bra; it appears, also, that even individuals of the same species may vary in this manner, (*Phoca grœnlandica*); and this recalls a suggestion already made by me (45, 15), which ought to be considered, although, at present, its importance may seem rather ideal than real; "it does not seem possible that the head and pelvis can be as strictly homologous in animals having a different number of vertebræ as in those with the same number; in other words, the heads or the pelves of two animals may be cephalic or pelvic modifications of *vertebræ*, without being such modifications of the same identical vertebræ." Even if we exclude the skull from the category of vertebræ, the difficulty is not removed; for if the atlas of Hyrax is homologous with that of Elephas, then the sacrum of Elephas is the homologue of the twenty-fourth vertebra and its successors, with Hyrax; or if we also assume that the sacra of the two are homologous, we must homologize 29 vertebræ in the one with 22 in the other; and, *practically* at least, this seems to be our only course.

I trust that the foregoing considerations will aid in removing the stumbling block of numbers, from the path of those who would otherwise accept the meketropy of pollex and primus. To my own mind they were hardly needed, so decided was the conviction formed in 1866, and expressed in 51, 52 and 57, that no difference in the numbers of phalanges ought to affect our recognition of a profound morphological law affecting the membra.

Note. Dr. Coues has kindly placed at my disposal the MS. of some unpublished investigations bearing upon this subject, which so nearly accord with my own views, that I add them here. April, 1872.

Susceptibility of variation in numerical composition he believes to be, *a*, in direct ratio of number of parts composing an organ, and *b*, in inverse ratio of morphical differentiation and telical specialization of the parts of an organ; and that, consequently, the *value* of numerical composition as a morphological or taxonomic datum can be estimated with reasonable confidence of at least approximate accuracy. Value is inversely as variability.

"It is notorious," he continues, "that an organ (whether central or peripheral — whether indispensable to the integrity of an animal, or merely a useful adjunct to its economy) composed of a few parts, does not exhibit the same percentage of variation in the number of these parts, as the same or a similar organ does when it is composed of many parts. For instance, the *normal* variation in the bones of the coccyx of Primates is at a minimum, if it be not, indeed, *nil*; whilst the ordinary individual variation in the coccyx of a longicaudate mammal, such as the *Jaculus hudsonius*, for example, amounts to four or five coccygeal vertebræ. The few dermal scutes of armadillos are sufficiently constant in number to afford specific characters, while the essentially similar but numerous

dermal scales upon the belly of a serpent may vary widely in number in different individuals of the same species. The rays of a small, sharply-outlined dorsal fin of a fish have no such variation in number as those composing a fin that extends the greater part of the length of the animal. The very numerous teeth of a serpent cannot be rendered with the certitude that attaches to the dental formula of a few-toothed mammal. In the lower families of birds possessing more than twelve rectrices, the number is fallacious even as a specific character, since it varies one or two pairs, at least, in different individuals of the same species, whereas in birds with eight, ten, or twelve rectrices these numbers mark whole familes, and the slightest variation is properly regarded as an anomaly. The few digital phalanges of birds are so constant (much more constant than their vertebræ) that deviation from the ordinary number becomes a character marking families.

"But it is unnecessary to dwell upon this obvious point, the more so since it is simply one part of the main proposition, that variation is greatest in organs composed of the most similar parts—parts that are essentially either morphically or telically *repetitive*, and conversely, that the variation in numerical composition is least in the structures made up of more perfectly differentiated or specialized parts. Any structure the essence of which admits of what is called 'vegetative repetition,' is susceptible of enlargement or curtailment by the development of more or fewer segments or moieties, and variability is a necessary result of such plasticity of organization. The examples adduced may be here cited again in illustration. Most of the caudal vertebræ of a long-tailed mammal are precisely similar in form and function—positive duplicates of each other, and in such a mammal as the house-rat, the coccygeal formula can only be given approximately, while the still more numerous dermal annuli of the tail, though corresponding in a general way with the bones themselves, must be enumerated simply in round numbers. The vertebræ of a serpent, essentially similar throughout the long series, represent no such fixed number as those of a mammal where they are differentiated in several groups, each with its own character. And even surveying organs composed of few parts, we find striking differences in variability. The presence, in an animal possessing five digits, of a supernumerary one, is in frequency out of any calculable proportion to the appearance of two functional digits in an animal that, like the horse, has normally but one—perhaps the improbability of the latter is on a par with that of the appearance of ten digits in a man. I am not informed as to the individual variability in the number of phalanges of cetaceans, and probably too few of these animals have been dissected for correct estimation, but there is every reason to suppose that the liability to variation here is as much greater than it is an ordinary mammal, as the increase in the number of phalanges.

