NARROW ^{87}Rb and ^{133}Cs HYPERFINE TRANSITIONS IN EVACUATED WALL-COATED CELLS

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ABSTRACT

An extension of our work on wall-coated cells has been made to include observation by a triple resonance technique of the 0-0 hyperfine transitions in ^{87}Rb and ^{133}Cs. Conventional rf excited lamps were used. Interest in such cells is for possible application in atomic clocks. The Rb cell would appear to remain especially promising in this respect.

INTRODUCTION

We have previously reported[1] observation of ^{87}Rb $|\Delta m_F| = 1$ hyperfine transitions in a 200 cm^3, evaucated wall-coated cell. The narrow Lorentzian component of the lineshape has a width of $\sim$ 11 Hz, FWHM, giving a $Q \sim 0.65 \times 10^9$ for the resonance. Interest in the $(F,m_F) = (2,0) \leftrightarrow (1,0)$ hyperfine transition for potential use in Rb frequency standards has focused our attention toward exploring the characteristics of this "clock" transition.

EXPERIMENT AND RESULTS

The apparatus available uses a conventional rf plasma excited Rb lamp which, after filtering, produces circularly polarized D1 light. The cell is placed in a shielded solenoid producing a 1.5G magnetic field. The use of the σ(+ or -) radiation permits observation of $|\Delta m_F| = 1$ hyperfine transitions by monitoring the intensity of the transmitted light. This detection scheme relies on a change in $\langle S_z \rangle$, the z-component of the electron spin. Where the population is pumped toward the (2,2) level, the largest signals correspond to the $(2,2) \leftrightarrow (2,1)$ Zeeman transition and the $(2,2) \leftrightarrow (1,1)$ hyperfine transition. However, since the change in $\langle S_z \rangle$ is zero for the 0-0 hf transition, the method is not suited for direct observation of this resonance. Nevertheless, by exciting the Zeeman transitions $(2,2) \leftrightarrow (2,1)$ and $(2,1) \leftrightarrow (2,0)$, we can detect a change in the (2,0) population caused by a 0-0 transition. This triple resonance scheme was employed to obtain initial data on the clock transition in the sealed, evacuated wall-coated Rb cell.

Linewidths as narrow as 9 Hz FWHM are observed for the 0-0 resonance. Figure 1 displays such an observation having a 13 Hz linewidth with a Lorentzian lineshape function fitted to the data. The symmetry of the resonance is a sensitive function of the tuning of the two Zeeman resonance drives. The observed wall shift of the hyperfine frequency due to atom-wall interaction is - 52 Hz at 26 °C. Both width and wall shift are consistent with our previously reported[1] observations made on the $(2,|2|)\leftrightarrow(1,|1|)$ hf transitions.

A 100 cm^3 sealed, evacuated wall-coated cell was also available for ^{133}Cs. We repeated the above procedure with the Cs cell using a conventional rf driven Cs lamp as the pumping source. The linewidth extrapolated to zero light and rf intensities is $\sim$ 101 Hz. The wall shift observed is - 180 Hz at 26 °C. Upon cooling the Cs reservoir to 5.5 °C and heating the cell wall to 50 °C we obtained a wall shift of - 160 Hz for the $(4,|4|)\leftrightarrow(3,|3|)$ transitions.

These initial results leave unanswered questions of aging, retraceability, and whether substantially better evacuated wall-coated cells can be fabricated. However, the observation of ^{87}Rb hf resonance with $Q \sim 6.5\times10^8$ in a sealed, evacuated wall-coated cell shows a potential for use of this type of cell in an atomic frequency standard. We are pursuing pumping/detection schemes permitting efficient direct observation of the 0-0 transition using laser diodes.

