

PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From June 16, 1870, to June 15, 1871.

VOL. XIX.

LONDON:
PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

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after each minimum, and concludes by a simple proportion of the remainder. He finds that the curve *ascends* more rapidly than it *descends*—the ascent taking in the mean 3·7 years, the descent lasting 7·4 years. We have established these data far more reliably in our last paper; and our curve gives 3·52 years for the ascent, 7·54 years for the descent (average of the three periods). Professor Wolf also thinks that although a single period may differ essentially in its character and form from the mean, still, on the whole, if the descent is retarded, the ascent in the same period is also retarded; if the former is accelerated, the latter is also accelerated. This is not quite borne out by our curve. He also overlooks the secondary maximum, which may lead to great conclusions if more investigated together with other matters.

M. Fritz comes to the following conclusions:—

1. The connexion between sun-spots and auroral and magnetic disturbances indicates an external cause, to be sought in planetary configurations.

2. The relative influence of the planets must be exerted in the following order:—Jupiter (greatest), Venus, Mercury, Earth, Saturn.

3. This influence cannot entirely depend on the time of rotation; but changes in the *magnetic* axes of these planets may have the most determining effect.

4. Investigating the comparative influences of them singly and together (as far as possible), at the times of conjunction and quadrature, he finds the greatest coincidence of maxima of sun-spots with the time when *Jupiter* and *Saturn* are in quadrature; and the greatest coincidence of minima when these planets are in conjunction.

5. There is also (a minor) coincidence of maxima when *Jupiter* and *Venus* are in quadrature.

There is also an extension of the paper for finding the connexions with auroras, and a statement that every 27·7 days there seems to be a monthly maximum, which may probably be explained (according to Fritz) by the tendency of a particular solar meridian to spot-formations, depending upon the presence of an intra-Mercurial planet.

March 30, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

The following communications were read:—

- I. "Experiments in Pangenesis, by Breeding from Rabbits of a pure variety, into whose circulation blood taken from other varieties had previously been largely transfused." By FRANCIS GALTON, F.R.S. Received March 23, 1871.

Darwin's provisional theory of Pangenesis claims our belief on the ground that it is the only theory which explains, by a single law, the numerous

phenomena allied to simple reproduction, such as reversion, growth, and repair of injuries. On the other hand, its postulates are hypothetical and large, so that few naturalists seem willing to grant them. To myself, as a student of Heredity, it seemed of pressing importance that these postulates should be tested. If their truth could be established, the influence of Pangenesis on the study of heredity would be immense; if otherwise the negative conclusion would still be a positive gain.

It is necessary that I should briefly recapitulate the cardinal points of Mr. Darwin's theory. They are (1) that each of the myriad cells in every living body is, to a great extent, an independent organism; (2) that before it is developed, and in all stages of its development, it throws "gemmules" into the circulation, which live there and breed, each truly to its kind, by the process of self-division, and that, consequently, they swarm in the blood, in large numbers of each variety, and circulate freely with it; (3) that the sexual elements consist of organized groups of these gemmules; (4) that the development of certain of the gemmules in the offspring depends on their consecutive union, through their natural affinities, each attaching itself to its predecessor in a regular order of growth; (5) that gemmules of innumerable varieties may be transmitted for an enormous number of generations without being developed into cells, but always ready to become so, as shown by the almost insuperable tendency to feral reversion, in domesticated animals.

It follows from this, and from the general tenor of Mr. Darwin's reasoning and illustrations, that two animals, to outward appearance of the same pure variety, one of which has mongrel ancestry and the other has not, differ solely in the constitution of their blood, so far as concerns those points on which outward appearance depends. The one has none but gemmules of the pure variety circulating in his veins, and will breed true to his kind; the other, although only the pure variety of skin-gemmules happens to have been developed in his own skin, has abundance of mongrel gemmules in his blood, and will be apt to breed mongrels. It also follows from this that the main stream of heredity must flow in a far smaller volume from the developed parental cells, of which there is only one of each variety, than from the free gemmules circulating with the blood, of which there is a large number of each variety. If a parental developed cell bred faster than a free gemmule, an influx of new immigrants would gradually supplant the indigenous gemmules; under which supposition, a rabbit which, at the age of six months, produced young which reverted to ancestral peculiarities, would, when five years old, breed truly to his individual peculiarities; but of this there is no evidence whatever.

Under Mr. Darwin's theory, the gemmules in each individual must therefore be looked upon as entozoa of his blood, and, so far as the problems of heredity are concerned, the body need be looked upon as little more than a case which encloses them, built up through the development of some of their number. Its influence upon them can be only such as

would account for the very minute effects of use or disuse of parts, and of acquired mental habits being transmitted hereditarily.

It occurred to me, when considering these theories, that the truth of Pangenesis admitted of a direct and certain test. I knew that the operation of transfusion of blood had been frequently practised with success on men as well as animals, and that it was not a cruel operation—that not only had it been used in midwifery practice, but that large quantities of saline water had been injected into the veins of patients suffering under cholera. I therefore determined to inject alien blood into the circulation of pure varieties of animals (of course, under the influence of anæsthetics), and to breed from them, and to note whether their offspring did or did not show signs of mongrelism. If Pangenesis were true, according to the interpretation which I have put upon it, the results would be startling in their novelty, and of no small practical use; for it would become possible to modify varieties of animals, by introducing slight dashes of new blood, in ways important to breeders. Thus, supposing a small infusion of bull-dog blood was wanted in a breed of greyhounds, this, or any more complicated admixture, might be effected (possibly by operating through the umbilical cord of a newly born animal) in a single generation.

I have now made experiments of transfusion and cross circulation on a large scale in rabbits, and have arrived at definite results, negating, in my opinion, beyond all doubt, the truth of the doctrine of Pangenesis.

