

Re: An Explanation of Dayton Miller's Anomalous Ether Drift Result

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Tom Roberts ?8A0;(0):

Sergey Karavashkin wrote:

Tom Roberts ?8A0;(0):

....

An upper limit on absolute motion of 6 km/sec is derived from his raw data, fully consistent with similar experimental results and the prediction of Special Relativity.

Dear Tom, I'm so pleased to see that you already confirm Miller's result not zero but 6 km/s

Obviously you did not read the paper. The 6 km is an UPPER BOUND AT THE 90% CONFIDENCE LEVEL. Indeed, the actual value obtained by the analysis is PRECISELY zero, but there is an errorbar on that result leading to the upper bound.

But you are wrong, of course, that SRT predicts it.

Not true; in the limit in which the rotations of earth and interferometer can be neglected, SR predicts a truly null result. Taking them into account one can show their effects are enormously less than the errorbar of 0.015 fringe. Indeed, even vastly more sensitive measurements like Brillat and Hall's cannot see the predicted effects of the rotations.

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Unfortunately, in the space-time contraction, according to the Fitzgerald hypothesis which Lorentz and Einstein used in their interpretations, the result must be strongly zero.

Hmmm. You seem to be saying two contradictory things. In any case, it is SR on which modern physics is based, not the Lorentz-Fitzgerald hypothesis.

Predicting some, even little, speed with respect to aether, relativists have to admit the motion with respect to some absolute reference frame which is strongly rejected by Relativity.

You need to learn what relativity actually says, instead of your clearly wrong guesses.

Though I much disagree with your manner to analyse, but if even with all your manipulations with data you had to admit the same value of speed as Miller claimed, this in the best way evidences Miller's results stable.

NOT TRUE AT ALL. Miller reported non-null results, and my re-analysis CLEARLY shows there is no signal at all, and the values of Figure 11 for the reasonably stable runs are IDENTICALLY ZERO.

You need to actually READ THE PAPER.

Tom Roberts

I have looked through your paper yesterday. It is written vaguely, no one to be able to understand, what you have subtracted from what:

<< Therefore if the data from the first turn is subtracted marker-by-marker from the data of every turn, the result is completely independent of any orientation dependence, and contains only systematic(time). >>

The matter is, the data of first turn contained errors and masking effects the same as others. Subtracting, you on one hand as if selected the systematic error, but on the other you introduced the error of the first turn into the data for all next, which could sufficiently change your result.

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Similarly, suggesting

<< It is convenient to take advantage of the 180 degree symmetry of the apparatus, and combine the data for markers 180 degrees apart. This gives 8 orientations, and 8 independent measurements of differences in systematic(time), shown in Fig. 10 >>,

you proceed from the idea that Miller had to obtain ideal characteristics. Factually such symmetry is basically impossible, the more that the degrees of turn have been measured not with the accuracy you would need. This is also a systematic error which you involved in your calculation, adding it from row to row.

I could continue but will not.

Miller was in this sense more correct than you are. He handled the data of each line separately in supposition that the full-turn effect is linear (which is seen in your Fig. 10). So he avoided to transfer the systematic errors from line to line. After such handling he already might average the results without systematic errors which you introduced to your computation.

Though I can say, you have extracted the systematic related to the full-turn effect with an outward elegance. But Miller also selected the systematic though outwardly not so elegant but more correctly than you did.

I would also mark, you could blame Miller that he flawed his experiment, should he, like Michelson, simply handle raw data as they are. But Miller accounted the full-turn effect maybe in his own way, and you account the full-turn effect in your own (and both ways give comparable results). So from this point it hardly is correct when you say, Miller flawed. As I showed above, your technique also can be criticised: you well know, all techniques of data handling are imperfect, especially if the signal is at the level of confidential interval. And spectral methods can introduce here no less error than a simple arithmetic averaging. Because the unavoidable errors of experiment, as well as temperature variations of which you and Shankland say, naturally introduce their spectral distortions that do not relate to the full-turn effect. Relying namely on the spectral technique, one can introduce more error than in usual arithmetic average where such effects are known to have the property of effective mutual compensation.

As to your

<< The 6 km is an UPPER BOUND AT THE 90% CONFIDENCE LEVEL >>

I would say, first, you have undervalued the data. Miller's speed not your 6 but 9,3 km/s has been derived from same raw data which you used by way of compensation of the full-turn effect. These are all

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100 % of the confidence interval and even more. Second, even your confidence level of 90 % is even too good from the view of relativistic experiments. :) LIGO expected to detect the gravity waves at the 10–15 % of confidence, and no claims from relativists. The same in other relativistic experiments. So these 90 % are too much from the point of relativists, and it is not worthy to doubt, Miller has obtained positive result. But I said you in that old discussion and am repeating now: we may not consider Miller's results quantitatively only qualitatively, as the fact of aetherial wind existence. To study aetherial wind, the more to obtain trustworthy data of its value and direction, we need other experiments that have nothing in common with the interferometric scheme and would distinctively register the phenomenon. These have to be the first-order experiments without masking effects inherent in the interferometric scheme.

Concerning, whether SRT predicts zero or non-zero effect, could you relativists first agree among yourselves, and re-read Einstein where he referred to the negative result of MMX. :)

And as to zero in average. If we sum any sinusoid over a period, you know it will give zero. Thus, is it worthy to average in Michelson's style over the hospital. When the chief doctor is asked, how his patients are, he answers: a half lies in fever and another is already dead and cold, so the average temperature over the hospital is 36,6 Celsius degrees and they in average are ok. :) Your handled curves show a clear periodic regularity. It is not worthy to turn things inside out.

Sergey

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