Measurement News

July 1996 Issue #78

1996 OLYMPIC MARATHON MEASURING TEAM

Bob Baumel, Andy Beach, Bernard Conway (CAN), Elaine Cornwell, Woody Cornwell, Dave Cundy (AUS), Jean-Francois Delasalle (FRA), Christian Delerue (FRA), John Disley (GBR), Julia Emmons, Rodolfo Martinez Figueroa (MEX), Luciano Ramirez Gallardo (MEX), Jean-Marie Grall (FRA), Barb Grass, Bill Grass, Jack Grosko, Shannon Grosko, Phil Henson, Scott Hubbard, Hugh Jones (GBR), David Katz, Ryan Lamppa, Dusty Lewis (AUS), Doug Loeffler, Isabelle Marechal (FRA), Mary Anne McBrayer, Tom McBrayer, Wayne Nicoll, Ed Prytherch, Gerry Rahill, Pete Riegel, Fran Seton (AUS), Don Shepan, Donna Valaitis (CAN), Mike Wickiser, Jay Wight, Norrie Williamson (GBR), Bob Woods, Dave Yaeger (CAN) (Not everybody is in the picture)

The final report is being prepared. See some tidbits inside.

MEASUREMENT NEWS
#78 - July 1996

* * * * * * * * *

MEASUREMENT OF THE 1996 OLYMPIC MARATHON

After years of preparation, the group measurement of the 1996 Olympic Marathon took place on Memorial Day weekend in Atlanta. 28 bicycle measurers from six countries took part, as well as several helpers whose assistance was as important as the riders'. On the first day, Saturday, we measured two calibration courses and the routes within the stadium. On Sunday we formed a long snake which, with excellent police protection, wound its way through the city and obtained measurements of the entire course outside the stadium. Everybody went home with complete data for the whole thing, and they are presently in the process of sending me material to be included in the final report.

The course (GA 96012 WC) passed its pre-race validation, and was shortened as a result of the validation. The final report is presently being assembled. It will be sent to all on the measurement team. As it is a fat report, it will cost something to print. My present estimate of the printing cost is $15 per copy.

Articles based on the measurement, and exerpts from the final report, will be appearing in MN over the next year.

Do you want a copy of the report?
If you want a copy, send me $15. I won't be sending out free copies. The demand will be high, and RRTC can't afford to support free distribution. I guarantee it will make interesting reading.

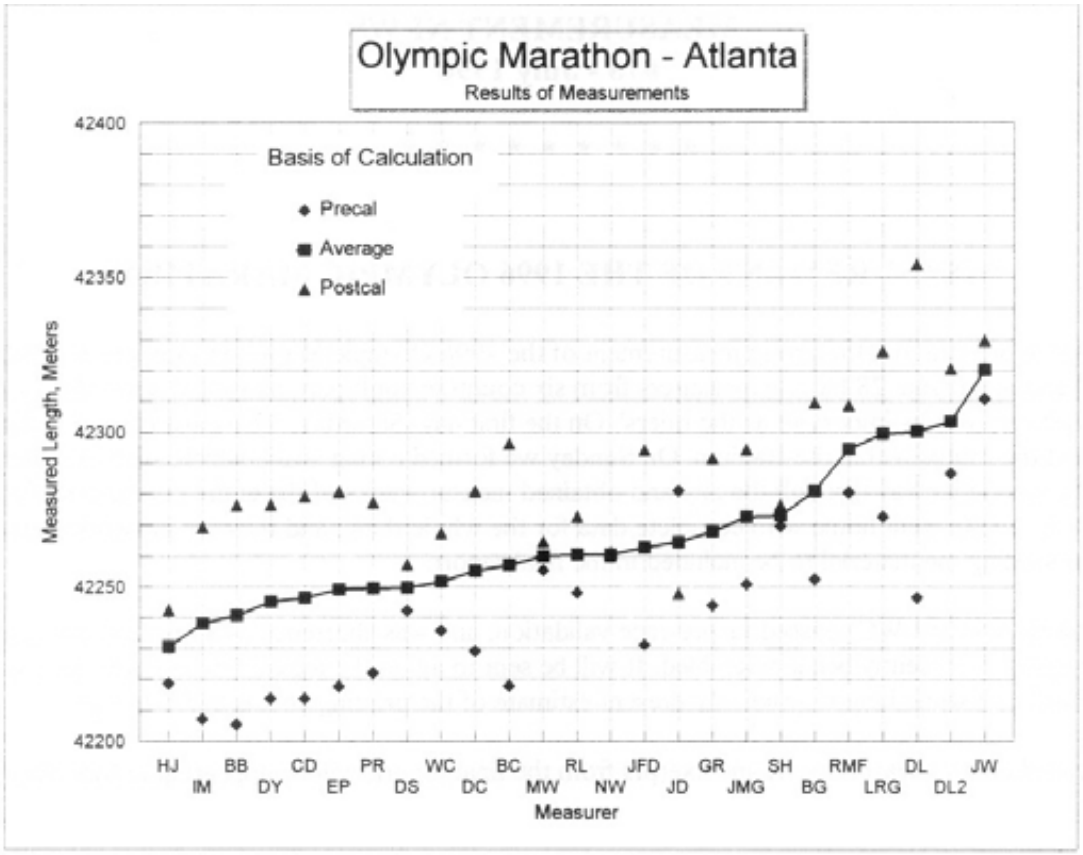
How long is the course? What adjustment should be made?
You will see a summary of the measurements obtained elsewhere in this issue.

PUZZLE OF THE MONTH

Based on what you see of the measurement, what do you think should be the final adjustment to the Olympic Marathon course? Why do you think your adjustment is correct? The actual adjustment will be revealed in the next issue.

WHEN THE GOING GETS TOUGH, THE TOUGH GET GOING

Tadeusz Dziekonski sent me the results of a marathon measurement he did in Wroclaw, Poland. I did not have to read his report more than once to be awfully glad I was not the measurer in his position. It was a measurement from hell. He was faced with frustrations that would have defeated many of us, but he emerged OK from his situation. It's one of the more extreme examples of grace and competence under pressure. See exerpts from his report elsewhere in this issue.



Values below include 1.001 SCPF

Measurer		Code	Length by Larger	Length by Precal	Length by Average	Length by Postcal	Range, m
Hugh	Jones	HJ	42218.8	42218.8	42230.7	42242.5	23.7
Isabelle	Marechal	IM	42207.2	42207.2	42238.2	42269.3	62.1
Bob	Baumel	BB	42205.5	42205.5	42240.9	42276.4	70.9
Dave	Yaeger	DY	42213.9	42213.9	42245.2	42276.5	62.6
Christian	Delerue	CD	42213.9	42213.9	42246.5	42279.2	65.3
Ed	Prytherch	EP	42217.8	42217.8	42249.2	42280.6	62.8
Pete	Riegel	PR	42222.2	42222.2	42249.7	42277.2	55.1
Don	Shepan	DS	42242.4	42242.4	42249.9	42257.4	15.0
Woody	Cornwell	WC	42235.9	42235.9	42251.6	42267.3	31.4
Dave	Cundy	DC	42229.3	42229.3	42255.1	42280.9	51.6
Bernie	Conway	BC	42218.0	42218.0	42257.2	42296.4	78.5
Mike	Wickiser	MW	42255.3	42255.3	42260.0	42264.6	9.3
Ryan	Lamppa	RL	42248.1	42248.1	42260.4	42272.7	24.5
Norrie	Williamson	NW	42260.0	42261.0	42260.5	42260.0	1.0
Jean-Francois	Delasalle	JFD	42231.4	42231.4	42262.8	42294.2	62.8
John	Disley	JD	42248.0	42281.1	42264.5	42248.0	33.1
Gerry	Rahill	GR	42244.1	42244.1	42267.8	42291.6	47.5
Jean-Marie	Grall	JMG	42250.8	42250.8	42272.7	42294.5	43.7
Scott	Hubbard	SH	42269.6	42269.6	42273.1	42276.6	6.9
Bill	Grass	BG	42252.5	42252.5	42281.0	42309.6	57.1
Rodolfo	Martinez	RMF	42280.6	42280.6	42294.7	42308.8	28.2
Luciano	Ramirez	LRG	42272.7	42272.7	42299.5	42326.4	53.7
Doug	Loeffler	DL	42246.6	42246.6	42300.3	42354.1	107.4
Dusty	Lewis	DL2	42286.7	42286.7	42303.6	42320.6	33.9
Jay	Wight	JW	42310.8	42310.8	42320.3	42329.7	18.9

R e p o r t

of the Wrocław Marathon course measurement,made by Tadeusz Dziekoński /Poland/ on April 13-14,1996.

I was asked by the Race Director to measure the new version of this race - due to public works at the start and finish area. Since next year this marathon will be held again on the original course, which was measured by me on March 28,1993.

New version of the course has only two modifications - at the start/ finish area/around 1 km from previous place/ and at Kamienna street /around 3 km from the start/.

Early morning of Sunday I measured/using electronic cyclo-computer/ part of the distance at above two places and compared this data to the measurement from March 28,1993. Next I received an optimal version /to place start and finish at Purkyniego street/ of two different loops - with difference of length/at Kamienna street/ around 700 m.

At a 23.887,41 m point/Kamienna str - see sketch/ of the measurement I was stopped by policeman who said: "You have to shorten the distance at Kamienna street". Next I had to make modification of the measurement /see sketch/:

- I came back to the corner of Ślężna and Kamienna streets and subtracted 5.939 counts from a sum of elapsed counts/1/236.775-975.000/,
- I lengthened the first loop/at Kamienna street-from pedestrian crossing to Komandorska street and fro/ by adding 2.973 counts,
- I followed the second loop on a longer distance at Kamienna street /from Komandorska to Drukarska streets and fro/ by adding 11.320 counts,
- per saldo I added 8.354 counts and started the measurement again at the opposite corner of Ślężna and Kamienna streets/at a 24.649,74 m point/

At arount 31 km point a bike's chain was destroyed and I lost around 1 hour waiting for reparation.

After the measurement organizer decided to place the finish line on the place from which I started the measurement/as start/ and the start line on the place at which I finished the measurement/as finish/. So,I had to cancell all 5 km splits but I did not have time to find correct splits before leaving of the last train to my home.

I made all calculations in my home and made corrections of all splits one day before the race day/without police protection/. Next I made another correction/on request of the Race Director/ and moved the finish line 4 m forward/the start line too/ but without any changes of splits.

encl: sketch

Białystok/Poland,May 7,1996

TADEUSZ DZIEKOŃSKI
ul. Chrobrego 4 m. 8
(skrytka pocztowa 14)
15-057 Białystok
POLAND

Pete Riegel - 3354 Kirkham Rd - Columbus, OH 43221
Phone: (614) 451-5617 FAX: (614) 451-5610
E-mail: Riegelpete@aol.com

June 26, 1996

Dear Tadeusz,

I just finished looking at your measurement of the Wroclaw Marathon. At first I could not understand what you did. Then I played with the numbers and I saw what you did.