The course of my experiments was as follows:—Towards the end of 1869, I wrote to Dr. Sclater, the Secretary of the Zoological Society, explaining what I proposed to do, and asking if I might be allowed to keep my rabbits in some unused part of the Gardens, because I had no accommodation for them in my own house, and I was also anxious to obtain the skilled advice of Mr. Bartlett, the Superintendent of the Gardens, as to their breed and the value of my results. I further asked to be permitted to avail myself of the services of their then Prosector, Dr. Murie, to make the operations, whose skill and long experience in minute dissection is well known. I have warmly to thank Dr. Sclater for the large assistance he has rendered to me, in granting all I asked, to the full, and more than to the full; and I have especially to express my obligations to the laborious and kind aid given to me by Dr. Murie, at real inconvenience to himself, for he had little leisure to spare. The whole of the operations of transfusion into the jugular vein were performed by him, with the help of Mr. Oscar Fraser, then Assistant Prosector, and now appointed Osteologist to the Museum at Calcutta, I doing no more than preparing the blood derived from the supply-animal, performing the actual injection, and taking notes. The final series of operations, consisting of cross-circulation between the carotid arteries of two varieties of rabbits, took place after Dr. Murie had ceased to be Prosector. They were performed by Mr. Oscar Fraser in a most skilful manner, though he and I were still further indebted, on more than one occasion, to Dr. Murie's advice and assistance. My part in this series was limited to in-

serting and tying the canulæ, to making the cross-connexions, to recording the quality of the pulse through the exposed arteries, and making the other necessary notes.

The breed of rabbits which I endeavoured to mongrelize was the "Silver-grey." I did so by infusing blood into their circulation, which I had previously drawn from other sorts of rabbits, such as I could, from time to time, most readily procure. I need hardly describe Silver-grey rabbits with minuteness. They are peculiar in appearance, owing to the intimate mixture of black and grey hairs with which they are covered. They are never blotched, except in the one peculiar way I shall shortly describe; and they have never lop ears. They are born quite black, and their hair begins to turn grey when a few weeks old. The variations to which the breed is liable, and which might at first be thought due to mongrelism, are white tips to the nose and feet, and also a thin white streak down the forehead. But these variations lead to no uncertainty, especially as the white streak lessens or disappears, and the white tips become less marked, as the animal grows up. Another variation is much more peculiar: it is the tendency of some breeds to throw "Himalayas," or white rabbits with black tips. From first to last I have not been troubled with white Himalayas; but in one of the two breeds which I have used, and which I keep carefully separated from each other, there is a tendency to throw "sandy" Himalayas. One of these was born a few days after I received the animals, before any operation had been made upon them, and put me on my guard. A similar one has been born since an operation. Bearing these few well-marked exceptions in mind, the Silver-grey rabbit is excellently adapted for breeding-experiments. If it is crossed with other rabbits, the offspring betray mongrelism in the highest degree, because any blotch of white or of colour, which is not "Himalayan," is almost certainly due to mongrelism; and so also is any decided change in the shape of the ears.

I shall speak in this memoir of litters connected with twenty silver-grey rabbits, of which twelve are does and eight are bucks; and eighteen of them have been submitted to one or two of three sorts of operations. These consisted of:—

(1) Moderate transfusion of partially defibrinized blood. The silver-grey was bled as much as he could easily bear; that was to about an ounce, a quantity which bears the same proportion to the weight of his body (say 76 oz.) that 2 lbs. bears to the weight of the body of a man (say 154 lbs.); and the same amount of partially defibrinized blood, taken from a killed animal of another variety, was thrown in in its place. The blood was obtained from a yellow, common grey, or black and white rabbit, killed by dividing the throat, and received in a warmed basin, where it was stirred with a split stick to remove part of the fibrine. Then it was filtered through linen into a measuring-glass, and thence drawn up with a syringe, graduated into drachms; and the quantity injected was noted.

(2) The second set of operations consisted in a large transfusion of wholly

defibrinized blood, which I procured by whipping it up thoroughly with a whisk of rice-straw ; and, in order to procure sufficient blood, I had on one occasion to kill three rabbits. I alternately bled the silver-grey and injected, until in some cases a total of more than 3 ounces had been taken out and the same quantity, wholly defibrinized, had been thrown in. This proportion corresponds to more than 6 lbs. of blood in the case of a man.

(3) The third operation consisted in establishing a system of cross-circulation between the carotid artery of a silver-grey and that of a common rabbit. It was effected on the same principle as that described by Addison and Morgan (*Essay on Operation of Poisonous Agents upon the Living Body*. Longman & Co., 1829), but with more delicate apparatus and for a much longer period. The rabbits were placed breast to breast, in each other's arms, so that their throats could be brought close together. A carotid of each was then exposed ; the circulation in each vessel was temporarily stopped, above and below, by spring holders ; the vessels were divided, and short canulæ, whose bores were larger than the bore of the artery in its normal state, were pressed into the mechanically distended mouths of the arteries ; the canulæ were connected cross-wise ; the four spring holders were released, and the carotid of either animal poured its blood direct into the other. The operation was complicated, owing to the number of instruments employed ; but I suspended them from strings running over notched bars, with buttons as counterpoises, and so avoided entanglement. These operations were exceedingly successful ; the pulse bounded through the canulæ with full force ; and though, in most cases, it began to fall off after ten minutes or so, and I was obliged to replace the holders, disconnect the canulæ, extract the clot from inside them with a miniature corkscrew, reconnect the canulæ, and reestablish the cross-flow two, three, or more times in the course of a single operation, yet on two occasions the flow was uninterrupted from beginning to end. The buck rabbit, which I indicate by the letter O, was $37\frac{1}{2}$ minutes in the most free cross-circulation imaginable with his " blood-mate," a large yellow rabbit. There is no mistaking the quality of the circulation in a bared artery ; for, when the flow is perfectly free, the pulse throbs and bounds between the finger and thumb with a rush, of which the pulse at the human wrist, felt in the ordinary way, gives an imperfect conception.

