EXPERIMENTS ON THE COMPARISON OF EMPIRICAL PROBABILITY WITH MATHEMATICAL PROBABILITY.

R. WOLF

Fourth experimental series
(Delivered 11 May 1850.)

In the well-known work "Un million de faits" I found the following, a note exciting my attention to the highest degree:

> "Let one trace on a plane surface a sequence of equally spaced parallel straight line; let one take a perfectly cylindrical needle, of a length a less than the constant interval d which separates the parallels, and let one project it at random a great number of times on the part of the surface which is covered by the lines. If one counts the total number q of times where the needle has been projected, and if one notes the number p of its encounters with any one of the parallels, the quantity $2aq : pd$ will express the ratio π of the circumference to the diameter with so much more exactly as the trials will have been multiplied. The error will be the smallest possible for a given number of trials, when the length a of the needle will be equal to the fourth of the product of the interval d of the divisions with the ratio π."

Without being able to find the original source and the reason for this note for the present, I decided to make a corresponding series of tests, since I by it, if also could not hope to obtain π, but at least new proofs for the regularity from a finite number of trials. On a board of about a square foot I drew a set of parallels at the distance of 45^{mm} and broke out from a knitting needle a bit of length 36^{mm}, – so that I had displayed within 1/100 exactly the most appropriate ratio according to the rule above. So equipped I made 3×50 trials, throwing the needle 100 times with each trial and noting each meeting with the parallels. With the first 50 trials I threw the needle parallel to the parallels of the board and with the second 50 perpendicularly, while I sought to cause all possible situations with the third 50 trials, thereby that I continually turned the board. Through this I received as number of the encounters of the needle with the parallels of the board in 100 throws so that from the beginning a so great regularity showed up that I believed use of the method of least squares for the computation of the means to be allowed. I received so for the first series of trials on average for 100 throws

$$21.76 \pm 0.64$$

throws in which the needle crossed the parallels. In the second series of trials

$$71.34 \pm 1.25$$

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Translated by Richard J. Pulskamp, Department of Mathematics and Computer Science, Xavier University, Cincinnati, OH. .

In the third test series

$$50.64 \pm 0.70$$

I compare the formula given above with it

$$\pi = \frac{2aq}{pd} \quad \text{or} \quad p = \frac{2aq}{d\pi}$$

so results for $a = 36$, $q = 100$ and $d = 45$

$$p = \frac{2 \cdot 36 \cdot 100}{45\pi} = 50.93$$

thus a number, which agrees within the found margin of error with the mean number coming from the third series of tests.

This result and the given overview of the trials speak clearly enough, and save me thus each further remark, — yes it seems to add hardly nothing, that I was sufficiently compensated thereby for the not small work.

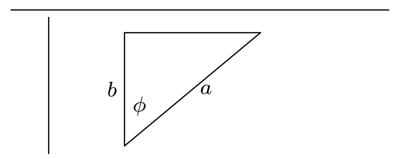
The number	In the first series of trials	The number	In the second series of trials	The number	In the third series of trials
13	1 Time	55	1 Time	41	1 Time
15	1	58	1	42	3
16	2	59	1	43	3
17	3	61	4	45	3
18	7	62	1	46	3
19	4	63	1	47	3
20	6	64	1	48	3
21	4	65	3	49	3
22	2	66	2	50	3
23	3	67	4	51	3
24	4	68	4	52	3
25	4	70	3	53	3
26	1	71	2	54	3
28	3	72	3	55	3
29	2	73	4	56	3
30	1	74	1	57	3
31	1	76	1	58	3
33	1	77	2	59	3
		79	3	60	3
		80	1	61	3
		84	2	62	3
		87	1	63	3
		88	1		
		89	1		
		90	1		
		92	1		
	50		50		50

Addendum to the fourth series of trials

(Delivered 7 December 1850.)

Professor Rudolf Merian in Basel shared a short time with me, after he had obtained my experiments concerning the number π, following with a simple derivation of that extraordinary formula for π:

“I retain the same letters, and put $b = a\cos\phi$, where ϕ describes the angle, which the needle a makes with the perpendiculars to the parallels. The probability, that the needle meets with one of the parallels at an angle ϕ, is apparent



$$\frac{b}{d} = \frac{a\cos\phi}{d}$$

The needle must be thrown such, that each angle ϕ, about which a deviates from the perpendiculars, is equal likely; thus the probability, that the angle falls between ϕ and $\phi + d\phi$ is

$$\frac{\mathrm{d}\phi}{\frac{1}{2}\pi} = \frac{2\mathrm{d}\phi}{\pi}$$

and the one, that instead of having an encounter in this position

$$\frac{a\cos\phi}{d} \cdot \frac{2\mathrm{d}\phi}{\pi} = \frac{2a\cos\phi\mathrm{d}\phi}{d\pi}$$

