same question, we have also to remember Tanaka's observation that a certain linkage found in the male silkworm is absent in the female.

Another far-reaching discovery has been made by F. Lillie. When in horned cattle twins of opposite sexes occur, the female is sometimes sterile, being called a free-martin. We were inclined to interpret these twins as arising by division of one fertilised ovum, but Lillie, in a study of material from the Chicago stockyards, found that an ovum had dehisced from each ovary, and the twins were therefore originally distinct. Moreover, he showed that in some instances the twins have an actual anastomosis in the fœtal circulation. We are thus driven to believe that the presence of a male embryo may influence—in cattle—the development of a female embryo, poisoning it, in so far that the development of the generative organs is partially inhibited.

Many complex cases of interaction between factors have been successfully analysed. Punnett's elaborate experiments on the colours of rabbits and sweet peas, Emerson's studies in Phaseolus, and several more such investigations are gradually laying a solid foundation from which the mechanism of factorial determination may be deduced. The discovery made by Nilsson-Ehle, and independently by East, that in some forms there are several factors with identical powers, is another notable advance.

Controversy is proceeding respecting the divisibility of factors. When on segregation, either in the gametes of F_1 or in later generations, instead of two or three sharply differentiated classes of zygotes, much intergradation occurs, or when one of the parental types fails to reappear, the result may be interpreted either as showing imperfect segregation, or as an indication that the number of factors involved is very large. The balance of evidence perhaps suggests that many factors can, and on occasion do, break up (as the sex-factor almost certainly does), some commonly, others exceptionally, while others, again, seem to maintain their individuality indefinitely unimpaired.

As bearing on evolutionary theory, the new work leaves us much where we were. Progress in genetic physiology has been rather a restraining influence. The notion that Mendelian segregation applies to varieties and not to species has been often refuted. One of the most useful contributions to this subject is Heribert-Nilsson's evidence respecting Salix hybrids. Wichura believed himself to have proved that they and their derivatives are simple intermediates between the parental forms, and this statement, which has passed current for fifty years, is now shown to be a mistake due to insufficient material. Interest also attaches to Castle's recent withdrawal of his conclusion that by continued selection certain Mendelian characters in rats could be modified, an opinion which, though consistent with his own experimental work, has not stood a crucial test. We are still without any uncontrovertible example of co-derivatives from a single ancestral origin producing sterile offspring when intercrossed. This, one of the most serious obstacles to all evolutionary theories, remains. The late R. P. Gregory's evidence that tetraploid Primulas, derived from ordinary diploid plants, cannot breed with them, though fertile with each other, is the nearest approach to that phenomenon, but the case, though exceptionally interesting, does not, of course, touch this outstanding difficulty in any way.

Space does not suffice to enumerate the practical applications of genetic science to economic breeding, of which some have already matured and many are well advanced.

TELEGONY.

BY PROF. J. COSSAR EWART, F.R.S.

THE belief in telegony is probably as old as the belief in maternal impressions, so intimately associated with Jacob's breeding experiments, recorded in the thirtieth chapter of the Book of Genesis. In prehistoric times, when breeds of sheep and cattle brought from the East by the Alpine race were crossed with the more recently formed European breeds striking new varieties would now and again appear. The ancient shepherds would doubtless endeavour to account for the differences between the cross-bred offspring and their pure-pred ancestors, and later biologists would be called upon to decide which of the views of the ancient breeders were most worthy of support.

The doctrine of the infection of the germ now known as telegony was more or less firmly believed in by men of science as well as by breeders up to the end of the nineteenth century. Beecher, writing at the close of the seventeenth century, says: "When a mare has had a mule by an ass and afterwards a foal by a horse there are evidently marks on the foal of the mother having retained some ideas of her former paramour, the ass." Agassiz held that the ovary was so modified by the first act of fecundation that "later impregnations do not efface that first impression." Similar views were entertained by Haller, Darwin, Herbert Spencer, Carpenter, Sir Everard Home, and others, and up to 1895, when I started my experiments, physiologists as a rule either admitted the possibility of the blood of a mare imbibing from that of the fœtus some of the attributes which it had derived from its male parent and thereafter handing them on to offspring by a different sire, or believed that some of the unused germ plasm

of the first mate penetrated the immature ova and eventually took part in controlling the development of offspring by subsequent mates.