These, then, are the three sorts of operations which I have performed on the rabbits ; it is convenient that I should distinguish them by letters. I will therefore call the operation of simply bleeding once, and then injecting, by the letter *u* ; that of repeated bleedings and repeated injections by the letter *w* ; and that of cross-circulation by the letter *x*.

In none of these operations did I use any chemical means to determine the degree to which the blood was changed ; for I did not venture to compromise my chances of success by so severe a measure ; but I adopted the following method of calculation instead :—

I calculate the change of blood effected by transfusion, or by cross-circulation, upon moderate suppositions as to the three following matters:—

(1) The quantity of blood in a rabbit of known weight.

(2) The time which elapses before each unit of incoming blood is well mixed up with that already in the animal's body.

(3) The time occupied by the flow, through either carotid, of a volume of blood equal to the whole contents of the circulation.

As regards 1, the quantity of blood in an animal's body does not admit, by any known method, of being accurately determined. I am content to take the modern rough estimate, that it amounts to one-tenth of its total weight. If any should consider this too little, and prefer the largest estimate, viz. that in Valentin's 'Repertorium,' vol. iii. (1838), p. 281, where it is given for a rabbit as one part in every 6·2 of the entire weight, he will find the part of my argument which is based on transfusion to be weakened, but not overthrown, while that which relies on cross-circulation is not sensibly affected.

As regards 2, the actual conditions are exceedingly complex; but we may evade their difficulty by adopting a limiting value. It is clear that when only a brief interval elapses before each unit of newly infused blood is mixed with that already in circulation, the quality of the blood which, at the moment of infusion into one of the cut ends of the artery or vein, is flowing out of the other, will be more alienized than if the interval were longer. It follows that the blood of the two animals will intermix more slowly when the interval is brief than when it is long. Now I propose to adopt an extreme supposition, and to consider them to mix *instantaneously*. The results I shall thereby obtain will necessarily be less favourable to change than the reality, and will protect me from the charge of exaggerating the completeness of intermixture.

As regards 3, I estimate the flow of blood through either carotid to be such that the volume which passes through it in ten minutes equals the whole volume of blood in the body. This is a liberal estimate; but I could afford to make it twice or even thrice as liberal, without prejudice to my conclusions.

Upon the foregoing data the following Table has been constructed. The formulæ are:—Let the blood in the Silver-grey be called a , and let its volume be V , and let the quantity u of alien blood be thrown in at each injection, then the quantity of blood a remaining in the Silver-grey's circulation, after n injections,

$$= V \left(1 - \frac{u}{V} \right)^n.$$

If the successive injections be numerous and small, so as to be equivalent to a continuous flow, then, after w of alien blood has passed in, the formula becomes $V \cdot e^{-\frac{w}{V}}$.

A comparison of the numerical results from these two formulæ shows that no sensible difference is made if (within practicable limits) few and large, or many and small, injections are made, the total quantity injected being the same.

In cross-circulation the general formula is this:—If V' be the volume of blood in the other rabbit, after w of alien blood has passed through either canula, the quantity of blood a remaining in the Silver-grey exceeds *

$$\frac{V}{V+V'} \left\{ V + V' e^{-\left(\frac{1}{V} + \frac{1}{V'}\right)w} \right\}. \text{ This becomes } \frac{V}{2} \left\{ 1 + e^{-\frac{2w}{V}} \right\} \text{ when } V=V';$$

also, when V' is infinite, it gives the formula already mentioned for injection by a continuous flow of purely alien blood.

TABLE I.

(Contents of circulation of Silver-grey Rabbit=100.)

Quantity of blood infused.	Maximum percentage of original blood remaining after					Period, in minutes, during which the continuous flow through each carotid has lasted.
	Successive injections of purely alien blood, each = $\frac{100}{12}$.		Continu-ous flow of purely alien blood.	Cross-circulation.		
	Number of injections.			Rabbits of equal size.	Blood-mate $\frac{1}{10}$ larger than the Silver-grey.	
25	3	77	78	80	80	2½
50	6	59	61	68	68	5
75	9	46	47	61	60	7½
100	12	35	37	56	55	10
125	15	27	29	54	52	12½
150	18	21	22	52	51	15
175	21	16	17	51	50	17½
200	24	12	14	51	49	20
300	36	4	5	50	48	30
400	48	1	2	50	48	40
infinite	infinite	0	0	50	48	infinite.

I now give a list (Table II.) of the rabbits to which, or to whose blood-mates, I shall have to refer. Every necessary particular will be found in the Table:—the weight of the rabbits; the estimated weight of blood in their veins; the operations performed on them, whether u , w , or x ; the particulars of those several operations; the estimated percentage of alien blood that was substituted for their natural blood; and lastly, the colour, size, and breed of their blood-mates.

* I am indebted to Mr. George Darwin for this formula.

TABLE II.

