A reply to a note of W. E. Castle entitled „Russo on sex-determination and artificial modification of the Mendelian ratios".

By A. Russo. University of Catania.

Professor Jordan of Virginia University in the April number of this years American Naturalist made some very favourable remarks regarding my researches on the problem of the determination of sex.

Mr. W. E. Castle in the July number of the same paper criticises in a most unfair manner, both the favourable remarks of Professor Jordan, and the results of my researches.

I take no notice of considerations which are outside the argument and altogether personal, I think it more serious and useful towards the progress of a knowledge of such an important problem, to present the facts, which Castle preoccupied with the theory of the gametic difference, has not taken into consideration.

It must be noted first of all that Castle, in criticising and then denying the results of my researches, cites the testimony of others, when it is elementary that a naturalist before denying natural facts, which is an easy matter seated at a desk, should by personal observations judge the state of things.

He in fact says, that two authors have obtained results contrary to mine, but when I demonstrate that the two authors have not carried out their experiments as they should have, and that therefore their results are erroneous from the point of view to which my researches are directed, I have also demonstrated that the criticism of Mr. Castle is guided by scientific bad faith [1]).

To Punnett, one of the two authors, I have already replied [2]) pointing out that he has only given per os the Lecithine, whereas I have always used injections. To Castle I now reply that chemi-

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1) The origin and scope of Castle's note is to try and prove at any cost the exactness of the theories of the school to which he belongs, and that is, sex is of a hereditary character, which follows the same Mendelian laws. He says that this theory is to-day accepted by all and that I have revived an old theory, that is of the nutrition and therefore of the action of external agencies on the determination of sex. In all this the bad faith of the author is most clear, as he uses any means to cause his own theory to be believed. It is possible that his theory is to-day accepted by many (certainly not by all, as most recent researches agree with my theory) but this does not demonstrate that it is true, and it is surprising that a man of scientific culture founds his assertions on the favour a theory may have at a certain moment.

Regarding the other insinuation I reply, that theories founded on natural facts are never old, and therefore Castle ought to bear in mind, before fancying a problematic ipotesis, the whole of the kowledge of the problem of sex.

2) Castle speaks of several authors who contradict my researches, whereas it is only Punnett and the student Basile who do so.

cal physiology teaches us that the Lecithine in the digestive tube becomes decomposed and is expelled from the tissues, retaining only a small part of phosphoric-glicerine acid. Experiments made by me in the same way as Punnet gave me up to 1907 the same results from which it is seen that the proportion of the sex does not change.

I give here a statistic in which the whole truth is schown, as Castle wishes, a statistic which up to now I have not thought as necessary to publish.

	1st confinement		2nd confinement	
	♂	♀	♂	♀
1	2	4	3	3
2	2	1	3	5
3	3	4	3	2
4	5	5	4	3
5	3	3	—	—
6	4	2	—	—
7	4	4	—	—
	23	23	13	13

Regarding the researches of the other authors I have demonstrated that they are wrong in having united the rabbits immediately after their confinement, and for this reason there were many embryo deaths, and for the technic of the injection wrongly used, in fact following these injections, as the same author says, several died, others had many and repeated abcesses which were cured by surgery. If all these technical errors, the results of this author, whom Castle calls to his aid, should be taken into consideration, let the impartial reader judge!!!

The accusation however made to me by Castle, and which has also been made to me before by others, is that in compiling my statistics I have chosen confinements in which the sex of the newborn was favourable to my argument.

A propos I must say that, perhaps I had too much faith in the good sense of some readers, who ought to have understood, without any other explanation, that the confinements were not chosen to deceive the public which would have been too simple!...

From my publications it is however clearly schown that, in rabbits which are injected, in the first confinement, an increase of ♀♀ is not obtained, because at the moment when the injections had been made the eggs already had acquired a metabolism all to themselves, in this case the effect wished was obtained in the following confinements. These confinements were chosen by me with the sole scope of showing in clearer light the phenomenon

of the increase of births in the female sex following injections of Lecithine, trusting to the commonsense of the good reader.