REFERENCES

1. H.G. Robinson and C.E. Johnson, Appl. Phys. Lett. 40, 771 (1982).

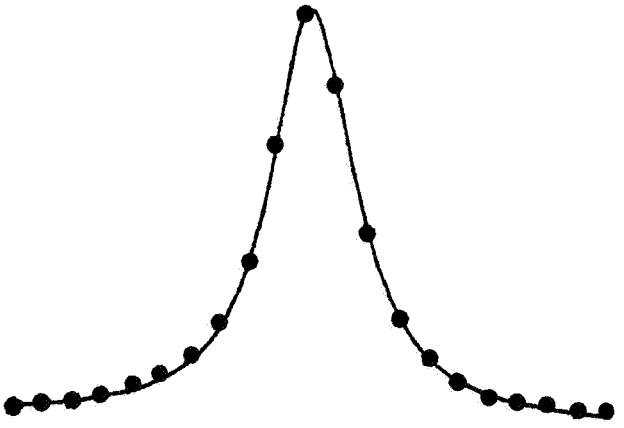

Figure 1. A 0-0 hf transition in ^{87}Rb with a linewidth of 13 Hz. The solid curve is a Lorentzian function fitted to the data points. Observation is by a triple resonance technique.

QUESTIONS AND ANSWERS

PROFESSOR JACQUES VANIER, Quebec, Canada

What kind of a coating material do you use in your cell?

MR. ROBINSON, Duke University

The material that's used here is, we believe, is TETROCONTAIN. These cells were built at a very chaotic time and the record keeping on these cells is not all it should be. But, we could tell, we could test that by taking the cell up to where we would melt the surface wax, find out unequivocably, but, we'd rather not do that, until we have played with the cells a little longer.

MR. DEHMELT:

Do you have an estimate of the optic frequency Zeeman for the transition in the rubidium cases? It varies -- more than I know.

MR. ROBINSON:

The line width of the zero-zero transition is very insensitive to the amount of power put on the Zeeman. The Zeeman line width is considerably in excess by broadening of RF, due to the transition due to the Zeeman transition power.

In other words, I would say the Rabi frequency is larger than 9 hertz for the Zeeman transition. We haven't understood that yet, but we have not worked with understanding it either, the experimental fact is that the zero-zero transition line is very insensitive to the amount of Zeeman power, you put on. The amplitude is sensitive. The amplitude is very sensitive, but the line -- it may be an Autler (?) -Townes situation.

AUDIENCE:

Yes. What do you think is the dominant contribution to the line width in these transitions you're seeing?

MR. ROBINSON:

The dominant contribution of the line width, we were able to, essentially, write a line width budget for the case of the Delta M-1 transition, and the dominant contribution, by far, is the effect on the wall. While it sits on the wall, there is a dispersion in the phase shift that you get. Each attack of the wall of the atom does not give you exactly the same phase shift as the random walk in phase. And it's that dispersion that is the dominant cause.

Out of that ten cycles for instance, at least six cycles worth of it is due to the random nature of this phase shift on the wall itself.

So, the wall is the dominant cause of the line width. The light intensity that we've used to take these resonances is on the order of, 1 1/2 microamps. That issue came up yesterday -- and I don't know how many microwatts per square centimeter that is, but that's the transmitted light intensity. It's a very, very low light intensity, so we don't get very much light broadening.

Now, we could go down a whole list of potential sources of line width, and they all reasonably add up. So, we have looked at that budget, but the dominant one by far is the wall itself.

DR. KELLOGG:

Do you have any plans to go the other way in the temperature spectrum? Maybe cooling the wall before you risk melting the wax?

MR. ROBINSON:

Well, theoretically, it looks like it's better to heat the wax. The lifetime, the time the atom sits on the wall, is related to the absorption energy.

What you want to do is to heat the wall up and, in fact, for the cesium case, we heated the wall up to about 55°C, and the line width narrows as you do this.

The wall shift decreases as you do this, or rather, the magnitude of the wall shift decreases. So, things get better as you heat the wall, until you reach a point where, perhaps, the wax begins to melt or change character, and then it turns around and goes the other way.

But, in fact, it looks like it's better to heat the wall in this particular case than, than to cool it.

These cells aren't expensive to produce. I don't know what -- how you count production costs, and whatnot, but there's just a trivial amount of wax invovled in them, and glassblowing time and so forth, so the cost is not really significant, I think.

AUDIENCE:

I would like to ask one question, please. About the insensitivity to magnetic field gradient. I believe that is true if you use a high magnetic field, but like in a hydrogen maser, when you go to lower fields, that is when used for frequency standards, you start to have some trouble with the Zeeman frequency. If it is of the magnitude of the collision frequency of the atoms, then you start to loose signal and you have a shift in the frequency of the line. So the insensitivity to magnetic gradient is true only for high magnetic field and not for low field.