Silver-grey Does.	Weight of rabbit.	Estimated weight of blood.	Nature of operation*.	Drachms infused, and period of cross-circulation.	Percentage of alienized blood.	Colour &c. of blood-mate.
	lbs. oz.	drachms.				
A	5 9	79	<i>u</i>	9	11	{ Common grey and white.
B	5 13	82	<i>x</i>	{ 10 min. perfect, 15 or 20 very good.	12	Yellow, large.
C	5 8	78	<i>u</i>	9.5	12	Albino, large.
D	5 4	75	<i>u</i>	8.5	12	Himalaya.
E	4 9	58	<i>x</i>	{ 13 min. good, 14 poor.	14	Common grey.
F	4 13	61	<i>u</i>	7.7	10	{ Black and white, large.
G	4 11	60	<i>w</i>	{ 25.5, in 6 injections.	35	{ Grey and black, speckled.
H	<i>x</i>	{ 15 min. perfect, 15 very good.	50	Common grey.
I†	<i>x</i>	{ 16 min. perfect, not much more.	nearly 50	{ Common grey and white.
J†	<i>x</i>	35 min. perfect.	...	{ Yellow, brown mouth (? Himalaya).
S	<i>x</i>	{ too unsuccessful to be worth counting.	? any.	{ Angora, fawn and white.
T	None.	None.
Bucks.						
K	4 14	62	<i>u</i>	9	14	{ Yellow, brown mouth.
L	4 13	61	<i>w</i>	{ 14, in 4 injections, total.	32	Yellow and white.
M	4 0	51	<i>w</i>	{ 24.5, in 6 injections, total.	14	Black and white.
N	4 9	58	<i>u</i>	7.5	13	{ Angora, grey and white, red eyes.
O (son of C (<i>u</i>) by K (<i>u</i>))	<i>x</i>	37½ min. perfect.	50	Yellow.
Q†	<i>x</i>	{ 15 min. perfect, 15 very good.	50	Yellow and white.

* Note (to 4th column).—*u* means simple transfusion, by one copious bleeding, and then injecting; *w* means compound transfusion by successive bleedings and successive injections; *x* means cross-circulation.

† These rabbits belong to a breed liable to throw "Sandy" Himalayas.

TABLE III.

Litters subsequent to first transfusion. Both parents Silver-greys. Average proportion of alienized blood in either parent = $\frac{1}{8}$; therefore in young $\frac{1}{8}$ also.

Out of	By	Number and character of litters.
A	K	4 true Silver-greys.
A	M	5 ditto, but 1 had a white foot to above knee.
B	K	5 true Silver-greys.
C	K	6 ditto.
D	K	4 ditto.
E	L	6 ditto.
		—
		30 all true Silver-greys, except possibly one instance.

Litters subsequent to second transfusion of buck. Both parents Silver-greys. Average proportion of alienized blood in young about $\frac{1}{4}$.

Out of	By	Number and character of litters.
A	M	6 true Silver-greys.

Litters subsequent to cross-circulation of buck only, the does being O or u. Both parents Silver-greys. Average proportion of blood in young between $\frac{1}{4}$ and $\frac{1}{8}$.

Out of	By	Number and character of litters.
S	O	5 true Silver-greys.
C	O	5 ditto.
T	O	3 ditto.
		—
		13 all Silver-greys.

Litters subsequent to cross-circulation of both parents (Silver-greys). Average proportion of alienized blood in young fully $\frac{1}{2}$.

Out of	By	Number and character of litters.
B	O	3 true Silver-greys.
H	O	7 ditto.
H	O	7 ditto.
I*	P*	6 ditto.
J*	Q*	6 ditto, all but one, a sandy Himalaya.
J*	P*	8 true Silver-greys.
		—
		37 36 Silver-greys, 1 Himalaya.

* These rabbits belong to a breed liable to throw "Sandy" Himalayas.

Litters subsequent to cross-circulation of both parents (common rabbits).

Average proportion of alienized blood in young a little less than $\frac{1}{2}$.

Out of blood-mate to	By blood-mate to	Number and character of litters.
E	R	8 none Silver-grey, all like father or mother.
E	Q*	5 ditto.
G	O	9 ditto.
I*	Q*	8 ditto.
J*	Q*	8 ditto.
		—
		38 none Silver-greys.

In another list (Table III.) I give particulars of all the litters I have obtained from these rabbits, classified according to the operations which the parents had previously undergone.

I will now summarize the results. In the first instance I obtained five does (A, B, C, D, and E) and three bucks (K, L, and M) which had undergone the operation which I call *u*, and which had in consequence about $\frac{1}{11}$ of their blood alienized. I bred from these †, partly to see if I had produced any effect by the little I had done, and chiefly to obtain a stock of young rabbits which would be born with $\frac{1}{8}$ of alien gemmules in their veins, and which, when operated upon themselves, would produce descendants having nearly $\frac{1}{4}$ alienized blood (the exact proportion is $1 - (1 - \frac{1}{8})^2 = \frac{15}{64}$). I obtained thirty young ones in six litters; and they were all true silver-greys, except, possibly, in one instance (out of the doe A (*u*) by the buck M (*u*)), where one, of a litter of five, had a white fore leg, the white extending to above the knee-joint. This white leg gave me great hopes that Pangenesis would turn out to be true, though it might easily be accounted for by other causes; for my stock were sickly (both those on which I had not operated and those on which I had suffering severely from a skin disease), and it was natural under those circumstances of ill health that more white than usual should appear in the young.

Having, then, had experience in transfusion, and feeling myself capable of managing a more complicated operation without confusion, I began the series which I call *w*. I left my old lot of does untouched, but obtained one new doe (G(*w*)), which had undergone the last operation, and three bucks (K (*u, w*), M (*u, w*), N (*u, w*)) which had undergone both operations, *u* and *w*. On endeavouring to breed from them, the result was unexpected, they appeared to have become sterile. The bucks were as eager as possible for the does; but the latter proving indifferent, I was unable to testify to their union having taken place; so I left them in pairs, in the same hutch, for periods of three days at a time. Attempts were made in this

* These rabbits belong to a breed liable to throw "Sandy" Himalayas.

