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LVI. On “random scattering” of points on a surface

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is established by my own experiments ; (2) the fact that there is a sudden fall in rigidity at a temperature about red heat—this is established by Mr. Tomlinson's Experiment I. as well as by similar experiments of my own ; (3) the assumption (? fact) that a wire of even 1 mm. diam. is not heated uniformly throughout its thickness, but that the inner part is hotter than the parts near the surface ; and lastly that the rigidity of the iron at the high temperature of reglow is less than the rigidity of iron at not so high a temperature before reglow.

A similar line of argument will explain the results recorded in Experiments VI. and VII.

I have made a great number of experiments on the return of magnetic properties in cooling iron ; and can at present only say with certainty that the matter is not so simple as appears from Experiment IV. of Mr. Tomlinson's paper. The relation between the reglow and return of magnetic properties varies with the specimens of iron and steel used. I have found some few specimens in which the reglow takes place in the middle of the return of magnetic properties, so that the galvanometer indication is *two* kicks, the first very small, just before reglow, the second and main kick after reglow, as if the metal was becoming magnetic, when the rise of temperature, coincident with reglow, caused it to pause for an instant. In general, however, the reglow precedes the return of magnetic properties, though by intervals which vary with the specimens observed. I hope to have more definite results when I have tested the numerous specimens of steel and iron which I have now in my possession.

LVI. On "Random Scattering" of Points on a Surface. By JOSEPH KLEIBER, *Privat-Docent in the Imperial University of St. Petersburg.**

IN a paper published in the Philosophical Magazine [ser. 3], vol. xxxvii. pp. 401–427 †, Prof. Forbes set forth some objections against the application of the principles of the mathematical theory of probabilities to the question of the distribution of stars in the sky. Mr. Todhunter quotes this paper in his well-known 'History of the Theory of Probabi-

* Communicated by the Author.

† "On the Alleged Evidence for a Physical Connexion between Stars forming Binary or Multiple Groups, deduced from the Doctrine of Chances."

lities,' agreeing apparently with the views of its author. He writes :—

"The late Prof. Forbes wrote a very interesting criticism on Mitchell's memoir. He objects with great justice to Mitchell's mathematical calculations, and he altogether distrusts the validity of the inference drawn from these calculations" (History &c. p. 334).

Relying on so high an authority, the same paper is referred to by J. Jevons in his 'Principles of Science.' He says :—

"The calculations of Mitchell have been called in question by the late James D. Forbes, and Mr. Todhunter vaguely countenances his objections, otherwise I should not have thought them of much weight" (Principles &c. vol. i. p. 286).

Now the article in question contains a very erroneous conception of the "law of great numbers," and its application to the investigation of accidental distribution, and the objections of Prof. Forbes against this application, together with the experiments made by him on the random distribution of grains thrown on a chess-board, furnish, on the contrary, a very good argument for and illustration of the very same views they are intended to invalidate.

Misinterpretations of the laws of "random scattering" like those of Prof. Forbes are not very rare, especially in the writings of statisticians, so I thought it worth while to show the error of these misconceptions.

It is a common error to confound random scattering with uniform distribution. It is true that the most probable result of a series of drawings from an urn containing an equal number of black and white balls will be an equal number of both ; but this result is in itself very improbable, because this is but one out of a great number of possible events, all giving unequal distribution, less probable individually, but more probable in sum, than the most probable result. Also, the most probable distribution of points on a surface, if scattered at random, is a uniform one, but this is very improbable. The *probable result* of a random scattering is therefore *not a uniform* distribution.

Let n points be distributed on a surface divided into m equal parts (squares). Then, although the probable number of points in each square is $\frac{n}{m}$, it is very probable that there will be squares containing less than $\frac{n}{m}$ points, while others will contain more.

Let us find the probable number of squares containing a given number of points. The probability of a given point

being in a given square is $\frac{1}{m}$; the inverse probability of its not being in this square is

$$1 - \frac{1}{m}.$$

Hence the probability that this square contains i points and no more is

$$p_i = \frac{n!}{i!(n-i)!} \left(\frac{1}{m}\right)^i \left(1 - \frac{1}{m}\right)^{n-i};$$

and the probable number of squares containing i points will be mp_i , or

$$m \cdot \frac{n!}{i!(n-i)!} \left(\frac{1}{m}\right)^i \left(1 - \frac{1}{m}\right)^{n-i} \quad \dots \quad (1)$$

We will apply these formulæ to the experiments described by Prof. Forbes. He writes:—

“I have thought it worth while to test a little by simple experiment the differences to which ‘mere chance’ gives rise in the grouping of bodies dispersed over a surface, by a method of ‘random scattering,’ which I conceive to be as nearly as possible analogous to Mitchell’s idea of chance as affecting the placing of the stars. . . . I placed a chess-board, having, as usual, sixty-four squares, on the floor, and I provided a large sieve into which I put a quantity of grains of rice, which did not fall through the sieve until it was somewhat shaken. I then shook the sieve at a considerable height above the chess-board until it was pretty well scattered over with grains. . . . The following diagrams contain the results of five experiments, the number of grains which fell on each of the sixty-four squares being counted and registered. . . . In these experiments we observe that the most loaded squares contain from nearly two to more than four times the *average* number of grains, whilst in four out of five experiments one or more squares were vacant. . . . If we were to take any one of these experiments, and attempt to calculate the antecedent probability of the grains so arranging themselves on Mitchell’s supposition, we should unquestionably find numerical chances far greater against these configurations being the result of accident, than those on which we are told that we form our most certain ordinary judgments. Thus, if an experimental argument may be admitted, the reasoning of Mitchell and his followers is altogether fallacious.”