MR. ROBINSON:

I'm not sure I understood all that, but you're commenting on the fact that we have 1 1/2 gauss field and we did that in particular to resolve all the Zeeman transitions so that we could individually diagnose things, see what was going on.

The Zeeman line width is about 2 1/2 cycles in these cells. There's no reason why we couldn't reduce that field as far as we see. It's -- we understand how the averaging process for inhomogeneity occurs, and there should be no problem at all in reducing the field with respect to that particular parameter. So I don't see any, just offhand, any problem in going down to very small fields. There's no problem with optical pumping as I understand it and going to essentially zero fields as long as we have some direction for the Z direction for the system.

AUDIENCE:

What I mean is, is it possible to go to a lower field? Also what about the uniformity of the RF field?

(QESTION NOT TRANSLATABLE IN THE RECORDING)

MR. ROBINSON:

The wall shift for the Delta M-1 transition was a much cleaner situation. And that was also -52 Hz. And we expect theoretically that the two wall shifts should be the same, to, within some small discrepancy. So the fact that we confirmed that by this very obtuse triple resonance technique I think is a good sign. It just means that we believe that result.

AUDIENCE:

Is there any plan to repeat the experiment with fresh cells?

MR. ROBINSON:

Yes, we need to do that. It's clear that there's some critical questions that arise as to how to get the cell to stabilize, will the wall shift itself drift forever, will it stabilize in a week or a month or two days, or just what. Same thing for the line width. Retraceability if you heat and cool these things. What happens?

We'd like to do some of these things. It just takes research time that we haven't managed to get.

AUDIENCE:

Was there any data over this ten year period as to how much the wall shift changed?

MR. ROBINSON:

No, our initial experimental use of these cells was in shimming magnetic fields to look at magnetic moments. We have a history of looking at G factors in atoms, and in fact, we were just not interested in the hyperfine transitions in these cells. So we never even looked at the hyperfine transition when they were originally made. That was very unfortunate but that was the case.

They're extremely useful in tuning away magnetic field gradients, because the averaging process gives you a measure of the magnetic field at a point, mainly the very center of the sphere, so it's actually measuring the magnetic field literally over a volume in one sense, but the average is legitimately mathematically that of a point in the center of the cell.

And not only that, but it produces essentially a Lorentzian line shape, so that if you have magnetic field inhomogeneity shims which we have, you can tune each one and it uncouples them. So that essentially there's only one pass. It makes all of these knobs orthogonal. So this is our use for such cells and we just didn't have the foresignt to look at these things.

It's clear that that needs to be done now. That is one of the main things if you look at the long-term drift of the wall shift.

AUDIENCE:

Do you know why the line width of the cesium was approximately ten times that for rubidium?

MR. ROBINSON:

There -- let's see, I think in principal from a theoretical point of view, you expect the cesium to be much worse because it has a higher polarizability. It's a much softer atom and we haven't put numbers into that. That would be a nice thing to do, to see if we could actually give a back of the envelope calculation to show that. Whether that's the factor that one would expect, I'm not certain. But that's certainly one reason.

A second thing is that we don't have enough cells to know whether the coatings that we get are reproducible. That is, from one thing to another These are one of a kind cells, and it may be that the cesium cell just simply isn't as well coated.

The third thing is that the cesium has a much higher vapor pressure at room temperature, as you know. Some of our data here was taken with a 5°C cesium stem. We saw visible traces of cesium all over the external arms of the cell. That is, it had moved around over the time of the cell, so that it left its original reservoir and migrated through the cell and out into some of the other arms that we have on this cell.

And, it could be that that wall was damaged, or actually had some cesium sitting on it. We actually noticed an aging, or de-aging if you like, process over a period of maybe ten hours when we cooled the cesium cell. We didn't notice that for the rubidium.

But, we just don't know the answer to the question and there's some good theoretical reasons why it could have happened. And I will first go to this, to the polarized stability of the atom. I think that's the most likely thing.