† I always allowed the bucks to run for awhile with waste does before commencing the breeding-experiments, that all old reproductive material might be got rid of.

way, to breed from them in seven instances; and five of them were utter failures. One case was quite successful; and that, fortunately, was of the same pair (A (u) and M (u, w)) which, under the u operation, had bred the white-footed young one. This time, the offspring (six in number) were pure silver-greys. The last case was unfortunate. The doe (E (u)) had been once sterile to its partner (N (u, w)), and she had been put again in the same hutch with him for a short period, but was thought not to have taken him. She was shortly afterwards submitted to the operation x . From this she had nearly recovered when she brought forth an aborted litter and died. I was absent from town at the time; but Mr. Fraser, who examined them, wrote to say he fully believed that some were pied; if so, it must have been under the influence of the cross-circulation. But I have little faith in the appearance of the skin of naked, immature rabbits; for I have noticed that difference of transparency, and the colour of underlying tissues, give fallacious indications.

My results thus far came to this, viz. that by injecting defibrinized blood I had produced no other effect than temporary sterility. If the sterility were due to this cause alone, my results admitted of being interpreted in a sense favourable to Pangenesis, because I had deprived the rabbits of a large part of that very component of the blood on which the restoration of tissues depends, and therefore of that part in which, according to Pangenesis, the reproductive elements might be expected to reside. I had injected alien corpuscles but not alien gemmules. The possible success of the white foot, in my first litters, was not contradicted by the absence of any thing of the sort in my second set, because the additional blood I had thrown in was completely defibrinized. It was essential to the solution of the problem, that blood in its natural state should be injected; and I thought the most convenient way of doing so was by establishing cross-circulation between the carotids. If the results were affirmative to the truth of Pangenesis, then my first experiments would not be thrown away; for (supposing them to be confirmed by larger experience) they would prove that the reproductive elements lay in the fibrine. But if cross-circulation gave a negative reply, it would be clear that the white foot was an accident of no importance to the theory of Pangenesis, and that the sterility need not be ascribed to the loss of hereditary gemmules, but to abnormal health, due to defibrinization and perhaps to other causes also.

My operations of cross-circulation (which I call x) put me in possession of three excellent silver-grey bucks, four excellent silver-grey does, and one doe whose operation was not successful enough for me to care to count it. One of my x does (B) had already undergone the operation u , and I had another of my old lot (C (u)), which I left untouched. There were also three common rabbits, bucks, which were blood-mates to silver-greys, and four common rabbits, does, also blood-mates of silver-greys. From this large stock I have bred eighty-eight rabbits in thirteen litters, and in no single case has there been any evidence of alteration of breed. There

has been one instance of a sandy Himalaya; but the owner of this breed assures me they are liable to throw them, and, as a matter of fact, as I have already stated, one of the does he sent me, did litter and throw one a few days after she reached me. The conclusion from this large series of experiments is not to be avoided, that the doctrine of Pangenesis, pure and simple, as I have interpreted it, is incorrect.

Let us consider what were the alternatives before us. It seems *à priori* that, if the reproductive elements do not depend on the body and blood together, they must reside either in the solid structure of the gland, whence they are set free by an ordinary process of growth, the blood merely affording nutriment to that growth, or else that they reside in the blood itself. My experiments show that they are not independent residents in the blood, in the way that Pangenesis asserts; but they prove nothing against the possibility of their being temporary inhabitants of it, given off by existing cells, either in a fully developed state or else in one so rudimentary that we could only ascertain their existence by inference. In this latter case, the transfused gemmules would have perished, just like the blood-corpuscles, long before the period had elapsed when the animals had recovered from the operations.

I trust that those who may verify my results will turn their attention to the latter possibility, and will try to get the male rabbits to couple immediately, and on successive days, after they have been operated on. This might be accomplished if there were does at hand ready to take them; because it often happens that when the rabbits are released from the operating-table, they are little, if at all, dashed in their spirits; they play, sniff about, are ready to fight, and, I have no doubt, to couple. Whether after their wounds had begun to inflame, they would still take to the does, I cannot say; but they sometimes remain so brisk, that it is probable that in those cases they would do so. If this experiment succeeded, it would partly confirm the very doubtful case of the pied young of the doe which died after an operation of cross-circulation (which, however, further implies that though the ovum was detached, it was still possible for the mother gemmules to influence it), and it would prove that the reproductive elements were drawn from the blood, but that they had only a transient existence in it, and were continually renewed by fresh arrivals derived from the framework of the body. It would be exceedingly instructive, supposing the experiment to give affirmative results, to notice the gradually waning powers of producing mongrel offspring.

APPENDIX I.

It is important that I should give details of the operations of cross-circulation. I may mention that, having to deal with many rabbits, I distinguished them permanently by tattooing bold Roman numerals in the inside of their ears.

I. Experiments of cross-circulation on one buck and two does, pure silver-greys, of a breed obtained from Mr. E. Royds, of Greenhill, Rochdale, the same breed as that on which all my *u* and *w* experiments had been made.

Oct. 19, 1870.—*Silver-grey buck*, O, out of doe A (*u*) by M (*u*), and therefore own brother to the white-footed young one, a small rabbit, just six months old. His blood-mate was a

Yellow buck, lop-eared, white throat, probably one-fifth heavier than the silver-grey. I avoided unnecessary weighing, because it frightens the animals, and tends to interfere with the final success. At 12^h 30^m I made cross-circulation; flow was perfect; 12^h 35^m, continued perfect; 12^h 40^m, perfect, but yellow to silver-grey perhaps the stronger; 12^h 44^m, ditto; 12^h 50^m, perfect both ways; 12^h 55^m, ditto; 1^h, ditto; 1^h 5^m, ditto; 1^h 7½^m, ditto. I then stopped and tied up. I tested the flow with a small and delicate but very simple pulse-meter on all these occasions, not liking to interfere overmuch with my fingers. I, however, used them at the commencement, at 12^h 50^m, and at 1^h 5^m.