Very nice work!

It was a situation that could have caused a panic in many measurers. You have only one day to do the work, and then you have all those difficulties! Many people would have gotten confused. I am not sure that I could have done as well. It was a beautiful recovery from a difficult situation.

Congratulations on a job well done!

Best regards,

Pete

ENCLOSURE TO THE REPORT OF
THE WROCŁAW MARATHON MEASUREMENT

MODIFICATION DURING THE MEASUREMENT:

1/ FINISH /⑥/ OF THE FIRST PART OF THE MEASUREMENT:
1/236.775 COUNTS

2/ BACK TO THE POINT ③ :
1/236775 - 1/242.714 = 5 939

3/ ④ - ② - ③ DISTANCE:
1/242.714 - 1/246.843 = 4.129

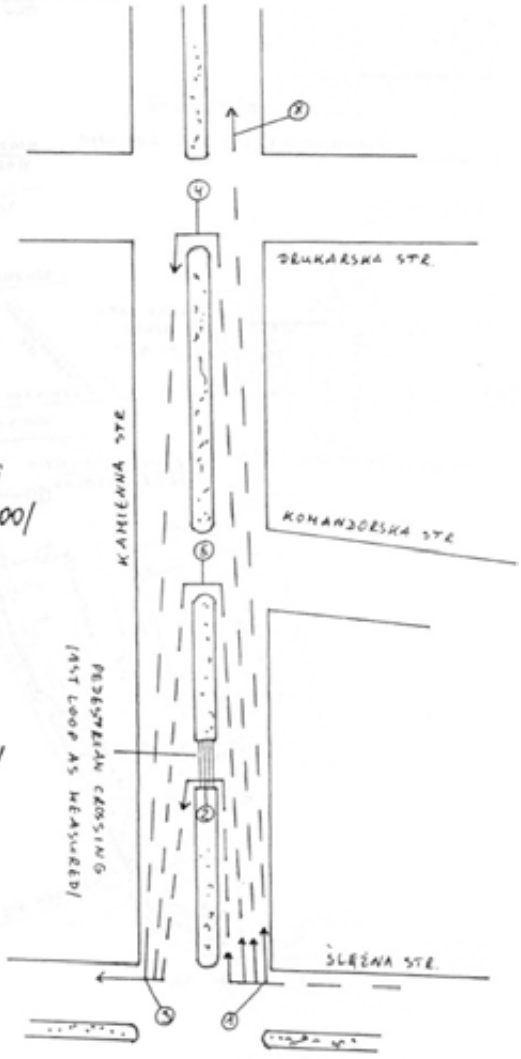
4/ ① - ⑤ - ③ DISTANCE:
1/246.843 - 1/258.163 = 11.320

5/ ④ - ⑤ - ③ DISTANCE:
1/258163 - 1/265 265 = 7.102

6/ DIFFERENCE BETWEEN:
- 3/ AND 5/ : 2.973
/7.102 - 4.129/

7/ MODIFICATION:
- COUNTS ELAPSED AT ④ POINT:
255.836 /1/236775 - 5.939 - 975.000/
- PLUS: 2.973
- PLUS: 11.320
- COUNTS ELAPSED AT ③ POINT:
270.129
- COUNTS TO THE FINISH:
192.244 /462.373 - 270.129/

8/ START /③/ OF THE SECOND PART OF THE MEASUREMENT:
265. 600



TADEUSZ DZIEKOŃSKI

ADJUSTMENTS
0 - 5 KM
20 - 25 KM

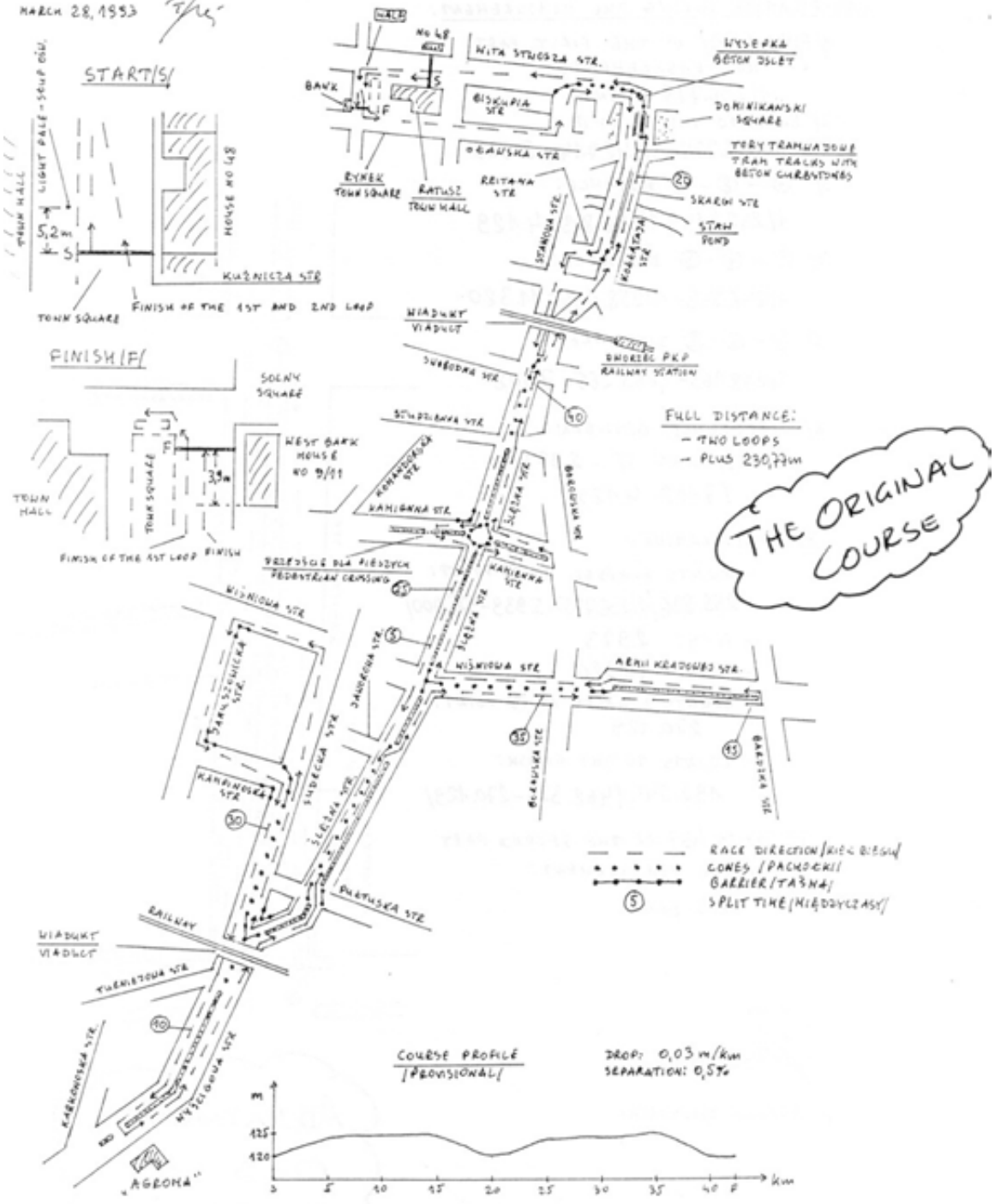
WROCŁAW MARATHON - POLAND

/COURSE MAP/

MAP/AJMS COURSE MEASURER
TADEUSZ DZIEKOŃSKI
POLAND
MARCH 28, 1993

NOT TO SCALE

N



FULL DISTANCE:
- TWO LOOPS
- PLUS 230,77m

THE ORIGINAL COURSE

WROCLAW MARATHON Measured by Tadeusz Dziekonski - April 13-14, 1996

Calibration course measured at 500 meters

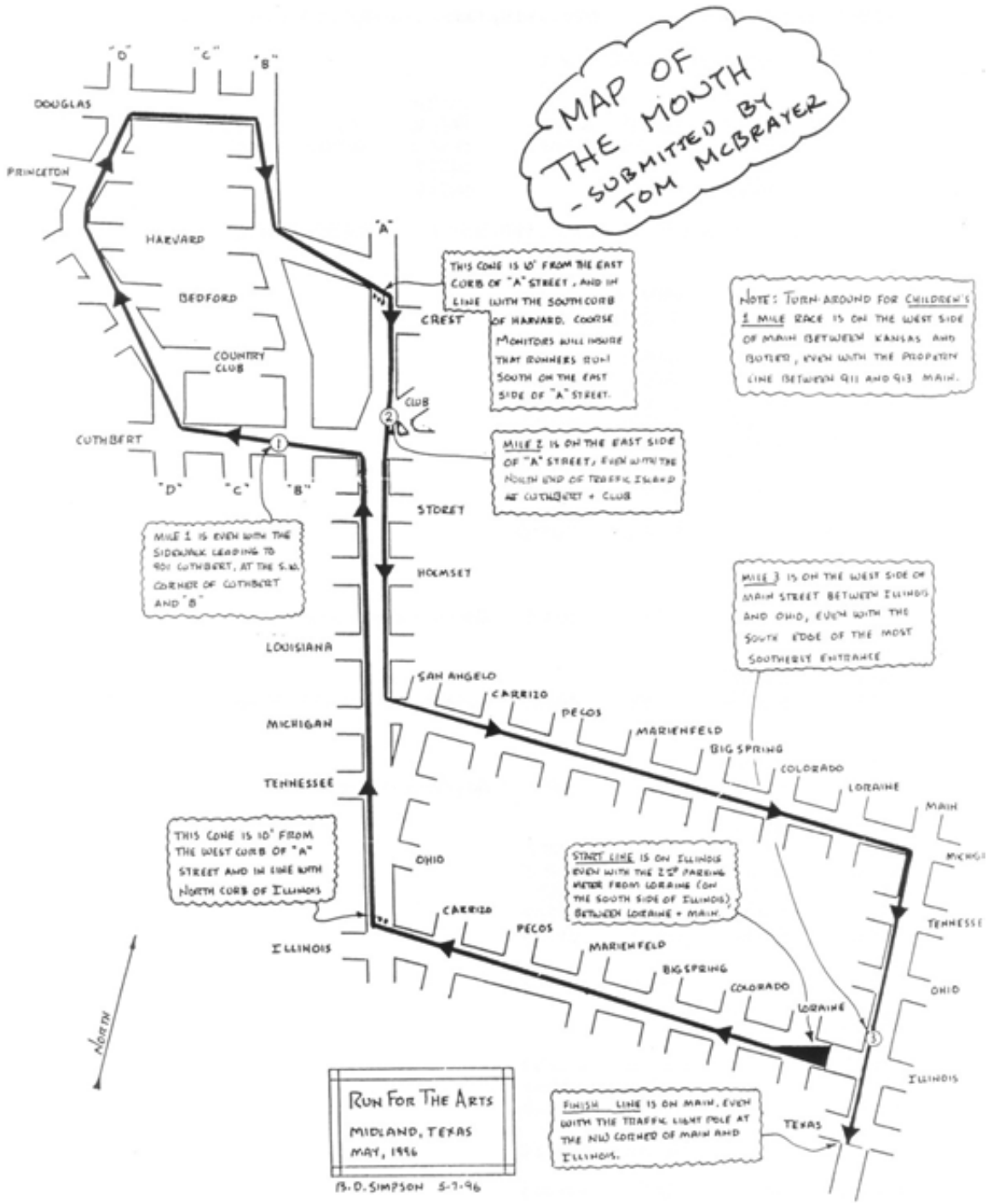
Precal			Postcal		
5473.5	5473.6	average	5474.0	5474.1	average
5474.0	10958.2	cts/km	5474.5	10959.2	cts/m
5473.5			5474.0		
5473.5			5474.0		

