beetle reared from a parthenogenetic ovum. With your permission I shall now endeavour as briefly as possible to give those circumstantial details without which a bald statement of results would not carry with it a rational conviction of the accuracy of my observations.

From beetles gathered in the beginning of last April I had a batch of eggs on the 7th, which hatched out on the 21st of the same month, and on May 13-15 yielded about thirty pupæ, which were immediately put into separate vessels. On the 20th-22nd appeared the imagines, of which ten subsequently turned out to be females, and were placed together in pots, but not before the greatly enlarged abdomen had given unmistakable evidence of their sex. The first eggs, three batches, were laid on June 2 (so completing the cycle, from egg to egg, in fifty-six days). On the 12th of the month I found in one of these batches, consisting of forty-two eggs, thirteen developed, of which two hatched out, the larvæ dying shortly afterwards. Others seemed to have partly hatched, but most eventually perished in the shell. At this time fertilised eggs were hatching in nine days. It appeared to me that several of the thirteen were imperfectly or monstrously developed; one, for example, having only one misshapen (?) mandible; another, excess in number and irregularity in grouping of the eye-spots; &c., &c. Again, on June 17 I found in a parcel of (twenty-five) eggs, laid June 6-7, six which had developed up to the hatching. In the usual course, at the time of hatching, the young larva comes out of the shell clear like barley-sugar, but blackens afterwards; in the case of these parthenogenetic larvæ which do not hatch out, this blackening takes place within the shell. In a third batch, of over twenty eggs, laid June 8, I found three eggs similarly developed. In the meantime, and afterwards, many dozen batches had been laid, in which, however, I did not detect any development.

A second experiment miscarried; but I was more successful with a third and fourth. From a batch of eggs laid June 5-6 I derived pupæ which on July 8 following I placed separately in pots, and obtained from them thirteen beetles, of which seven turned out to be females. About the same time I brought in from the fields some well-grown larvæ, the beetles from which were isolated immediately after their exclusion, and subsequently yielded eight females. These (seven and eight) were all kept in separate pots during the course of the experiment. Of the seven no less than five laid eggs which afterwards developed parthenogenetically. They laid as many as ten parthenogenetic batches among them, but while some of them laid three such batches, others laid only one. These were invariably the first batches laid, and none of the batches laid subsequently contained any viable eggs while the experiment lasted, which was in some cases up to the tenth batch. Of the eight beetles of the fourth group, only one laid one parthenogenetic egg in its first batch. The number of parthenogenetic eggs in a batch varied from one to seven. In four batches there was only one such egg; in three batches five, and in the other four batches two, three, six, and seven respectively. The total eggs in a batch averaged 4.17, and as there were thirty-six parthenogenetic, the proportion over all was 1 in 12½. However, as may be supposed, the proportion in the individual batches varied very much, one small parcel of only eleven eggs having as many as five developed.

In most of these cases also the larvæ perished in the shell. A few hatched out more or less completely, and died. Two, however (of the seven in one batch), were more fortunate. These came out on July 29, and for some hours seemed very feeble and barely alive. Next morning I found that one, which subsequently took the lead of its fellow in all respects, had crawled away under cover, and the other was soon able to follow its example. I could not find that they had eaten anything till the evening of the 31st. After that, however, they thrived apace. The larger one passed its first and second moults on the 3rd and 7th of August, the smaller following it on the 4th and 8th. The former pupated on the 14th, and the imago was excluded on the 19th. The latter, having pupated, August 15-16, appeared to go on well till the time for the exclusion of the beetle, when its further development became arrested, and it died. The survivor was weakly at first, and rather imperfect always as regards the elytra, which are somewhat small, and do not close in the middle line. It has, however, thriven well, and developed that enlargement of the abdomen peculiar to the female. But up to the present (September 22) it has laid no eggs, nor shown any inclination towards males placed in the pot with it.