Oct. 20, 1870.—*Silver-grey doe*, B (*u*), a fine large animal; her blood-mate was a *Common large grey lop-eared doe*, about one-tenth heavier than the silver-grey.

1^h, cross-circulation established, apparently perfect; I mean the throbbing of the canula and artery were obvious; 1^h 6^m, felt and found the flow quite good; 1^h 12^m, common to silver-grey quite good, *vice versa* poor; 1^h 15^m, ditto; I disconnected and cleaned and removed clots and reconnected. This I repeated several times; there was still much trouble in maintaining a proper flow from silver to common grey, but common to silver was always good. The operation continued till 1^h 40^m; then I disconnected; and as the silver-grey had received too much, I let her bleed to 4 drachms.

Oct. 27, 1870.—*Silver-grey doe*, H, moderate size; her blood-mate was a *Common large grey doe*, certainly more than a tenth heavier than the silver-grey. There was some trouble with her, as the carotid was abnormal, and three offshoots from it had to be tied before the canula could be inserted.

12^h 48^m, cross-circulation established, perfect pulse, but silver to common the fullest; 12^h 53^m, perfect; 1^h, silver to common perfect, *vice versa* rather poor; 1^h 2^m, ditto; 1^h 7^m, common to silver stopped; I disconnected and cleaned and reconnected, and by 1^h 12^m had reestablished perfect cross-circulation; at 1^h 30^m I had stopped silver to common and made common to silver better; got five minutes good flow, then repeated cleanings and got three minutes more. My estimate at the close of the operation was that the silver-grey gave blood freely for thirty-five minutes, and received it freely for about the same time.

II. Experiments of cross-circulation on two bucks and two does of a silver-grey breed, reputed pure, and looking well-bred animals, but liable to

show russet marks. They were procured of Mr. Vipan, of March, Cambridgeshire, and are of the same breed as those on which Mr. Bartlett made his well-known experiments about the production of Himalayas (Proc. Zool. Soc. 1861). They are liable to throw "Sandy Himalayas," as I found myself, as Mr. Bartlett also found, and as Mr. Vipan informs me is the case. I distinguish this breed by asterisks (*).

Oct. 6, 1870.—*Silver-grey buck*, P*, moderate size; his blood-mate was a *Common grey buck*, with some russet on his back and white on his belly; he was the larger of the two animals.

12^h 50^m, cross-circulation established, perfect; 12^h 55^m, ditto, but silver to common, I think, a trifle the stronger; 12^h 59^m, ditto; 1^h 5^m, common to silver very faint. I stopped them and cleaned out twice and successively; 1^h 15^m, good, but common to silver was the least good; 1^h 25^m, disconnected. My estimate was that there had been an equivalent to fully twenty-five minutes, and perhaps thirty minutes, of capital flow both ways.

Oct. 7, 1870.—*Silver-grey buck*, Q*, moderate size; his blood-mate was a *Yellow buck, white belly, large*.

11^h 40^m, cross-circulation established; 11^h 45^m, quite good; 11^h 50^m, good but not perfect; 11^h 55^m good; 12^h both stopped. Then I made several disconnexions and cleanings, and obtained short periods of success; at 12^h 35^m I finally stopped. My estimate was thirty minutes' good running: the silver-grey received more than his share; there was a slip in the operation, and five drachms of blood were lost between the rabbits; so I did not care to let the silver-grey bleed more.

Oct. 6, 1870.—*Silver-grey doe*, I*, moderate size; her blood-mate was a *Common grey doe, large*.

3^h 40^m, cross-circulation was established; 3^h 44^m, excellent; 3^h 50^m, excellent; 3^h 55^m, excellent; shortly after, something was twisted or otherwise went wrong, and both stopped. I had a good deal of trouble and but little further success. Ten drachms of blood was lost between the rabbits (partly by leakage of the canulæ).

Oct. 7, 1871.—*Silver-grey doe*, J*, moderate size; her blood-mate was a *Yellow doe, dark about mouth*, and also of moderate size. I afterwards became convinced she was simply a sandy Himalaya.

At 2^h 5^m established cross-circulation; 2^h 13^m, quite good; 2^h 20^m, excellent; 2^h 25^m, excellent; 2^h 30^m, ditto; 2^h 35^m, ditto; 2^h 40^m, ditto, then disconnected. An accident occurred at the end, by which the silver-grey lost four drachms of blood.

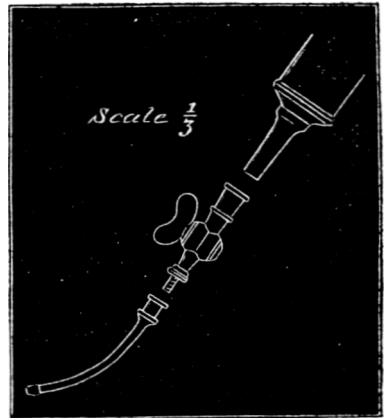
APPENDIX II.

Description of the method of performing the operations.