Up to the end of last century Lord Morton's experiments with a male quagga and a young chestnut seven-eighths Arabian mare were regarded as affording strong evidence of telegony. Hence at the outset I decided to repeat as accurately as possible Lord Morton's experiment. The quagga being extinct, a Burchell zebra was mated with Arab and other mares belonging to different breeds and strains. The mares, after producing one or more hybrids, were mated with Arab and other stallions.

In an account of my experiments, illustrated by numerous figures, published in the Transactions of the Highland and Agricultural Society of Scotland for 1902, it is pointed out that, though, to start with, I believed there was such a thing as telegony, I eventually came to the conclusion that "there never has been an undoubted instance of infection in either dogs, rabbits, or horses." Though a full account of my investigations, by Mr. Hermon C. Bumpus, appeared in the *American Naturalist* (December, 1899), and an abstract was published in the 1910 Report of the United States Bureau of Animal Industry, it is related in a recent American work on evolution[1] that the idea of telegony "rests mainly upon what are known as the Penycuik experiments (Ewart, 1899), the initial one of which was the impregnation of a mare, 'Mulatto,' by a quagga, a species of zebra which is now extinct. The offspring of this union was the foal 'Romulus,' which showed the quagga-stripes of his father very distinctly. Later, 'Mulatto' was bred to a pure Arab stallion and her second foal also showed traces of stripes, although by no means as distinctly as his half-brother 'Romulus.' . . . Definite instances are neither numerous nor well authenticated with the exception of the one in question, and even this may be due to some other cause."

It is scarcely necessary to say that I am not responsible for the idea of telegony—without going far afield, Lull might have discovered that the doctrine of "infection" had been dealt with by Agassiz and was especially associated with a mare belonging to Lord Morton—but it may be as well to point out that I used a Burchell zebra (the quagga had been extinct for nearly a quarter of a century); that the hybrid "Romulus," instead of being striped like his sire, approached in his markings the very richly striped zebra of Somaliland; and that the two subsequent foals of "Mulatto" were decidedly less suggestive of zebras than pure-bred foals of a near relative of "Mulatto" who had never even seen a zebra.

In 1910, when giving a course of lectures in Iowa, I gathered that the doctrine of telegony had few adherents in America. This view is supported by a statement in the recent work by Jordan and Kellogg, who "think it probable that the phenomena called telegony have no real existence."

PROGRESS OF CHEMISTRY.

By Sir Edward Thorpe, C.B., F.R.S.

THE half-century which has elapsed since the first issue of NATURE has witnessed an extraordinary development of science in general, but in no department has it been more marked, or the changes more profound, than in chemistry. Before dealing with the period over which the existence of this journal extends, it may not be uninteresting to indicate, in the broadest possible outline, the main features of progress in chemical science to which the growth we have witnessed during the last fifty years is in reality due.

The opening years of the nineteenth century constituted a new era in the history of chemical science. The revolution initiated by Lavoisier and his associates—Morveau, Laplace, Monge, Berthollet, and Fourcroy—was by this time accomplished, and its influence had extended throughout Europe. The French chemists, who emancipated chemistry from the thraldom of a false German doctrine, swept phlogistonism into the *limbus fatuorum* of extinct heresies. The early years of that century saw the passing of the more prominent adherents of Stahl's philosophy; of the English chemists, Priestley died in 1804, and Cavendish, who for some years previously had ceased to pursue chemical inquiry, followed him six years later.

Within the first quarter of the century appeared some of the most eminent of those who were destined to consolidate the principles upon which the New Chemistry was founded. Dumas and Wöhler were born in 1800, Liebig in 1803, Graham in 1805, Laurent in 1807, Gerhardt in 1816, Wurtz, Kopp, and Marignac in 1817, Kolbe and Hofmann in 1818, Pasteur in 1822, Alexander Williamson in 1824, and Edward Frankland in 1825. But there was already a generation at work the members of which, although not specially distinguished for their direct contributions to speculative chemistry, yet served by their labours to strengthen the foundations upon which it is based; among them were Wollaston and Davy, born in 1766, and Gay-Lussac, born in 1778. Berzelius, who was born in 1779, first published his electro-chemical theory in 1827. A revolution scarcely less momentous than that of Lavoisier had, moreover, by this time been effected by John Dalton; the enunciation of the atomic theory in 1807–8 wholly altered the aspect of chemistry; henceforth it was brought within the domain of mathematics, and its laws and processes were established on a

[1] Lull, "Organic Evolution." (New York: The Macmillan Co.)