When it became obvious that no more parthenogenetic eggs were to be obtained from these beetles, I placed the survivors of them in succession in a pot with a (the same) male beetle, with

the result that most afterwards laid fruitful eggs in the ordinary way. I mention this because it seems to be in contradiction, as far as these insects are concerned, with the statement of von Siebold ("Beiträge," p. 89): "Es ist nun eine bekannte Sache, dass, wenn Insecten-Weibchen vor der Begattung erst einmal Eier zu legen angefangen haben, ihre Männchen alsdann mit ihren verspäteten Liebesbezeugungen bei ihnen nichts mehr auszurichten im Stande sind."

If now I may be permitted to make a few general observations on some of the points indicated for further inquiry, rather than established, by the foregoing experiments, I would say: (1) that parthenogenesis seems to occur chiefly in the first-laid batches; (2) that it is peculiar to some females, while others appear to be exempt from it; (3) that confinement and domestication, as it were, acting hereditarily, which we already know so profoundly to affect the generative system in the higher animals, appear to favour this mode of reproduction in *Gastrophysa raphani*; (4) that there are degrees of viability in parthenogenetic embryos, so that the development seems to be arrested chiefly at certain points, as at the hatching of the egg and the exclusion of the imago. In this respect the *Gastrophysa* egg behaves very much as the ovum of *Bombix mori* is reported to do (v. "Beiträge," pp. 230-232). (5) Another point in which *G. raphani* agrees with other parthenogenetically reproductive arthropods is its many-broodedness in a season. There may be three or four generations in direct succession in the year, and there is a constant succession of eggs all the time. In this it appears to differ from any of its allies with which I am acquainted. (6) Finally, the case of *G. raphani* would seem to be one of true parthenogenesis in its most restricted sense—the same beetle which in the unimpregnated state lays sterile eggs, with here and there one capable of development, after receiving the male element, laying eggs which are fertile and develop in the ordinary way. That is to say, the ova are true ova, and not pseud-ova or buds, the parent a perfect female and not an "Amme" like the summer Aphis.

J. A. OSBORNE
Milford, Letterkenny, Ireland, September 22

Observations of Auroræ on August 12 and 13

THE finest display was between 10 and 11 o'clock on the evening of the 12th, when a magnificent corona was formed almost exactly on γ Cygni. At this time the bases of the columns on the eastern horizon were distinctly red. Unfortunately the spectroscope could not be brought to bear until the aurora had faded to a small fraction of its greatest brightness. With a very small dispersion (Vogel spectroscopie) the spectrum was continuous from W.L. 557 mm. to W.L. 473, with strong traces of a finely banded spectrum, terminating abruptly at the great line 557. Towards the violet an isolated line was seen and measured.

On the 13th the great line was seen and also traces of the others. The measures may be summarised as follows:—

Wave-length.	Spectroscope.	No. of measures.
557.16 $\pm$ 0.20	Grubb	8
527.5	"	1
469.6	"	1
473.2	Vogel	1
430	Grubb	In middle of field
434	Vogel	1

The Grubb spectroscopie has a dispersion of $5^{\circ} 40'$ from C to λ . The first line was measured with an illuminated micrometer wire, the others with the edge of an opaque screen. The Vogel spectroscopie has a scale of bright lines with sixty divisions to the whole visible spectrum.

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Ice at High Temperatures

FROM Mr. Hannay's letter (NATURE, vol. xxii. p. 483) and from private communications I have received it appears there has been a little misconception as to the manner in which I judged of the temperature of the ice in the experiment referred to in NATURE, *ibid.*, 435. Mr. Hannay's theory, that the ice was protected from the hot glass by an intervening layer of vapour, at first occurred to myself and to others as the true explanation of the phenomenon, but that this explanation will not serve in the present case is, I think, proved by the fact that a thermometer was imbedded in the ice and rose to temperatures varying in different experiments between 120° and 180° C., at