Day's Constant = 10958.6978 cts/km 10.9586978 cts/m
including 1.001

		Interval Counts	Interval Meters	
Start	975000			
5 km	1029790	54790	4999.7	
10 km	1084580	54790	4999.7	
15 km	1139370	54790	4999.7	
20 km	1194160	54790	4999.7	
21.1975 km	1206187	12026.5	1097.4	
stop x	1236775	30588.5	2791.3	
Point 1	1242714	-5939	-541.9	
Sub Total 1		255836	23345.5	
via 2				
Point 1	242714			
Point 3	246843	4129	376.8	Remove between Start and 5 km
via 4				
Point 1	246843			
Point 3	258163	11320	1033.0	Add between 20 km and 25 km
via 5				
Point 1	258163			
Point 3	265265	7102	648.1	Add between Start and 5 km
Point 3	265600			
25 km	269421	3821	348.7	
30 km	324211	54790	4999.7	
35 km	379001	54790	4999.7	
40 km	433791	54790	4999.7	
Finish	457844	24053	2194.9	
Sub Total 2		192244	17542.6	

Calculation of Total Length

Sub 1	255836	23345.5
123	-4129	-376.8
143	11320	1033.0
153	7102	648.1
Sub 2	192244	17542.6
Total for Course	462373	42192.3


MAP OF THE MONTH
- SUBMITTED BY TOM McBRAYER
"D"
"C"
"B"
"A"
DOUGLAS
PRINCETON
HARVARD
BEDFORD
COUNTRY CLUB
CUTHBERT
CREST
CLUB
STREET
HOLMSET
LOUISIANA
MICHIGAN
TENNESSEE
OHIO
ILLINOIS
SAN ANGELO
CARRIZO
PECOS
MARIENFELD
BIG SPRING
COLORADO
LORAINE
MAIN
TEXAS
NORTH
THIS CONE IS 10' FROM THE EAST CURB OF "A" STREET, AND IN LINE WITH THE SOUTH CURB OF HARVARD. COURSE MONITORS WILL INSURE THAT RUNNERS RUN SOUTH ON THE EAST SIDE OF "A" STREET.
NOTE: TURN AROUND FOR CHILDREN'S 1 MILE RACE IS ON THE WEST SIDE OF MAIN BETWEEN KANSAS AND BOTTEO, EVEN WITH THE PROPERTY LINE BETWEEN 913 AND 913 MAIN.
MILE 2 IS ON THE EAST SIDE OF "A" STREET, EVEN WITH THE SOUTH END OF TRAFFIC ISLAND AT CUTHBERT + CLUB
MILE 1 IS EVEN WITH THE SIDEWALK LEADING TO 901 CUTHBERT, AT THE S.W. CORNER OF CUTHBERT AND "B"
MILE 3 IS ON THE WEST SIDE OF MAIN STREET BETWEEN ILLINOIS AND OHIO, EVEN WITH THE SOUTH EDGE OF THE MOST SOUTHERLY ENTRANCE
THIS CONE IS 10' FROM THE WEST CURB OF "A" STREET AND IN LINE WITH NORTH CURB OF ILLINOIS
STREET LINE IS ON ILLINOIS EVEN WITH THE 2 1/2 PARKING METER FROM LORAINE (ON THE SOUTH SIDE OF ILLINOIS) BETWEEN LORAINE + MAIN
FINISH LINE IS ON MAIN, EVEN WITH THE TRAFFIC LIGHT POLE AT THE NW CORNER OF MAIN AND ILLINOIS.
RUN FOR THE ARTS
MIDLAND, TEXAS
MAY, 1996
B.D. SIMPSON 5-7-96

May 9, 1996

Berry Simpson
3003 Whittle Way
Midland, TX 79707

Tom McBrayer
4021 Montrose
Houston, TX 77006-4956

Dear Tom,

MAP OF THE MONTH!

Once again I am re-certifying my Run For The Arts 5K course. In years past the reason was that the Arts Celebration kept expanding into the street and obstructing my start and/or finish. This year, alas, it appears to be my fault.

For the past two years that we have used the existing course runners have complained that it's long. (And rightly so, since all runners think all courses are too long!) The first year was hot and muggy so I blamed the weather for their slow times. Last year, however, was perfect weather ... and extremely consistent runners were all turning in slow times. I told them I would remeasure before the 1996 race.

My gut reaction, true to my engineering profession, is always that I am right and that my calculations and measurements are correct until proven wrong. I remeasured this 5K (using our newly established but yet to be confirmed calibration course) and discovered it to be 175 meters (570') too long. About 3.5% too long. IT COULD HAVE GONE THE OTHER WAY!

I suspect the reason for this discrepancy was that our old calibration course had deteriorated ... the road had been repaved at least twice since the original measurement and we had triangulated out points onto the new road. We must have gotten this wrong the last time we did it, making our calibration incorrect. It was this suspicion that encouraged us to create a completely new calibration course, 500 meters, using professional surveyors and electronic measurements.

When my new Run For The Arts course came out so much shorter than the previous one I was a little nervous that I might have made another mistake. Last Saturday I ran the course with a surveyor's measuring wheel and got 16,563' (5K = 16,404'). Since this is an uncalibrated wheel, I felt this was close enough (1%) to confirm the original measurement with my bicycle.

ABOUT WHAT I'D EXPECT

Anyway, thanks for allowing me to ramble on ... but I felt some explanation was in order. We will race on the new course this year. I'll tell them it's different from the certified course advertised in the flyer, and will make no claims to certification of the new course until I hear from you.

Sincerely,
BDS

IF YOU CAN'T FIND THE END POINTS - - -

Contribution Of Wobbles To Measurement Error

By M.C.W.Sandford, 22 Stevenson Drive, Abingdon, Oxon, OX14 1SN, United Kingdom.

June 1996

Introduction

Every measurer tries to ride a perfect straight line on his calibration runs, and on his course measurements he rides the shortest possible course, which consists mainly of a series of straight lines which would be taken up by a piece of string stretched taut along the course. Any wobbles will result a greater distance travelled by the front wheel. I have looked back at my measurement track revealed after riding through a puddle or on soft ground, admittedly not the best riding conditions, and have been concerned to see the front wheel wobbles of 10 to 20 cm relative to the straighter track followed by the back wheel. How much error do wobbles contribute to our measurements?

In MN 66, July 1984, Roger Gibbons and Pete Riegel discussed the issue. One argument advanced was that the calibration ride with the fewest counts was the most accurate. On the other hand it was argued that a rider will also wobble during a measurement so the average of the calibration runs would be a better approximation to the rider's performance during measurement.

Pete Riegel has addressed the accuracy of calibration rides in MN 43, Sept 1990. For each set of 4 rides he took the lowest count as the most accurate and worked out the average difference between this reference and the other three rides in the set. The average difference for each of three groups of experienced measurers was 0.023%, 0.027%, and 0.033%. So wobbles appear to make only a small contribution to the overall error, easily encompassed by the 0.1% SCPF. My concern was that even the shortest calibration run of a set of four might contain significant wobbles which might vary with conditions perhaps giving rise to a systematic error between the ideal conditions of the calibration course and difficult conditions during measurement of a running course. I would only be fully convinced that my wobbles do not make a major contribution to the errors if I could measure directly the effect of the wobbles on my best calibration run.

In the first part of this investigation I looked at the average differences in a large number of calibration runs with both solid and pneumatic tyres. In the second part I directly measured the wobbles in a calibration set and calculated the true path length taken by the front wheel and so can quantify the contribution of wobbles to measurement error.

The Copenhagen Drive Calibration Course

All the data reported here were obtained on a new calibration course which is situated only 1 km from my home. The convenient location has enabled me to readily make many calibration rides, under well controlled conditions.

The course surface is a very smooth tarmacadam pavement for pedestrians and cyclists marked by the highway authority with a 14.5 cm wide white line using thick road marking paint of the type used for road lane marking. The surface of the paint appears to have an even smoother surface structure than the tarmac. Since the course curves smoothly through an angle of about 40 degrees over its length, it is important to follow the line accurately. My aim is to ride along the centre of the paint line. A course ride at the inside edge of the line, i.e. 7.25 cm smaller radius, will give a measurement 5 cm shorter than on the mid-point of the line. In practice I believe the **maximum** error due to an error in following the centre of the line is about half this amount i.e. +/- 2.5 cm. I have measured the course with a steel tape, and, after making due allowance for the stretching of the tape along a series of chords, arrived at a length of 650.60 m.

The Solid Tyre

The solid rubber tyre is the same one that I reported on in MN 75, p36, a "Green Courier" from the Green Tyre plc. The size was 32-630 (27 x 1.25), mounted on a standard front wheel 630-17 (27 x 1.25). The tyre is 30 mm in width and the tread in contact with the road is a rectangular section 5.5 mm wide

Measurements of Average Deviation of Calibration Rides

Table 1 gives the data obtained in 15 sets of rides in the temperature range 7 to 20.2 C. I have not included a few data points at higher temperatures where it appears that the temperature coefficient of the tyre changes substantially. From the plot of variation with temperature it will be seen that the tyre exhibits a very small negative temperature coefficient of expansion of $0.0614/7200\ C^{-1} = -9 \times 10^{-6}\ C^{-1}$, although the scatter on the data points is fairly large. The negative coefficient is unexpected but I note that data, which Pete Riegel kindly sent me, on another tyre from the Green Tyre Co. also suggests a negative coefficient although much larger in magnitude, $-70 \times 10^{-6}\ C^{-1}$.