If Castle does not care to accept this justification as a reason of the choice of sex, I may add that in my researches I have taken into account also the births in the first confinement and that the result confirms the theory of metabolism of the eggs in the production of sex. As I have demonstrated in a recent note[3]) in the first birth of rabbits injected with Lecithine it often happens that not only the ♀♀ do not increase but the ♂♂ are in a great majority, with a general increase in the number of births. The injections of Lecithine have in this case the effect of increasing the general metabolism, of enriching the ovary of plastic material and of impeding the degeneration of the eggs of catabolic type, capable of producing males.

This is a union of facts which Castle, before judging so lightly ought to study well.

Putting now aside this argument, I would like to say that Castle, to discredit the results of my researches, avails himself of a note of Heape who thought that the eggs classified by me as male eggs were degenerated.

Too truly the opinion of Heape is completely erroneous, for the reason that posterior researches have confirmed to me, that in a rabbits' ovary two species of eggs exist, distinguished separately by a special type o Metabolism, that is, eggs of an anabolic or constructive type, containing globules of Lecithine, and eggs of disassimilative or catabolic type, containing crystals of acid fat. I defy therefore Castle to prove unfounded the results of my researches, which I now precise as follows.

1st The injections of Merks' Lecithine (solution of 15—20 $^0/_0$ in vaseline oil), opportunately made, improve the general metabolism and enrich the eggs with deutoplasmic material.

2nd In the ovary of normal rabbits two species of eggs exist, one of an anabolic, type rich with Lecithine globules, the other of a catabolic type, containing instead crystals of acid fat.

3rd The eggs of a catabolic type are more easily subject to degeneration, the last stage of which is fatty degeneration.

4th The catabolic eggs at the beginning of the catabolic process give males; when the degeneration begins the eggs can still be fertilized giving dead embryo of male sex.

5th The eggs rich with deutoplasmic globules of the anabolic type give females, this is demonstrated from the fact that, after

3) Russo. Sui prodotti del diverso tipo di metabolisms osservato nelle ova della coniglia e sul loro valore per il problema della sessualita. Arch. di Fisiologia-Firenze 1910.

the injection of lecithine many eggs are rich with nutricious material, and at the same time increase the births of female sex.

6th In the first confinement after the treatment with Lecithine often, instead of an increase of the ♀♀ the ♂♂ increase, in this case the increase of the ♀♀ will take place in the following confinement.

7th In general after the Lecithine treatment there is an increase in the number of births.

Regarding the criticism which Castle makes against me as to the cases of deviation from Mendel's law of prevalence, I must above all declare that my researches on this argument were made with the scope of giving reasonable interpretation to this same law. I wanted that is, to prove, if, by the production of the young with a prevalent character, only the chromosomes would be imputable or if it were not a question of general metabolism; that such metabolism belongs to protoplasm or to elements which figure in the nucleus, it does not matter for the demonstration of the principle which moves my researches.

Some cases in fact, contrary to the affirmation of Castle, cannot be explained by the chromosomic theory or from omo et eterozigoti, as is demonstrated in a case referred to a himalayan female rabbit which was crossed for three generations with the same black male and which should always have given young of the same character, on the contrary, while in the first generation it gave young of a black character, in the second and third generation, the female having been injected with lecithine, gave birth to young of maternal character, and others of a mixed character (paternal and maternal). Castle preoccupied always with the idea of condemning my researches, gets out of this, by supposing that the black male is eterozigo with regressive character, white, black and mixed, which would explain the birth of the young with such a character. But this should have occurred from the first generation, as is known by the elements which govern Mendel's laws and which Castle ought to know.

In conclusion if Castle has no other argument to render more consistent and less pettyfoggy his criticism, I must believe that my researches are of some signification which Castle cannot or will not understand, and that they open a new way in biological researches as has already been declared by valient contemporary biologists.

Verlag von Georg Thieme in Leipzig, Rabensteinplatz 2. — Druck der k. bayer. Hof- und Univ.-Buchdr. von Junge & Sohn in Erlangen.

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Zoologisch-Botanische Datenbank/Zoological-Botanical Database

Digitale Literatur/Digital Literature

Zeitschrift/Journal: Biologisches Zentralblatt

Jahr/Year: 1911

Band/Volume: 31

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Artikel/Article: A reply to a note of W. E. Castle entitled „Russo on sex-determination and artificial modification of the Mendelian ratios". 29-32