69(9)	12.7(6)	18.4(2)	33.7(10)	65.87(4)	73.8(12)
capture modes (%)						
to bound final state	18.6(7)	9(2)	11(1)	~15	26(3)	~15
with 1 neutron emission	50	47(8)	66	55	46	57
with 2 neutron emission	18	31(8)	10	10	13	20
with charged part emiss.	13(2)	13(2)	13(2)	20(4)	15(2)	8
E_{rec} (assume 85 MeV/c)	0.35	0.30	0.26	0.20	0.14	0.10

Table 2: from Dave Measday

2. With correction: If we need to correct, there again are two different strategies. Assume we achieve c<0.01 ppm overall.
    a. If we know the concentrations to 10% then we need to calibrate the effect of the impurities to 10%, because of possible uncertainties in transfer rates. Again the H_2O problem is difficult for chromatography.
    b. The relation between δr and experimental yield can be expressed as
    d**r = Ye f(y_Z)** k_Z / e_Z
    Both Ye and δr scale linear with x, because it is a small parameter. Here f(y_Z) ~(2+y)/$(1+y)^2$ changes only by 6% between O and N, i.e. is nearly independent of the exact composition. κ_Z is a correction for the difference in the δr/Ye relation due to epithermal effects, hopefully small as well (but needs to be studied). ε_Z characterizes the capture detection efficiency for different impurities, should also be similar for O and N, again needs confirmation.

What can we do for the 2003 data:

The point of argument 2b is that the relation between δr and Ye is constant within 10% for common impurities C,N and O, d**r = K Ye.** If this holds, we need to determine this constant and then can extrapolate to zero Ye. For this method an impurity point is required, where mu capture on the proton is a small correction.

E.g. if we add 20 ppm of N_2 Y=45x50= 2250 ppm. The corresponding δr= 3735 ppm, i.e. 3x larger than μp capture. If we assume a conservative error of 20% in the capture rate we can determine K with 6% error. If K is independent of Z within 10%, we can correct our presently measured decay rate. Assume that the correction is of the order of 100 ppm, it error would then be ~16 ppm, sufficient for our present data. Such a high N_2 point will also give us clear signature of the capture topology for N_2. In order to determine δr to 6%, a statistical precision of 2250 ppm * 0.06= 224 ppm is required. This needs $2x10^7$ events or 1-2 shifts. How to achieve a 20 ppm filling. We would need to fill a clean one or less liter volume with ~10 mbar. 10 mb liter/400 bar liter=2.510^{-5}.

On second thought, is this extra point necessary? We have already observed a significant change of purity (see fig.2). For a simple linear relation y=a + b x, I find (please check)

$$\Delta a= \Delta y_2\ x1/(x1-x2)$$

where only the uncertainty of the second y point has been included. As our x points are already separated by a lever arm of about $x_2/x_1=5$, we get $\Delta a=1/4\ \Delta y_2$. But we would use precious statistics for that. The complete problem should be worked out more carefully. What is the optimal concentration for an additional impurity calibration point?

A related question is whether we can use previous fillings with higher impurities for this calibration. The problem then lies in the stability of ε_Z, are we sure that has not changed with all the electronics changes? For the same reason it is questionable to use high levels of water, because the signals might change. Probably the data from Sep.19-24 is ok and corresponds to a 10 fold higher contamination. Still a well defined measurement with controlled N2 would be very valuable to establish the method and compare to the more complicated Sep.19-24 situation. This could also check the basic assumptions mentioned in 2b.

3 Deuterium impurities

Apart from cranking up the TPC so that Alvarez muons can be observed, I don't see a supplemental measurement right now. We have to try to extract that from the data (time dependence, spatial cuts etc., contact Valery conc his program). Moreover, clearly it is critical to provide good samples for ETH and PNPI.

Adding a well-defined amount of deuterium does not seem attractive, as long as we lack a direct deuterium monitor.

Let us look at the chances of detecting mu+He. Some basic consideration:

Statistics

Cd (ppm)	**N(mu+He)/10^8 good mu**	**Days**
10	2000	<1 day

However, the detection efficiency could be ~0.5 and we need to collect mu+ as well.

Observation

	mu stopping in sec E	**Fusion mu**	**mips**
E (MeV at TPC entrance)	2.4	5.4	
dE/dx (keV/(mg/cm2)	11.3	22.5	2
P_mu (MeV/c) required to simulate	34	42 Ag to increase scattering	
TPC HV (kV)?	5	5.2	6

In conclusion, if we can keep the TPC at 5.2, we might try to see 2.4 MeV muons. If we see them, we could try to run 1.5 days to find the signal or at least determine our sensitivity. That is interesting because

1. ~5 ppm sensitivity should be sufficient for the present statistics
2. it would be an in-situ calibration
3. it is one of our best bets for the future
4. it could show the value of FADC over TDC analysis

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[1] T. Suzuki et al., Phys. Rev C35 (1987) 2212

[2] S.S. Gershtein and L.I. Ponomarev, Muon Physics III, p. 164

[3] A. Werthmueller et al., Hyperfine Interaction 116 (1998) 1

[4] H. Schneuwly et al., Muonic Atoms and Molecules, Ascona Workshop 1993, p. 212