The average deviation varies between 0.17 and 1.03 with a mean of 0.55 counts or 0.008%, 3 times smaller than the mean error reported by Pete Riegel for various groups of experts. I was pleased with this comparison since it suggests my rides on my calibration course are considerably more consistent than this reference set. Before I make any claim as to

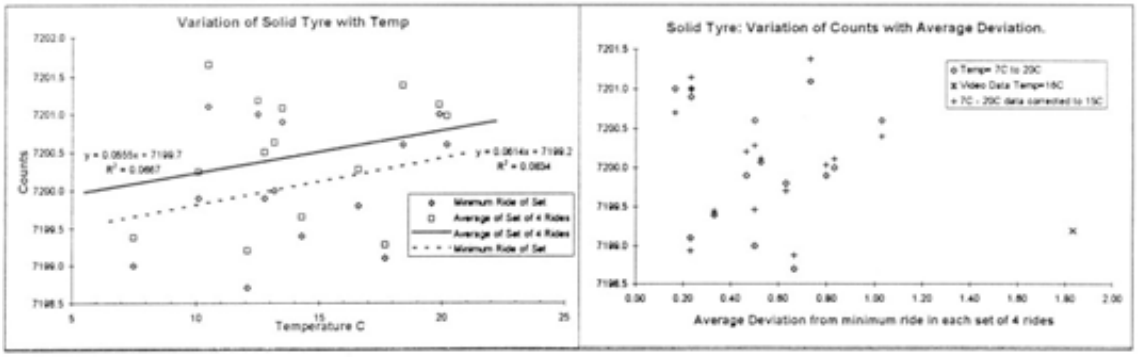
my riding skill, which is most unlikely to be exceptional, I contribute part of this to the excellent temperature stability of the solid tyre. A similar analysis of of calibration runs taken with pneumatic tyres gave a mean average deviation of about 1 count. I attribute much of this increase to the sensitivity of my pneumatic tyres to temperature. With a coefficient of expansion of 150×10^{-6} C^{-1} which is typical of my pneumatic tyres a temperature fluctuation of 1 C would give rise a change of 1 count. Temperature fluctuations of this magnitude are common particularly in partly cloudy conditions typical of fine weather.

If the average deviation is an indicator of the absolute magnitude of the wobble error the one would expect a correlation between the minimum counts and the average deviation. The data are plotted below both with a temperature correction to 15 C and without. There is no correlation. Consistent rides seem just as likely to occur associated with large minimum counts as with small minimum counts. I conclude that low values of average deviation do not necessarily indicate that the minimum ride of the set is a good ride with few wobbles.

The data give us no information about the contribution of wobbles to the best ride, only about the range of variation of wobbles over the set of rides. More importantly there is no information about the possible systematic variation in wobbles between the calibration ride and a course measurement. It is clear that a method is required which will directly measure the extra distance added by wobbles.

Table 1. Calibration Rides with Solid Tyre (7 C - 20 C)

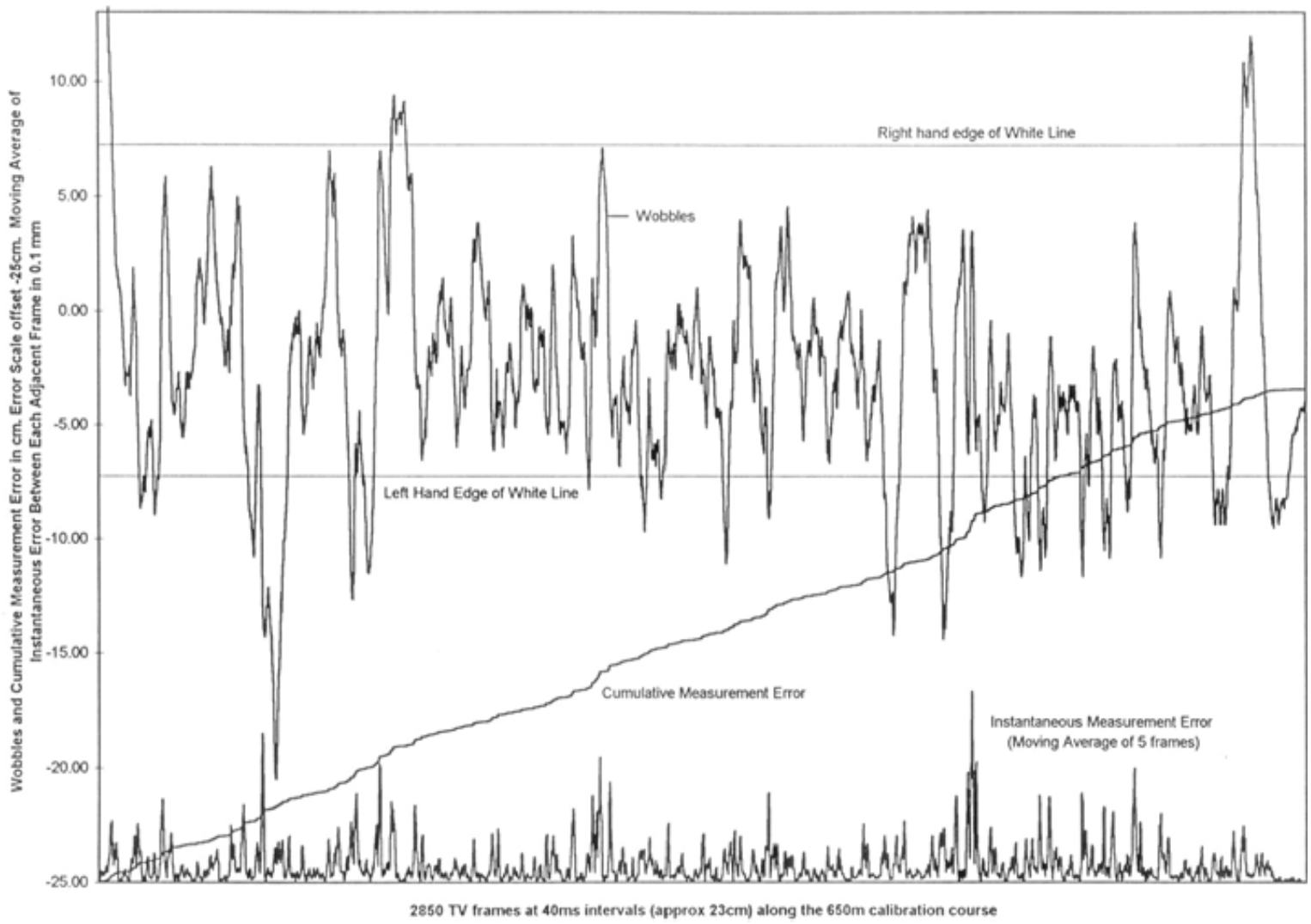
DATE	TIME	Temp C	Ride1	Ride 2	Ride 3	Ride 4	Average Deviation	Minimum	Average	Minimum corrected to @15C
24/4/96	15:35	20.2	7200.6	7201.7	7200.7	7200.9	0.50	7200.6	7201.0	7200.3
24/4/96	16:29	18.4	7202.7	7200.8	7200.6	7201.4	1.03	7200.6	7201.4	7200.4
4/5/96	16:32	13.2	7200.7	7201.2	7200.0	7200.6	0.83	7200.0	7200.6	7200.1
4/5/96	17:17	10.5	7201.7	7202.0	7201.1	7201.8	0.73	7201.1	7201.7	7201.4
12/5/96	07:55	7.5	7199.1	7199.9	7199.0	7199.5	0.50	7199.0	7199.4	7199.5
12/5/96	11:18	12.1	7199.4	7199.6	7199.1	7198.7	0.67	7198.7	7199.2	7198.9
12/5/96	15:11	14.3	7199.5	7199.8	7199.4	7199.9	0.33	7199.4	7199.7	7199.4
30/5/96	21:18	19.9	7201.3	7201.0	7201.2	7201.0	0.17	7201.0	7201.1	7200.7
31/5/96	06:10	12.8	7200.4	7199.9	7201.3	7200.4	0.80	7199.9	7200.5	7200.0
31/5/96	17:03	16.6	7200.7	7199.8	7200.6	7200.0	0.63	7199.8	7200.3	7199.7
1/6/96	06:45	10.1	7201.1	7199.9	7200.0	7200.0	0.47	7199.9	7200.3	7200.2
1/6/96	18:45	17.7	7199.4	7199.1	7199.4	7199.2	0.23	7199.1	7199.3	7198.9
8/6/96	07:18	13.5	7200.9	7201.1	7201.1	7201.2	0.23	7200.9	7201.1	7201.0
16/6/96	06:50	12.5	7201.6	7201.0	7201.0	7201.1	0.23	7201.0	7201.2	7201.2
	AVERAGE		7200.7	7200.5	7200.3	7200.4	0.53	7200.1	7200.5	7200.1
Set of rides with camcorder										
1/6/96	13:27	15.9	7202.6	7200.0	7200.5	7199.2	1.83	7199.2	7200.6	7199.1



Method of Measurement of Wobbles with a Camcorder

The Copenhagen Drive calibration course, defined by a white line, provided an ideal reference to measure wobbles by the lateral displacement of the front wheel relative to the line. I clamped a mini-tripod holding a camcorder to the frame of the bike. The camcorder viewed the point of contact of the front wheel with the ground from just behind and to the left

WOBBLES MEASURED BY A CAMCORDER, CALCULATED COURSE MEASUREMENT ERROR



of the top of the front fork. I set the camera to the sports mode to force minimum exposures to give least blurring when in motion. I made a set of four calibration rides. At first I found the bicycle somewhat difficult to ride with this arrangement. My right knee occasionally knocked the legs of the mini tripod which protruded 15 cm to the right of the frame. I soon adopted a slightly skewed posture. A better executed attachment would be recommended if this exploratory measurement was repeated. I also believe that a period of practice with the new arrangement would be desirable if one sought to achieve results fully characteristic of normal measuring. Despite these limitations I believe the results obtained throw considerable light on the effect of wobbles.

The data was analysed by playing back the recording on an 16 inch TV using a video recorder in the pause mode to freeze a frame. By means of a scale fixed horizontally across the TV screen the positions were recorded of one or both of the edges of the paint line and also of the side of the bicycle wheel (near the point of contact with the ground). The position of the wheel varied very little since the camera was fitted to the bicycle frame and as the front wheel is steered it rotates slightly about the point of contact which remains fixed in the field of view with an accuracy of about 1 mm. Some difficulty was occasionally encountered with a jitter in the horizontal position of TV frame, but this was readily removed when more than 2 mm by measuring the relative position of the wheel and the line edge.