Now P is the probability, that an encounter takes place by random throws, so according to the well-known basic law of the probability calculus it is

$$P = \int_0^{\pi/2} \frac{2a\cos\phi\mathrm{d}\phi}{\pi d} = \frac{2a}{\pi d} \tag{1}$$

or, because by repeated trials $P = \lim \cdot \frac{p}{q}$ it is,

$$\frac{p}{q} = \frac{2a}{\pi d} \quad \text{or} \quad \pi = \frac{2aq}{pd} \tag{2}$$

“This calculation assumes, that $a < d$. If this does not occur, I put $d = a\cos\alpha$ like this. Now so long as $\phi > \alpha$, the meeting is certain, and therefore one has for $a > d$

$$P = \int_0^{\alpha} \frac{2\mathrm{d}\phi}{\pi} + \int_{\alpha}^{\frac{\pi}{2}} \frac{2a\cos\phi\mathrm{d}\phi}{\pi d} = \frac{2\alpha}{\pi} + \frac{2a}{\pi d}(1 - \sin\alpha) \tag{3}$$

The foregoing law thus takes place no longer, —it is caught first with $\alpha = 0$ or $d = a$ to become valid, whence

$$\frac{\pi}{2} = \frac{q}{p}.”$$

Professor Merian added then to his derivation yet the following comment:

“In that memorandum quoted by you it means: The error will be the least possible for a given number of trials, when $a = \frac{\pi d}{4}$, i.e. in other words, when $P = \frac{1}{2}$, or when the probability of the encounters and the non-encounters are equal, so the probability is greatest, that with the equal number of throws the difference $P - \frac{p}{q}$ is enclosed between certain limits. I hold this theorem for false; it seems to me, that one should find all the greater agreement between computation and observation should take place, ever greater P is made, — thus the greatest agreement for $a = d$ or for $P = \frac{2}{\pi}$.”

I cannot agree with this remark; because on the one hand it seems to me to contradict each experience, that the best value lies at the limit of the altogether possible values, — on the other hand there stands contrary a new experimental series, that since then I made to these purposes.

Retaining the old system of parallels with spacings of 45mm, I chose for the trials namely a needle of length 45mm likewise, so that according to Mister Merian's opinion an especially satisfactory outcome was expected to me. I made again previously 50 experimental series of 100 throws each and found from them them

$$p = \frac{1}{50}\left[\begin{array}{l} 57+58+59+5\cdot 60+2\cdot 61+4\cdot 62+8\cdot 63+3\cdot 64+2\cdot 65 \\ +3\cdot 66+5\cdot 67+5\cdot 68+4\cdot 69+2\cdot 70+2\cdot 71+73+74 \end{array}\right]$$
$$= 64.96 \pm 0.56$$

so that I find,

$$\frac{p}{q} = 0.6496 \text{ instead of } P = 0'.6366 \text{ or } P - \frac{p}{q} = -0.0130,$$

while the earlier trials had given,

$$\frac{p}{q} = 0.5064 \text{ instead of } P = 0.5093 \text{ or } P - \frac{p}{q} = +0.0029,$$

thus more than 4 times more narrow limits were extracted. An investigation of the needle and the applied system of parallels made on that again, showed me, that the needle was exactly 45^{mm} long and the parallels varied only within $1/10^{\text{mm}}$ of 45^{mm}, in fact quite even in $+$ and in $-$. If I would accept in addition, in the most unfavorable case $d = 44.9^{\text{mm}}$, so will become $\alpha = \arccos\frac{d}{a} = 3^\circ 49' = 0,0666$, and according to 3 will correspond

$$P = 0,6367 \text{ what still } P - \frac{p}{q} = -0,0129,$$

thus almost as much as a while ago. How should it be explained now, that an outcome so much more worse would have been achieved with the same material, with the same caution and by the same observer directly under more favorable circumstances? One sees however the ratio, under which these trials were made, as an unfavorable one, thus the large deviation is explained by itself, — in such case even far more trials would have been necessary. That also with these trials there was an oscillation to the right, it results from it, that is given from this, that resulted in,

the first	10	series of trials	$0,6570$
	20		$0,6520$
	30		$0,6510$
	40		$0,6530$
	50		$0,6496$

— but since coincidentally the first oscillation was a little large and on the other hand the directing power was smaller, so the rest position could not be achieved yet in 50 vibrations. Of the 50 numbers, originated out of those p as mean, namely 22 were too small and 28 too large, — with the first 10 however only 4 too small and 6 too great, in fact the least did not go under 60, while the largest was 73.