It is essential to a fair chance of success that the operator should have a large and thriving stock of full-grown rabbits. They cannot be procured at will in the market; and young ones are so timid and tender that

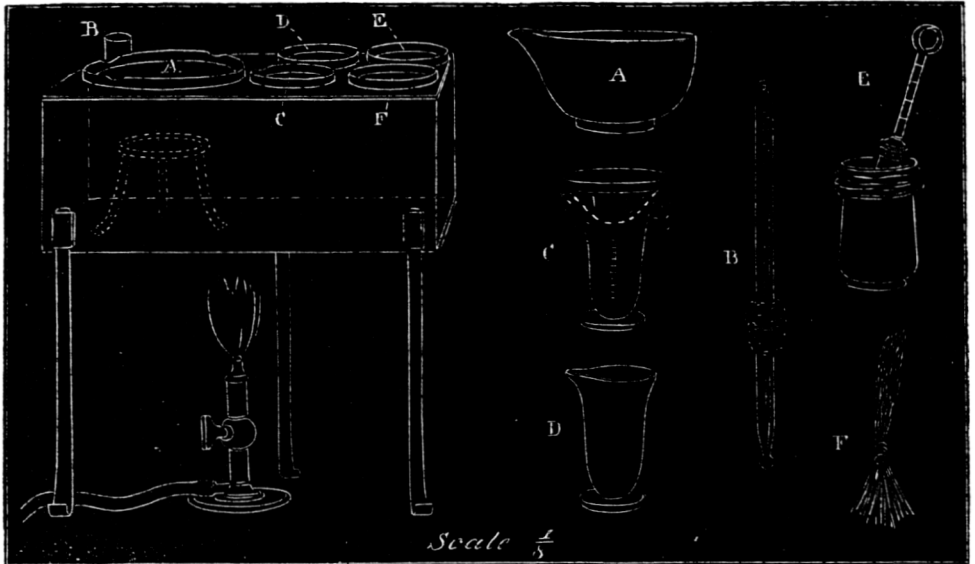
they are not fit to be operated on. The next essential point is an operating-table, with ample and proper apparatus for holding the rabbits easily but rigidly. It is most improper to subject a helpless animal to an operation without taking every precaution for its success, so as to minimize the necessity for operating. The chief hindrances to success are, entanglement of instruments, or the breaking loose of blood-vessels, both owing to an unexpected start; also an animal will struggle violently, and become terrified if he is loosely held, hoping to get away, whilst if he is firmly secured he lies as though magnetized, without signs of fear or discomfort, and with his pulse and breathing perfectly normal. I regret extremely that, although I took pains to inquire, I did not at first hear of Czermak's recently devised apparatus for holding the head. I began by the old plan of putting the animals in a bag and holding them, which was very unsatisfactory. Then I devised a plan of my own, which was good, but inferior to Czermak's, and I therefore abstain from describing it. The latter, with recent modifications, can now be obtained at Mr. Hawkesley's, 4 Blenheim Street, Bond Street, London, to whom, I should say, I have been greatly indebted for the care and thought he gave to successive and very numerous modifications of my instruments (far more numerous than I care to describe). A drawing of Czermak's apparatus will be found in the 'Berichte der K. Sächs. Gesellschaft der Wissenschaften zu Leipzig,' 1867, p. 212.

For injections, I used a five-drachm ebonite syringe, whose stem was boldly graduated to drachms. The canula (to be inserted into the vein) was screwed into a light stopcock. This was filled with water, which, so long as the cock was closed, did not run out for want of a vent-hole. When it was thrust in the vein and the vein was tied round it, I held the syringe full of blood near the open end of the stopcock, drove out all air by allowing a few drops of blood to fall into its mouth, then pushed its nozzle firmly in, opened the cock and began to inject, steadily and slowly, at the rate of about one drachm in twenty seconds. When the syringe was emptied, I turned the stopcock, withdrew it, rapidly filled it, emptied it and again filled it with warm water, and returning to the canula with the same precautions as before, I threw in about $\frac{1}{4}$ drachm, to wash the blood out of the canula and adjacent vein. I do not think I lost more than three (or perhaps four) rabbits by injecting air, although the removals and replacements of the syringe were very numerous, often ten times in a single operation of the *w* kind.



My apparatus consisted of a zinc warm-water bath, represented on the left of the diagram (p. 408); the vessels drawn to the right of it fitted into holes in its lid, as indicated by the letters. A is the basin to catch the supply blood;

it was whipped up by the whisk F; then poured into C, which consists of a short funnel with muslin below, resting in the top of a glass measure; when the blood had strained through, the funnel and muslin were set on the top of D, to get them out of the way and, at the same time, to keep them warm for future use; B is the thermometer; E is a spill-case full of water to contain the syringe. In addition to these, I required a large slop-pail, a jug of hot, and another of cold water.