I obtained a 4 head video recorder with a 'jog' control which enabled individual frames to be cleanly displayed at 40 ms intervals which corresponded to intervals of 23 cm along the course. I placed a millimetre scale across the TV screen. I estimate my precision in recording the positions as better than 2 mm on the screen, or 3 mm on the road. I chose ride 4 to analyse because this had the minimum counts (see table 1) and was therefore potentially the ride with fewest wobbles. It also felt much smoother than ride 1 when I had been learning to cope with the gusty head/side wind and with my knee hitting the camcorder mounting. The set up proved to be quite stable and it was easy to achieve a good precision. Over two days I typed the data for 2850 frames straight from the TV screen into a spreadsheet on my computer. I have not yet had the time to analyse rides 1, 2 or 3.

Calculation of Measurement Error from Wobble Position.

Using Pythagoras's Theorem it is easy to show that, if the distance between samples is x and the front wheel is displaced transversely by a distance y between samples, then the distance recorded by the wheel is approximately $x+y^2/2x$. To calculate the extra distance due to wobbles over the whole course we simply sum the values of $y^2/2x$ for each sample interval. y is the difference in transverse positions on the TV screen between successive samples and, making the approximation of constant speed, x is the length of the course divided by the number of samples.

The Results from the Measurement of Wobbles and their Interpretation

The plot of the results shows the wobbles relative to the position of the white line. Most of the ride was on the 14.5 cm wide white line and the average position is displaced about 3 cm towards the left hand edge, perhaps due to a small aiming error as I sighted the line some 20 m ahead and tried to follow it. The large displacement at the beginning is not a wobble since the white line curves very rapidly over about 1 m and the route from the end of course nail follows a straight line.

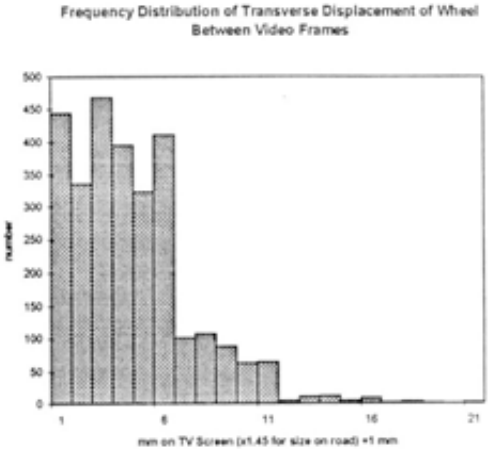
The instantaneous measurement error, derived from the formula $y^2/2x$ for each pair of adjacent frames, is shown with its zero offset at -25. The Y scale is such that 1 unit corresponds to 0.1 mm. The data have been smoothed for a clear presentation by performing a moving average over 5 data points. This removes much of the high frequency fluctuation and one can see a spiky pattern where periods of larger error lasting up to one or two seconds are interspersed with quieter periods. Reference to the wobbles curve shows that actual direction of displacement often reverses during the high error episodes. I speculatively interpret this as periods when I am fighting to control a my balance. The smoother periods are when the human-machine control system is spot on balance.

The instantaneous error, without any smoothing, has been added frame by frame to produce a cumulative measurement error. The scale zero is offset, and the units are cm so the total error over the 650 m course is given at the right hand end of the graph, 23 cm, i.e. 2.5 counts. The slope of the cumulative error is fairly constant over large distances, but on the fine scale it can be seen to comprise a number of steps of varying height (may be when I was fighting for balance) with periods with a smoother slope about half the average slope (when I was better balanced).

Due to the compressed scale of the graph, the data from individual frames cannot be seen clearly. Therefore, to give an idea of the angle of displacement that dominates, I have plotted the frequency distribution of the transverse displacement between adjacent TV frames in the graph on the next page. The scale along the bottom is mm on the TV screen. A plotting problem with my spreadsheet puts the zero displacement bar opposite 1 on the axis. Since the error is proportional to the square of the displacement nearly all the error is produced by displacements between 4 and 10 mm (6 and 15 mm on the road). This is an important observation since it shows that the very numerous displacements of 0 to

3 mm, which may contain significant experimental error, contribute little to overall error. In fact my experimental procedure is free of experiment noise.

Using the formula $\tan^{-1}$ (mm on TV x 1.45/230), a typical displacement of 5 mm corresponds to a steering deflection of 1.8 degrees or a 5 mm movement at the end of the handlebars. Wobble free cycle riding demands a steady hand! I speculate that the ultimate limit may lie in the balance sensors, the eye and perhaps the inner ear, and may be in the control algorithm learnt by the brain. The speed of response of the nervous system may also play a part. These factors lead me to believe that the physical and mental state of rider may influence the quality of the ride.



I turn now to the most important question which I set out to address. How much do wobbles contribute to measurement error? I make the assumption that for the fifteen calibration runs in table 1, after making the small temperature correction, the variation in ride count is produced entirely by wobbles. The minimum corrected ride count was 7198.9 counts. I analysed a ride with a corrected count of 7199.1, almost the same, and measured the path followed by the front wheel to be 23 cm (2.5 counts) longer than a wobble free following of the white line. The true minimum ride for a rider with perfect balance and zero wobbles would therefore be 7196.6 counts. The average deviation of 0.53 from an average minimum of 7200.0 should be described as an average deviation of 3.9 counts from a wobble-free ride of 7196.6. Next one asks whether this average deviation of 3.9 counts (0.05%) vary between calibration conditions and measurement conditions. I still have no direct answer to this. But some conclusions can be drawn from table 2 which shows the deviation from the wobble-free count for the 15 sets of calibration rides in the generally the fairly good conditions of the calibration course.

Table 2. Calculated deviations from the measured wobble free course length of 7196.6 counts at 15C.

DATE	TIME	Temp C	Deviation from 7196.6 with temperature correction				Average Deviation of 4 rides in a set	
24/4/96	15:35	20.2	3.7	4.8	3.8	4.0	4.1	
24/4/96	16:29	18.4	5.9	4.0	3.8	4.6	4.6	
4/5/96	16:32	13.2	4.2	4.7	3.5	4.1	4.1	
4/5/96	17:17	10.5	5.4	5.7	4.8	5.5	5.3	
12/5/96	7:55	7.5	3.0	3.8	2.9	3.4	3.2	
12/5/96	11:18	12.1	3.0	3.2	2.7	2.3	2.8	
12/5/96	15:11	14.3	2.9	3.2	2.8	3.3	3.1	
30/5/96	21:18	19.9	4.4	4.1	4.3	4.1	4.2	
31/5/96	6:10	12.8	3.9	3.4	4.8	3.9	4.0	
31/5/96	17:03	16.6	4.0	3.1	3.9	3.3	3.6	
1/6/96	6:45	10.1	4.8	3.6	3.7	3.7	4.0	
1/6/96	18:45	17.7	2.6	2.3	2.6	2.4	2.5	
8/6/96	7:18	13.5	4.4	4.6	4.6	4.7	4.6	
16/6/96	6:50	12.5	5.2	4.6	4.6	4.7	4.7	S.D.
	AVERAGE		4.1	3.9	3.8	3.9	3.9	0.8
1/6/96	13:27	15.9	5.9	3.3	3.8	2.5	3.9	

Table 2 shows the deviation from 7196.6 for a set of 4 rides is on average 3.9 with a standard deviation of +/-0.8 counts which corresponds to 0.01%, a negligible error, well within the SCPF so it can be ignored. To take an extreme case, a bad set such as the 4 May set, which is probably on the point of feeling bad, has a deviation of 5.9 counts compared with the best set which has 2.5 deviation giving a total error of 0.05%, significant but still covered by the SCPF. However, if one compared with a perfect ride, the error could be as large as 5.9 counts or 0.08%

Conclusions

I observed a variation of calibration constant due to wobbles with a standard deviation of 0.01%. A total range of 0.05% was observed over 15 sets of calibration rides. It would be interesting to see the effect of different riding conditions and condition of the measurer. It would also be interesting for several measurers to compare their performance with the same tyre. This would be an easy way of looking at the variation accuracy of straight line riding between different measurers, although we must always remember that self consistency is the key to course measurement.

Subj: RE: Sour Grapes!
Date: 96-05-04 02:50:30 EDT
From: BCallan369
To: Riegelpete

Hi Pete;

Couldn't help but see the article in this months Measurement News concerning Mr Jan Scheers claim that the Las Vegas Half Marathon must be short because of the finishing times of Eddie Hellebuyck and others.

I feel real good about the measurement of the course and think either Mr. Scheers didn't notice the drop per Km on the certification (if he even is capable of reading it) or he has a personal problem with Eddie Hellebuyck. He further stated that Hellebuycks' time in the Ghent 10K is also a problem. I think the real problem is in Mr. Scheers mind . I wonder what Eddie Hellebuyck did to him. Maybe he owes Mr Scheers some money and Scheers is getting even by bad mouthing him. If I were a Jersey type of guy (of course I moved away from Jersey a long time ago) and not just a simple musician from Las Vegas, I would consider calling up the Guido Brothers Escort Service in Fairfield, Ct and have a contract put out on Mr Scheers.

Best Regards,

Bill Callanan

E-MAIL ADDRESSES

This list will continue as a result of several requests. New addresses, and address changes will be posted as received.

Bob Baumel	Bobbau@pcok.com
Andy Beach	Abeach@ti.com
Tom Benjamin	benjtr@aol.com
Bill Callanan	Bcallan369@aol.com
Bernie Conway	Bernconw@enoreo.on.ca
Tom Ferguson	Ktjsudad@lava.net
Zean Gassmann	Zeansusan@aol.com
Basil Honikman	Honikman@silcom.com
Alan Jones	AlanLJones@aol.com
Tom Knight	Tdk@stanford.edu
Tom & Mary Anne McBrayer	104613.56@compuserve.com
Jack Moran	Jmoran@skypoint.com
Gene Newman	Brrunner@aol.com
Pete Riegel	Riegelpete@aol.com
Brian Smith	Bnewbatt@aol.com
Jay Wight	Jaywight2@aol.com

See Bob Baumel's homepage at http://www.pcok.com/~bobbau/

THE MARSH WALL PUZZLE

This puzzle generated a variety of answers, as well as some moral admonitions to me. Here are the answers that were submitted. Letters on the subject may be seen elsewhere in this issue.

	Along the Center	Along the SPR	
Bill Glauz	15.11	7.33	
Bob Baumel		7.8	
Brian Smith	10.762		
Alan Jones		8.62	
Pete Riegel	14.73	5.75	(this was done later, after I discovered the scale on the drawing)
Jean-Francois Delasalle	14		
Roger Gibbons		8.452	
David Reik		7.7	
Hugh Jones		9.2	

The Start was adjusted by 14 m, shortening the course.