But if we calculate the probable number of squares. *con-*
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taining a given number of grains in these experiments, putting in our formula (1) $m=64$, n =total number of grains, we find, on the contrary, deductively, a distribution very similar to that obtained by Prof. Forbes in his experiments.

The following diagram represents the results of Prof Forbes's first experiment.

Experiment I.

4	3	3	6	2	4	2	1
4	4	4	3	1	4	3	1
6	2	3	2	3	5	3	2
3	0	4	3	4	7	1	3
0	4	4	4	0	3	4	4
3	2	3	3	2	5	5	5
2	3	3	0	3	5	1	1
1	3	3	2	1	2	0	2

In the following table I give the comparisons between the theoretical and observed numbers of squares containing given numbers of points.

Number of grains in a square.	Number of squares.	
	Observed.	Calculated.
0	5	3.6
1	8	10.4
2	11	15.1
3	19	14.4
4	13	10.3
5	5	5.8
6	2	2.8
7	1	1.1
Above . . .	0	0.4

The observed and calculated values agree sufficiently. A still more close agreement is presented by Experiment II.

Experiment II.

2	7	4	1	6	5	5	3
5	3	3	5	6	7	2	8
7	6	3	2	7	6	1	4
4	8	8	3	4	5	2	4
0	5	6	7	4	2	3	7
1	6	3	4	6	8	5	2
2	4	4	7	5	3	4	8
2	4	5	5	3	3	3	4

Number of grains in a square.	Number of squares.	
	Observed.	Calculated.
0	1	0·8
1	3	3·4
2	8	7·6
3	11	11·2
4	12	12·4
5	10	10·9
6	7	8·0
7	7	5·0
8	5	3·0
Above	0	1·7

We see from these diagrams that the experiments of Prof. Forbes by no means contradict the formulæ of probabilities applied to the question of distribution by chance.

But these experiments are not quite free from objections. It might be stated that by throwing grains of rice on a chess-board we must obtain a *more uniform* rather than an accidental distribution, as it is evident that the number of grains being augmented so as to cover the whole surface of the chess-board, we shall have a pretty equal number of grains on each square. Another arrangement of the same experiment may be less objectionable, and I tried it, in order to give a more complete illustration of the application of the principle of random scattering.

Taking a table of logarithms with seven places, I con-

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sidered the last two figures of the logarithms of a series of consecutive numbers, as designating the coordinates of points, which I supposed distributed within a square divided, for convenience, into 100 equal parts ; so that the sixth place of the mantissa designated the line, the seventh place the column in which the point was to be put. Two hundred and fifty points were distributed in this manner, and the result was as follows :—

6	2	1	2	1	1	3	2	2	4
3	0	2	3	4	2	5	1	2	5
2	1	5	3	1	3	5	0	1	2
3	7	4	6	1	1	4	1	0	0
1	1	2	3	3	2	4	0	6	2
1	4	2	3	2	1	3	2	2	1
3	4	2	4	3	5	4	2	1	1
1	2	2	2	2	2	1	3	1	4
1	5	2	5	1	3	3	4	0	1
0	2	4	3	5	2	2	6	4	2

Applying to this case the formula above, in which we put

$$m=0, \quad n=50,$$

we obtain the following result :—

Number o points in a square.	Number of squares.	
	Observed.	Calculated.
0	7	8.1
1	23	20.5
2	28	25.7
3	17	21.5
4	13	13.4
5	7	6.7
6	4	2.8
7	1	1.0
Above . . .	0	0.4

Summing up our conclusions, we see that, contrary to the opinion of Prof. Forbes, the theory of probabilities does not affirm that a "perfectly uniform and symmetrical disposition of the stars over the sky would (if possible) be that which could alone afford no evidence of causation or any interference with the laws of 'random.'" On the contrary, I have endeavoured to show that this theory gives a sufficient account of the possible irregularities of distribution such as those observed by Prof. Forbes in his experiments, or those presented by the stars in the sky. The ingenious argumentation of Mitchell respecting the improbability of the observed groups of stars being merely accidental is by no means in contradiction with the true principles of probability, and the exact meaning of the "law of great numbers."

St. Petersburg, October 2, 1887.

LVII. *Intelligence and Miscellaneous Articles.*

MAGNETIC CIRCULAR POLARIZATION IN COBALT AND NICKEL.

BY H. E. J. G. DU BOIS.

IF we consider an infinitely extended plate of any thickness in a uniform magnetic field normal to its plane sides; the material being supposed homogeneous, isotropic, and devoid of retentiveness, the induced magnetization $\mathfrak{I}$ is given by the equation

$$\mathfrak{I} = \frac{1}{4\pi + 1/\kappa} \mathfrak{H}_a,$$

where $\mathfrak{H}_a$ is the original field, κ the susceptibility. From Rowland's data for "Burden's best" iron, cast cobalt, and cast nickel the relation between κ (an essentially variable quantity in the case of these metals), and $\mathfrak{I}$ is known, and curves are accordingly plotted corresponding to the above equation. Their shape is represented

with great approximation in the annexed diagram. The ordinate of the singular point is the maximum $\mathfrak{I}_m$ given by Rowland, its abscissa is $4\pi \mathfrak{I}_m$; the absolute values stand as follows:—

	"Burden's best" iron.	Cast cobalt.	Cast nickel.
Ordinate	1390	880	494 C.G.S.
Abscissa	17440	11060	6200 C.G.S.