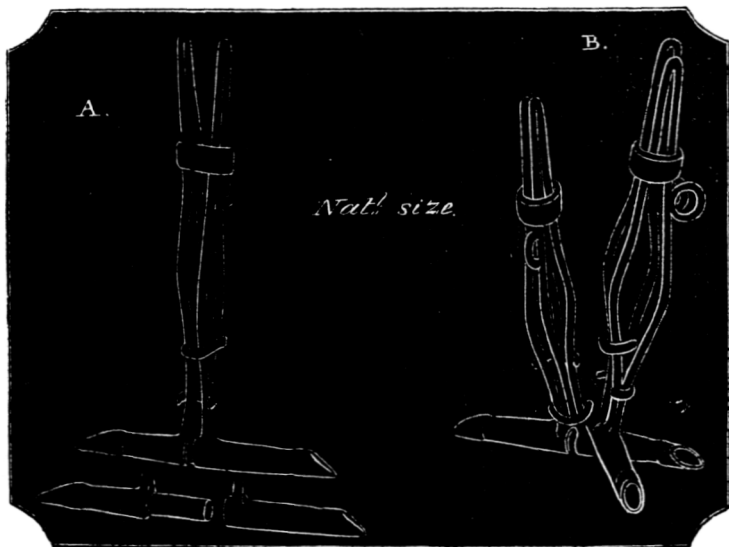


The sketch shows my latest outfit of basins and warm water for injecting. It was not perfected until I had nearly finished the experiments. Scrupulous cleanliness is requisite, and great orderliness; for the hazard lies, not in the performance of one difficult operation, but in making a mistake in some one of a great many easy operations. The course of an operation was as follows:—(1) secure the animal, (2) remove fur from neck, (3) anæsthetics, (4) expose jugular, (5) cut a slit in it and let the animal bleed as much as he can easily bear, about six drachms, (6) stop the flow with gentle pressure by spring forceps; the animal was then left for a minute while (7) Dr. Murie and Mr. Fraser divided the throat of the supply-rabbit, I catching the blood in a warmed basin and whipping it up, to defibrinize it, as it fell. I continued doing this while Dr. Murie was (8) inserting the canula; and when he was nearly ready he called to me, and I (9) filtered the blood, noting its amount, as a guide to what I had to dispose of, (10) drew up a syringe full, (11) injected a convenient number of drachms or half drachms, indicated by the graduations on the syringe-handle, (12) returned the overplus to the glass of supply-blood, (13) cleansed syringe and injected water, (14) let the rabbit bleed three or four drachms,—and then recommenced the series. I have not re-inserted in this description before (11) and (13) what I previously described about turning the stopcock &c.; nor have I spoken of the continual jotting down of notes in my case-book.

At the end of all, the vein was *tied*. It was, no doubt, the surest plan to avoid future hemorrhage, especially as the blood was defibrinized; but the rabbits were apt to suffer from phlebitis, and I lost some thereby.

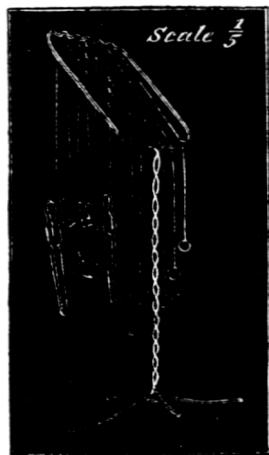
Owing to the extreme rapidity and stiffness of the coagulation of rabbit's blood, it is quite easy to estimate the quantity that may have been spilt on the operating-table. It has simply to be sponged into a measuring-glass.

Cross-circulation would be a very easy operation in animals whose carotids were even a trifle larger than those of silver-grey rabbits; but it is difficult with these, because the smallest canula which can be used with propriety can only just be forced into the largest of them. It is no use operating with small canulæ; in every case, a layer of fibrine is sure to line the tube; if the bore is small this layer chokes it, while a layer of equal thickness in a larger tube leaves a free central passage. I found canulæ $\frac{1}{20}$ inch in diameter of bore were worthless; those I used were $\frac{1}{13}$ inch. If I were to operate again, I should not use silver-grey rabbits, on account of their smallness, but "Belgian hare" rabbits. When the canulæ are brought home together, the wire hooks, shown in the sketch, secure them; but I also slipped an India-rubber band over the tips of their handles. The cut ends of the artery were held open and stretched out by a pair of delicate curved forceps (a suggestion due to Dr. Murie), and the



canula was pressed in (the shape of its mouth was the result of many trials and {modifications}), and a ligature was put on. In the diagram, A represents one pair of canulæ, both opened and closed. B shows their position at the time of crossed circulation. It will be observed that each artery requires four pieces of apparatus, viz. two spring forceps to stop the blood, and two canulæ. Thus, when the throats were brought close together, to connect the arteries cross-wise, there were no less than eight

separate pieces at work in a deep hollow, close together, and attached to delicate arteries, none of which could be permitted to twist or interfere with each other. I append a reduced sketch of one of the two frameworks over which, as previously described, I suspended these instruments, with attached counterpoises, and so avoided all confusion. Both pair of canulæ and two pair of forceps are here represented; they might be so arranged; but it is better to divide the instruments, equally, between the two frames.



For removing clogs from the canulæ, I tried a great many plans, none with as much success as I could wish. I have, however, been able to extract clots from the artery itself, a good quarter of an inch beyond the canulæ, with a wire whose end had been cut with a file into a delicate solid corkscrew.

I washed out the canulæ, before reconnecting, with a thin stream of water sent through the quill of a small bird, which I had fastened, by help of a short India-rubber tube, to my syringe.

The wounds require careful dressing, just like those of a man. The rabbits bear the operations wonderfully well, and appear to suffer little or no pain when the influence of the anæsthetics happens to have left them temporarily sensible. They are often quite frisky when released, and sometimes look as though nothing whatever unusual had happened to them, all through the time of their recovery.

II. "Contributions to the History of Orcin.—No. I. Nitro-substitution Compounds of the Orcins"*. By JOHN STENHOUSE, LL.D., F.R.S., &c. Received March 1, 1871.

The action of nitric acid upon orcin has been studied by several chemists, but with comparatively negative results. Schunck † in this manner obtained a red resinous substance, which by further treatment with the acid was oxidized to oxalic acid; and in 1864 De Luynes ‡ found that orcin dissolved in cooled fuming nitric acid without evolution of nitrous fumes, and that the addition of water precipitated a red colouring-matter; the long-continued action of the vapour of fuming nitric acid on powdered orcin likewise produced a red dye apparently identical with the above. These, however, were resinous uncrystallizable substances.

Although under ordinary circumstances only resinous products are obtained by treating orcin with nitric acid, yet, when colourless orcin in fine

* A Preliminary Notice with this title was published in the 'Chemical News,' August 26, 1870.

† Ann. Chem. Pharm. vol. liv. p. 270.

‡ Ibid. vol. cxxx. p. 34.