Jean Francois Delasalle adds the thought that without an exact measurement no adjustment should be made. He also says (translated by computer) "A thought and a symbolic homage to the victims of the bomb - the loss of time for the marathon will be only 3 or 4 seconds."

Dear Pete,

I guess you haven't been running for enough years to remember that Runner's World was originally named Distance Running News. Long Distance Log, as I recall, came out of New Jersey and was started by Browning Ross.

Re: Your Marsh Wall puzzle. I can't believe that a major marathon course measurement is based in part on distance estimates taken off a map rather than by direct measurement!

new course distance = {original 42.195 m + SCPF} - {old A to B} + {new A to B}

The portions directly measured are the {original course} and the {new A to B}. You have no direct measurement for the {old A to B} which is necessary to calculate the {new course distance}. Note that even if you were to measure the {old A to B} directly, the SCPF should be applied to both the old and new AB sections, adding another 0.134 m to the 42.195 m from the SCPF.

In this instance, I doubt any serious errors would result from what you did. However, if it is permissible to rely on distances obtained from a map for a portion of the course, how big of a portion of a course can one take distances measurements from a map?

Once I measured a 10 km road course which went through a wash that had been flooded out. Large piles of sand and running water made it impossible to measure using a bicycle. I knew these obstacles would be cleared by race day but my schedule did not permit waiting until the road was cleared. It was a perfectly straight section, no tangents to worry about. We steel-taped the section which amounted to about 58 meters. It certainly would have been easier to use a map to get the distance!

What should you have done with the London Marathon course? In any long measurement, e.g., 20 km or more, I try to put in known reference points using two (or three) measurements to determine their distance from the finish (or start, depending on how I am measuring the course). Suppose the London Marathon course had five such reference marks, equally spaced roughly 8.44 km apart, call them V, W, X, Y, and Z. Suppose your detour were between W and X. All you would have to do is measure the new distance between W and X, or roughly 8.44 km. Add that to the previously measured (certified) segments, Finish-V, VW, XY, YZ and Z-Start, and you have the course distance directly.

Maybe this doesn't address the question you posed but in my opinion, the only use of map measurements is to obtain a rough layout for a course. Such "measurements" should NEVER be used in lieu of proper direct measurements on the course.

Sincerely,

Ken

Ken Young
7550 Folsom-Auburn Rd #808
Folsom CA 95630

Pete Riegel - 3354 Kirkham Rd - Columbus, OH 43221
Phone: (614) 451-5617 FAX: (614) 451-5610
E-mail: Riegelpete@aol.com

May 14, 1996

Ken Young - 7550 Folsom-Auburn Road # 808 - Folsom, CA 95630

Dear Ken,

Jean-Francois Delasalle also chastised me for my moral lapse. Enclosed is what I sent him.

When I was on the spot, I had only an unclear fax copy of the construction drawing. I took one measurement within the space available in an attempt to determine the scale of the drawing. I had not yet discovered it. Bob Blythe said the difference was about 14 -15 meters. He was thinking centerline. My rough calculations indicated a range of 5 to 20 meters depending on the assumed configuration. Only later did I discover that the scale was on the drawing, and I had not seen it. If I had seen it I would have used it and probably allowed a 6 m adjustment.

Because a lightpole limited movement (Start line could not be closer than 3 m to the lightpole because of scaffolding requirements) my choices were: zero change, up to 8 m change, no less than 14 m change.

Knowing that the course had been checked out by me in 1994 using SOSS over a lot of intermediate permanent reference points, I permitted a 14 m shortening of the start.

A remeasurement between the nearest two reference points would have involved 10 to 15 km of riding. I judged this overkill for what was involved, thus performed as described.

While in theory I have sinned, in practice I feel it is no more than a minor boo-boo. Either way you cut it, I plead guilty.

In defense, I don't think a remeasurement of a 12 km portion of the course was likely to do a better job than the one I did. I violated a principle, but I won't apologize for my practice.

I realized my error about Long Distance Log just as the issue was to go to press and figured to hell with it - let a reader tell me I was off base. Thanks for the correction.

Best regards,

Pete Riegel - 3354 Kirkham Rd - Columbus, OH 43221
Phone: (614) 451-5617 FAX: (614) 451-5610
E-mail: Riegelpete@aol.com

May 14, 1996

Dear Jean-Francois,

I have been away. I received your puzzle answer. I agree that in theory I should not have permitted an adjustment of the start, but I was weak.

The course was measured in 1991 and 1994 using sum-of-shortest-splits (SOSS) from three riders over many segments. I was not the shortest. The sum of my personal rides was about 10 metres greater than the distance obtained by SOSS.

By SOSS the course was equal to 42194.34 m

By my ride only the course was 42205

My estimate of the Marsh Wall difference was 5.8 m. I permitted the course to be shortened by 14 metres.

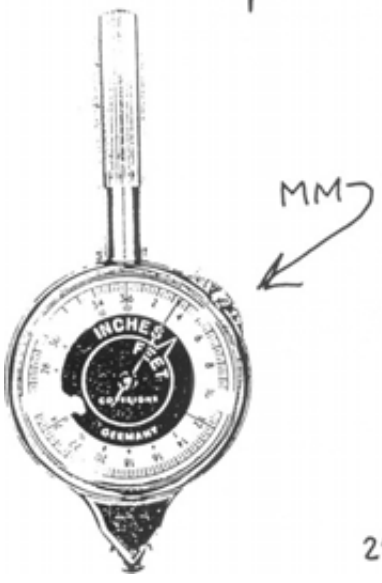
The length of the 1996 London course, by my measure, was 42205-14+5.8 = 42196.8 m.

The course was OK, but you were right. Without a direct measurement it is not proper to adjust the start.

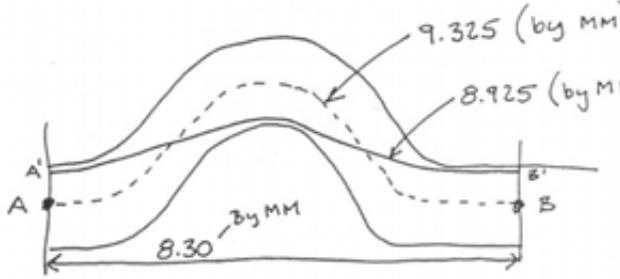
cc: J. Disley

Best regards,

Pete



PETE'S DATA:



A-B (STRAIGHT) = $134\left(\frac{8.30}{9.325}\right)$ = 119.27 m

Δ = 14.73 m

A-B (SPR) = $134\left(\frac{8.925}{9.325}\right)$ = 128.25 m

Δ = 5.75 m

June 19, 1996

Peter Riegel, 3354 Kirkham Road, Columbus, OH 43221-1368

Dear Peter Riegel:

I tried "The Marsh Wall Puzzle." I used my flexible ruler to measure distances on the diagram on page five of the May "Measurement News." I got 192 mm for the distance between A and B along the centerline of the new roadway, 184 mm for the SPR between A and B along the new roadway, and 173 mm for the distance between A and B as the old route went. You say the distance between A and B along the centerline of the new roadway is 134 meters, so the SPR between A and B along the new roadway must be 128.4 m and the old route between A and B must be 120.7 m. Therefore, you should subtract 7.7 m from the start to compensate for the diversion.

I hope this wasn't really how the course was adjusted; if this paperwork was sent to me for approval, I would reject it. "Course Measurement Procedures" doesn't list map measuring as an acceptable method to measure courses for certification. You wrote, in your November 19, 1995 letter (on page 24 of the January, 1996 "Measurement News) to Michael Renner regarding the Bloomsday short course, "At the conclusion of any measurement, including one in which adjustments were made, it should be possible to reconstruct a complete chain of certifiable measurement data between locatable reference points, be they hydrants, drains, or nailed split points." Didn't anyone record, and save, measurement data, including counts recorded at locatable intermediate points, for the London Marathon? Wouldn't the correct thing to do have been to measure between one of those "locatable reference points" on one side of the diversion to another such "locatable reference point" on the other side of the diversion, and then to construct a "complete chain of certifiable measurement data" for the new course? When I measure courses, I like to record counts at points on the course nearest to the same landmarks that are used to locate the course markings, and I like to include all my raw measurement data on the certificate. That way, anyone who has the certificate can check my work, and, if someone wants to change part of the course, adjustments can be made with the "complete chain of certifiable measurement data" intact.

Speaking of complete chains of certifiable measurement data, such a chain apparently did not exist for a course Wayne Nicoll measured, and certified, in my state, Connecticut. While validating the Lake Waramaug 100-K and 50 miler, Wayne measured a 50-K course that used the same loop around the lake that the 100-K and 50-miler use. Wayne sent me a copy of the certificate for the 50-K course, and I already had what turned out to be all of his measurement data; the measurement data was in the validation report Wayne had sent me. I was confused, and so wrote him a letter; this is what I wrote:

December 13, 1995

Dear Wayne:

I want to make sure I have my facts straight regarding the Waramaug 50K. You measured the course once. The second measurement consisted of measurements of two segments by Guido Bros., certified by me, and a measurement of an additional segment by Rick Favier.

The segment measured by Rick Favier was not certified. He retained no data. He filled out no forms.

The certificate you signed for the 50K only mentions yourself as a measurer. The dates of the Guido Bros. measurement, and Rick Favier's measurement, are not mentioned on your certificate.

You indicated on your certification form for the course that the difference between the two best measurements of the course was zero. In your letter to me dated 11/6/95 you wrote: "the difference of zero is an error" and say that the course that Favier laid out was 7.98501 m longer than what your measurement indicated was necessary, so I should "annotate" my copy of the certificate to reflect the difference as eight meters. But you say you used your average calibration figure, rather than your larger calibration figure, the one from your post-measurement calibration. Using your larger calibration figure, the 50K course measures 49.993 km.

If my understanding of the facts is correct, this course should be de-certified. As the recent problems with the Bloomsday course emphasize, certifiers must be held to the same standards as anyone else. I have long thought that there is an obvious conflict of interest when we approve courses that we measured ourselves. When no one else is looking at one's data, the temptation to rationalize deviations from the prescribed procedure, if the deviations save time, is very strong. I would be surprised if you would approve an application for certification that consisted of the data (as I understand that data) you had for the Waramaug 50K. I would approve no such application.

Sincerely,

David Reik
26 Griswold Drive
West Hartford, CT 06119

I got back this reply:

12-20-95

Dear David -
Regarding the Lake Waramaug 50K measurement. I have nothing further to offer on the subject. I am too busy with other volunteer matters, including the planning for the Olympic road course measurements. If you want to pursue this further, feel free to direct your comments to Pete Riegel. I may wish to add my comments at a later time. I am fully confident the 50K course is at least the full distance and I challenge you to find it otherwise. I do not intend to de-certify the course.

Cheers, Wayne.