"The abrupt and marked increase in the number of phalanges of cetaceans as compared with ordinary mammals, and the imperfect discrimination of phalanges, metacarpals and carpals in these mammals, seem to be explicable upon the same principles that account for the great number, small size and mutual resemblance of the vertebræ of prehensile tailed mammals, and those that use a long flexible tail as a balance. There is the same teleology in either case—it is the production of perfect pliability; and in both, the increase seems to be sim-

ply a matter of repetition. It is probably as impossible to homologize individual bones of a cetacean manus with those of an ordinary mammal, as it is to homologize the immense number of caudal vertebræ of the genus Mus, for instance, with the few of a neighboring genus, Arvicola. In all such cases as these, where variability is at a maximum, the importance of numerical composition, either as a taxonomic or as a morphical character, is obviously at a minimum. If the Cetacea agreed with ordinary mammals in other respects, the composition of the manus would afford no better grounds for these wide separation than the number of caudal vertebræ in certain other families.

"If we take the other extreme, of a solidungulate animal, we find such strong differentiation of the osseous elements of the manus, that every single one of the few bones has its own shape and size, and each of the distal segments, at least, performs a perceptibly distinct function; even a sesamoid is elevated, functionally, almost to the rank of a phalanx. Here the variability is virtually *nil*; if it occur at all, it would be entirely abnormal; and the slightest normal difference in numerical composition, either in number of digits or of their phalanges, has a generic, if not a higher, value.

"The value of numerical composition of the pollex and primus as a morphological character, has been estimated by different anatomists at its two possible extremes—some considering it an insuperable objection to the antitropic homology of pollex with quintus, and others finding it little or no obstacle to such a view. Two considerations have had great weight with me, in reducing my estimate of its value so low, that it presents itself as no valid objection, when taken in connection with the strong evidence derived from other sources. In the first place, the question can only arise in respect to five-fingered mammals, a part, at least, of the digits of which have three phalanges each; and since here we have the maximum known number of digits, and the next to the maximum known number of phalanges (Cetacea alone having more) the susceptibility of variation in numerical composition is nearly at a maximum, according to the principles already laid down, and hence the value of numerical composition is nearly at a minimum so far as the manus is concerned.

"Secondly, it is certain that pollex and primus are telically correspondent (analogous), and no less so that the modification each has undergone in its composition is simply telical. Both have been strongly differentiated from the other digits in the same way, and for the same purpose. It is presumed that no anatomist questions the homology of the whole manus of a bird, a reptile and a mammal; yet the homology cannot be pushed to the individual osseous elements without recognition of vastly more difference in numerical composition than we are called upon to admit in the present case of pollex and quintus, and hence without tacit depreciation of the morphical import of mere number. The manus and the pes of a bird cannot be homologized with each other, according to any one of the current modes of comparison, without greater allowance still for telical modification in the matter of numerical composition. For myself, if I attempt to recognize any homology between the manus of a man, for example, and that of certain chelonians and of a cetacean, beyond a homology of the members in their aggregate, I must consider that a medius digit, for example, with three phalanges, corresponds to one with several more than three, and be-

lieve in telical suppression of a phalanx in one case, and a similar redundancy of phalanges in the other case. If I undertake to compare the manus of a bird with its pes, either antitropically or otherwise, I must admit with every single digit a difference in the numerical composition of its homologue. Until our morphological insight has penetrated far enough for the solution of such problems as these, it seems perfectly reasonable to maintain that the objections on the score of numerical composition that have been urged against the antitropic homology of pollex with quintus, and of minimus with primus, apply with manifold force to a majority of the homologies that anatomists consider determined."

V. GENERAL PROBLEMS.

The radical difference of opinion respecting the morphical relations of membra which the historical sketch exhibits between such Syntropists as Owen, for instance, and such Antitropists as Wyman, is not to be accounted for by any assumption of difference in their knowledge of facts or their intellectual power, but rather, as it seems to me, by a recognition of the dissimilarity of the premises which they have admitted, and the methods of reasoning which they have followed: in the one case, the human body has been chiefly employed in making the comparison, and attention has been early diverted to the correspondence of the pollex with the primus in respect to size, numerical composition and relative position, when the manus is in its natural attitude of pronation, as with many quadrupeds. In the other case, more attention has been given to the telical antagonism of the ancon and genu with many animals, and to the relative position of the membra during the early stages of development.

In more general terms, the idea of Syntropy is based upon the obvious resemblance in respect to *size*, *numerical composition and natural attitude* of certain *highly specialized* parts of *peripheral* organs belonging to animals of *high zoological rank*, and in the *adult* condition; while the idea of Antitropy is based upon the antagonism of *relative position* of *proximal* and *less specialized* parts with animals *lower* in rank or at *earlier* stages of development.[1]

Now, without doubt, the question under discussion is primarily one of *structure* rather than of *function*; it is a *morphological* and not a *teleological* problem. Before it can be solved, it is evident that we must first ascertain which are correct of the two groups of premises above

[1] These ideas were advanced by me in part in 45, 21, and more distinctly in 57, (Props. 9 and 10).