I attached a note to the next course I sent to Wayne. The note read:

Dear Wayne - Thanks for the quick card re: 50K. I guess you're not contesting my understanding of the facts.

So is our system such that, if a certifier is "fully confident" the course is "at least the full distance," then the course can be certified? I thought we had certain procedures we all had to follow. - David

An apparently common departure from the dictum that we should always, even when doing adjustments, have "a complete chain of certifiable measurement data between locatable reference points" occurs this way: The measurer wants to change a segment of the course which includes the

start or finish. The measurer uses an uncalibrated bicycle and rides from a point which will be both on the old and the new course to the old start or finish, and then rides from the same point, going the same number of counts, to establish the new start or finish.

Although, in a July 27, 1993 letter to me, you wrote, regarding this technique, ". . . a deterioration in quality occurs each time an adjustment is made," people seem to have a hard time seeing that there is anything wrong with the method. One measurer told me this practice was fine because the numbers "cancel out." In a May 15, 1993 letter to me about an instance of this adjustment method being used, Wayne Nicoll wrote:

> The technique used does not result in any deterioration of the quality of the measurement. We would calibrate the bike if we were determining a specific distance, but in this case we are not seeking the value of the exact distance. We only want to be sure the new start line is the same distance from the merge point as the old start. That they have done. It appears they rode the two paths with care to have achieved such good numbers.
>
> This procedure is not spelled out in the measurement book but is covered in training of new certifiers and is commonly given as appropriate guidance by certifiers to measurers who need to make similar changes. It is the only situation I know of where you can measure with an uncalibrated bike.

I've tried to come up with some way to make it clear to people that this procedure breaks the chain of certifiable data. The best explanation I can come up with is this: When we create a race course, we are actually making copies of a calibration course. We string together enough copies of the calibration course to get the distance we want. But our copies, made using our Jones Counter-equipped bicycles, are not perfect. We estimate that our copies differ in length from our calibration courses by about plus or minus 0.08%, or eight meters per 10,000 meters. When the shortcut adjustment procedure described above is performed, a copy is made of a segment of the race course instead of a calibration course. The copying will not be exact, just as the copying of the calibration course that established the race course was not exact. In fact, the copying of the segment of the race course will probably be more inexact than the calibration course copying, because, unlike the calibration course, the segment of the race course will probably not be be straight, and so will not be so clearly defined and easy to follow precisely. Also, I don't believe people using this shortcut adjustment technique generally ride four times over the segment being reproduced, average the four readings, ride the new segment twice using the ride that establishes the longer new segment, and ride over the old segment four times and use the average of the second set of four rides if it is larger than the average of the first four rides over the old segment. That would make sense if we wanted to do this copying of a segment of a race course with the same precision with which we copy calibration courses when we create race courses. But even if we did copy the segments with the same precision with which we copy calibration courses, we would be still introducing more inaccuracy than our program is supposed to have. Because of the inaccuracy in calibration courses (which are, themselves, generally not-quite perfect, strung-together copies of steel tapes), plus the inaccuracy of the copying of the calibration courses with counter-equipped bicycles, we believe race courses measured to be 10,010 meters could be as short as 10,000 meters or as long as 10,020 meters. A segment to be reproduced of a 10-K race course, plus the remainder of the race course, could equal 10,000 meters, or 10,020 meters. The addition of the imprecision of the method used to copy the segment could put the course over 10,020 meters, or under 10,000 meters. Another layer of copying has been introduced; if the steel tape is thought of as the "original," the race course, instead of being a copy of a copy, is, in part, a copy of a copy of a copy. Things have gotten fuzzier.

If complete measurement data, including readings at locatable intermediate points, had been recorded and saved, then adjustments to the course could have been made while maintaining "a complete chain of certifiable measurement data."

I think the moral of the story is that we should not be approving courses we measured ourselves. The fact that there is never as much time as we would like there to be causes people to convince themselves that they have a complete chain of certifiable measurement data, when they do not. If we all had to write out our chains of data and send them to someone else for that person's inspection, I think it would be harder for people to kid themselves.

I was interested in Paul Hronjak's piece on page 21 of the May, 1996 "Measurement News." Note how Paul discovered that the measurer sending in documentation to him had not used the whole road; Paul wrote: "I then looked at her excellent map and saw that she had carefully drawn a line which was not SPR." On page 39 of the January, 1996 "Measurement News," you wrote, as you have written before, "And a single-line map is OK if it's noted that the runners have the entire roadway throughout the course, and is supplemented with a S/F sketch and splits." Apparently, the person who sent in the measurement to Paul Hronjak filled out the "Application for Certification of a Road Course" form and answered "yes" to question 23, which reads: "Did you measure an unrestricted route? Do the runners have use of the entire road, from curb to curb?" Our "Application for Certification of a Road Course" states "Be sure you map shows the exact measured path." and "Include a line representing the actual measured path." Page 26 of "Course Measurement Procedures" discusses this requirement in detail. I think Paul Hronjak's experience indicates that it is a wise requirement. The "Shortest Possible Route" concept is not one that people grasp easily. Perhaps the "confusion" is produced by love of life; riding the SPR can involve riding into blind curves, against traffic, in the middle of the lane. Anyway, the requirement to indicate on your map where, within the roadway, you rode, for the whole course, is a useful way to detect confusion those submitting measurement documentation may have about what constitutes the "shortest possible route." If we want to abandon it as a requirement, we should decide to do that by some sort of democratic process. Unless I am to be the supreme ruler, I prefer democracy.

In my information sheet that I send out to anyone who has given me a hint that he might be interested in the certification program, I say, "If a 10K is certified, you can be almost sure that the shortest legal route is longer than 10,000 meters and shorter than 10,020 meters. Greater accuracy could be achieved, but not without many more hours of work." I would be interested in finding out how we could achieve greater accuracy if we were willing to spend more time. The Olympic marathon might have been a good course to try methods that would give greater accuracy than the method we use. I would think that numerous calibration courses along the route would be essential to any method attempting to do better than our normal procedure. Wasn't that done for the measurement of the 1984 Olympic marathon? I think some sort of careful marking of where the SPR was would be something you would do, maybe not painting a continuous line, but at least something like the Guido Brothers did once: They placed traffic cones at strategic points so the measurer had to something to ride at. It would be nice to have a surveying team measure all the straight parts of the course with an electronic distance meter, and mark the endpoints of those straight sections so each straight portion of the course could serve as a calibration course. Maybe the entire course could be measured with some sort of custom measuring wheel equipped with a Jones Counter and some sort of guides so the operator could stay 30 cm from curbs. I would think a measuring wheel could be made to be more accurate than a bicycle because its slower speed would allow the SPR to be more exactly followed, the weight on the wheel could be made to be constant regardless of the incline, and the wheel could be kept vertical while going around turns. I thought M.C.W. Sandford's article entitled "Sensitivity of Solid Tyres to Road Surfaces" on page 36 of the January, 1996, "Measurement News" was very interesting. Maybe that article indicates that the ideal measuring wheel would have some sort of tire that was soft so as to minimize the effect of small irregularities in the road surface, but that was non-pneumatic so as to minimize the effect of temperature changes. Given the time constraints of real life, our current system is a good compromise between speed and accuracy, but, for the occasional course, it might be intriguing to see how accurate we could get. Such an effort might allow us to speak with more confidence about how accurate we actually are with our standard method. Barrie Almond, a runner in my club, is an Australian native and was involved in the measuring of the course for the 1956 Olympics in Melbourne. He said they measured the multi-loop course both with a steel tape, and with the calibrated bicycle method, and got good agreement between the two methods.

Sincerely,

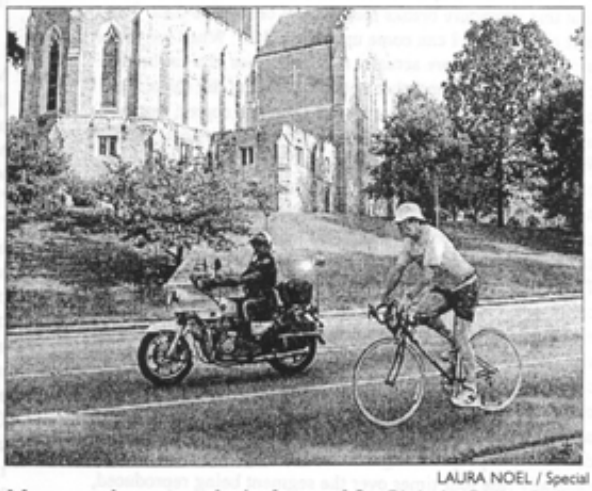
David Reik

David Reik, 26 Griswold Drive, West Hartford, CT
(860)236-9160 (Note the new area code.)

MEDIA COVERAGE OF OLYMPIC RIDE -DON SHEPAN ON THE SPR

Not exactly coverage — oh well!
Julie

AT



LAURA NOEL / Special

Measured approach: In front of St. Philip's Cathedral on Peachtree Street, one of a group of 28 cyclists, with a police escort, trace the Olympic marathon route, making final measurements of the course.

David Reik - 26 Griswold Dr - W. Hartford, CT 06119

Dear David, June 27, 1996

Wayne wrote to me some time ago describing his Lake Waramaug measurement and your dissatisfaction with his methodology. He said you might be getting in touch. Now I see you have, and I'll try to address your concerns:

To begin with, it would have been child's play for Wayne to make up some plausible data instead of openly describing the method he used. He is hiding nothing, but instead has been open and honest concerning his methodology. He readily admits that what he did was not consistent with the letter of the law, so to speak. However, he believes that no serious loss of accuracy was present, and I tend to agree. Neither Wayne nor I believe it was a method anyone should employ on a regular basis. This measurement was an anomaly, and is not the run-of-the-mill methodology used by Wayne.

A note on Wayne's use of the average constant: Although we strongly recommend the use of the larger constant, use of the average is permitted. Some people feel that use of the larger constant makes courses "too long." While I tend to agree, this criticism is generally brought to bear when someone has a huge calibration change and doesn't want to add the extra distance caused by it. The results of the Olympic ride showed some interesting things regarding the larger constant. We had 25 measurements of the course. If we pair them, we get 300 different measurement pairs. Of these pairs, 207 agreed within the required 0.08 percent. Of the 207 pairs of measurements, the lesser of each would have survived validation 91 percent of the time if the average constant had been used as the layout constant. If the larger constant had been used, the survival rate rose to 99 percent. To me this illustrates the value of the larger constant.

In general I am reluctant to certify a course which is based on an adjustment of a previous course, unless the two courses are measured by the same person. However, I will sometimes do it. When I do, I counsel the later measurer that by adjusting the previous course they are assuming responsibility for the entire course. They become the measurer of record. I won't listen to alibis about the short course being due to error by the "other guy."

I often give a break to a new measurer when they make minor mistakes, just to give them confidence to keep on measuring. If I give a person a terribly hard time on the first go-around, they will become discouraged, and I don't want that. This means that a percentage of first measurements are on the shaky side. Probably some are short. I doubt they are short by much, not enough so that any runner would notice. I don't mean to imply by the preceding that I will certify anything sent to me. That is not the case. But I don't hold people to a perfect standard. In some

PLEASE REPLY TO: PETER S. RIEGEL, *CHAIR, ROAD RUNNING TECHNICAL COUNCIL*
3354 KIRKHAM ROAD, COLUMBUS, OHIO 43221-1368
HOME PHONE 614/451/5617, FAX 614/451/5610

cases I will calculate an amount that I want them to add to put their course length into a region that relieves any doubts I may have. I often use sum-of-shortest-splits for this, which is not a standard method in the book, but which is useful in making shaky measurements "safe." I prefer to do this rather than make the measurer go out and do it again. If they don't want to add the distance, they can always go out and measure again. I give them the option.

Should we certifiers certify our own courses? It would certainly add a layer of safety if we had to submit each of our measurements to another certifier to review. Wayne and I have, on occasion, exchanged information on measurements in which we have questions, and checked each other. However, to require this as a standard procedure would add time and work, and I doubt enough errors would be found to justify it, except as a way of keeping our hearts pure. Certifiers would not have their jobs if they were not proven people. I don't say perfect, but good enough to understand what is needed, and do it.

As for maps, if a line map is used, it cannot show the path measured within the roadway. However, if it is noted on the map that the route is everywhere unrestricted, the map is good enough to do the job. It clearly defines how the course was measured. Some complicated courses are not as well-defined by a show-the-whole-roadway-with-the-measured-line map as by a simple line drawing showing the streets to be followed.

The certification system in the US is a balancing act. In order to get the measurements done we allow anybody at all to do the work, and we review the paperwork. While this is beneficial in finding new talent, it also means that a percentage of courses measured by inexperienced people will be marginally short. We could increase overall accuracy by requiring that all courses be measured by experienced, vetted people. This is how it is done in the rest of the world. However, we have limited manpower, and large numbers of courses to be measured. We do not have enough experienced people who would be willing to shoulder the burden of measuring all the US courses. And we would still have to find a way to bring new people into the game and train them. We use on-the-job training, and accept that it isn't perfect.

The success of our present methods can be gauged by how well things turn out when courses are validated. Our present success rate for certifiers is above 95 percent, and I don't think I have ever seen a course flunk which would have been saved by more reviewing. The errors are generally those involving loose riding, which cannot often be detected just by looking at the numbers.

Thanks for your letter and the Marsh Wall solution.

Best regards,

Historical Measurement

Many of us have plotted the progression of World Records or World Bests over the years, or the winning times of Olympic distances, or of a marathon such as the Boston AA (BAA). A common method is to plot "pace" (time per unit distance) against the year. Such plots for different distances and even different events can be overlaid and compared, also inviting mathematical modeling to describe what we see.

A variation is to plot splits taken over the years at, say South Framingham in the BAA run. Since the winner is unknown at a checkpoint, such historical data is often of just the checkpoint leader. By collecting such marathon splits at regular checkpoints, such as every "mile" on a track, or known distances, we get an idea how marathons were run throughout history. Such data may be used to estimate distances to historic checkpoints, notably those of the BAA, and to estimate lengths of races.

Many races cannot be neatly categorized. Some years ago a William Wilson was discovered to have run a marathon on Apr 22, 1909 in 2:46:00 2/5, better than the commonly accepted World Best (WB) then. This was enough to have the performance inserted in a WB Progression list, until I mentioned what else was reported, that the "time for half the distance [was] 1 hour and 5 minutes." I can't even estimate how long the race was but I'm not about to believe it was regulation, regardless of any such claim.

The first four marathons were in Europe. The fifth, still 1896, was over reportedly 25 miles from Stamford, CT to Columbia Oval. It was won in 3:25:55 3/5 by John J McDermott, who went on to win the inaugural BAA, the next year, in 2:55:10, over what was reported at the time to be 25 miles too. The NY course was never used again, but in 1908 much of it was run in a race from Rye to Columbus Circle won by Matt Maloney, an Irish immigrant, in just 2:36:26 1/5, "over roads deep and slippery in mud and slush [and] for the greater part covered with frozen snow... ." The NY Times continued:

> The course ... was carefully measured by a corps of civil engineers, accompanied by representatives of the Amateur Athletic Union, so there can be no doubt as to the Maloney record being made over the full distance of 26 miles and 385 yards.

However, this same paper soon changed its mind:

> there is grave question as to whether the Rye event was over the exact Marathon distance. The course was never officially measured, so that Maloney's figures cannot stand for a record.

There seems to me to be a considerable difference between these accounts! In any case, the performance does not appear in WB progression lists, as it should if it really were 26/385.

Some of us, however, find the next marathon over this course to be the most intriguing. It does appear in WB Progression lists, yet, curiously, appears to have been basically the reverse of the 1908 course, but passed through Rye and finished at Port

Chester (PC). It was the inaugural PC Marathon, 1925, won by "Whitey" Michelson in 2:29:01 4/5. The PC Daily Item made no claim as to the distance, but the NY Times repeatedly claimed it was 26/385, though oblivious to the performance then being a WB, which it would remain for 10 years. However, the Times also gave the 10-mile split, 57:14 1/5, from which I estimate the course to be barely 24 miles. Of course, descriptions of the race and the course help clarify the issue.

I'd be delighted to provide what descriptions I have to anyone interested in trying to measure these courses by just running a car over them. A car ride should suffice to discriminate between the reported and real distances. The St Louis Olympic Marathon, 1904, was reported as 40 k at the time. It was measured in 1981 by car over modern roads as 26.3 miles (42.3 k). I'd have expected it to be longer since it was won in 3:28:53, but it was fairly hot and the course terrible. I have other examples where drivers have shown historical courses to be well long or short.

The original BAA was set at 25 miles by cyclometer. Two years later, 1899, the finish was moved out of Irvington Oval to in front of the old BAA clubhouse (where it remained until 1965). The start was also moved then, from the box mill at Ashland to a railroad bridge. It was moved again in 1907, but apparently not again until 1924 to make the course what was thought to be 26/385 to conform to the newly adopted regulation length for an Olympic Marathon. The BAA then continued to be one of the Olympic Trials; indeed the best to run as it was so much shorter than the others.

Until recently I believed that the first two BAAs could have been 25 miles, that the 1899-1906 course might then have been only slightly shorter (40 k) and that only the 1907-23 course was much shorter. I am now led to believe that they were all basically the one length, as the BAA and others claim, but much shorter than the 24½ miles now claimed.

That the BAA was much shorter than the espoused 25 miles was known in the midwest no later than 1907, when Sid Hatch, Chicago's first great marathoner, returned from his first BAA. He had the experience to recognize a short course. New Yorkers realized it no later than 1912, when they measured the course by car following Mike Ryan, the winner, and reported 23.1 miles. I, too, ignored this when I first read it. I mentioned it for another reason to Roger Gynn, whom I hold to be the world marathon expert. He was the one who immediately recognized that this distance might be correct. I first thought he'd simply misinterpreted what I'd said, but rapidly realized that it should be easy to discriminate between whether the length of the 1907-23 course was 23.1 or 24½ miles. That's when the shock set in for me: realizing that the 23.1 was in the credible range while the 24½ wasn't even close. That was more than seven years ago. Since then there was one brief time when I thought I might have an explanation. Otherwise I have found nothing but corroboration for the 23.1. There is nothing sacrosanct about this distance. It just happens to be the only credible one I have read, and I have never read anything so definite outside that one NY Times report.

Contrary to popular myth, the 1924 course was not measured. What appears to have happened is that the 1923 course was believed to have been 24½ miles, so 1.7 miles was added at the start. This is confirmed by the 1924 race report in the NY Times,

which said that it took the leaders 9 minutes to get to the old start. In fact the 1924-26 course was about 24.8 miles. That's why Johnny Miles appeared to run as fast as he did in 1926.

The BAA course was never officially measured until the week before the 1927 race and found to be only 125 to 185 yards short, but reconstruction had just taken place or was still ongoing. In the 1927 heat Johnny Miles, defending, was just one former champion who fell victim in the early miles to the pools of tar from the newly laid seal. The course was not officially measured again for another thirty years, when it was found to be a good 1187 yards short.

I have seen two maps of the original course. One is in Bruce Kidd's biography of Tom Longboat and claims to be from the Boston Globe of Apr 19, 1897, but there appears to be no scale. The other is on p 18 of Hal Higdon's Boston, where the 1 through 20 intrigue me. They surely represent miles and remind me of the Marsh Wall problem Pete Riegel posed in the previous MN.

Anyone who studies early US marathons notices anomalous times, especially for the BAA. If the explanation were simply the overall slope of the course (rather than the absence of the final 1.4 miles!) then this advantage should have continued after 1926, since the start had been pushed steeper uphill from Ashland towards Hopkinton. It vanished immediately. Only three runners broke three hours in 1927, though it was hot. Amazingly, Clarence DeMar was totally fooled by what had happened. He thought he was declining in 1928 when in fact 1930 was his best year, when he turned 42, winning four of his six marathons. He then had a pair of 2:34:40s a couple of months apart. They were his career PBs. His next best times came at age 46. As smart as he was he never realized why he had had to run more than half an hour longer in winning the bronze at the 1924 Olympics, in the best of his three Olympic times, than he had in winning the BAA two years earlier.

If Michelson's time at the 1925 PC is disallowed as a WB then his time there in 1927 would be a WB, only it was seemingly over the same course. The following year the course was changed. Second-place DeMar then couldn't break three hours there. Yes, it was hot. Probably long then too. No wonder he retrospectively felt he was declining then.

WB Progression lists for the marathon are more to do with running short courses than the regulation distance. Unfortunately statisticians aren't about to abandon such lists. Then I find it enlightening to try to determine just how short some courses were. Others may be enlightened only by reports of people finding the original BAA course, under the reconstructions, and trying to measure it. Marathoning has come further than has been realized.

Early Olympic steeplechases are clearly now listed as being longer than they really were. At Paris, 1900, there were two races, of 5 and 8 laps. The track was 500 m, so these are now listed as 2500 and 4000 m. From the winning times and other considerations the races were surely inside the track. I estimate the lap at about 460 m. Similarly, the steeplechase at St Louis, 1904, is now listed as 2590 m when even 2590 yards is probably an exaggeration. The alternative is that some of these early runners would have beaten Volmari Iso-Hollo at the Olympics in the 1930s.

Malcolm R Heyworth (JHeyworth@aol.com)
6647 North Loron, Chicago, IL 60646-2709