Without attempting to defend any of the hypotheses mentioned it may here be pointed out that relatively slight modifications in germinal organization would convert one type into another.

A distinguishing characteristic of the mutation theory is the recognition of elementary characters or properties which manifest themselves in many separate parts of the adult, as, *e. g.*, the presence or absence of hairs or certain colors; if mutations are germinal the widespread distribution of such characters in the adult are easily explained. Relatively slight modifications of the germ, however produced, may lead to profound and widespread modifications of the embryo and adult. I have elsewhere shown reason for believing that the cause of inverse symmetry which occurs regularly among some species and occasionally among all, man included, is to be found in the inverse organization of the egg, and that this inverse organization may be due to the maturation of the egg at opposite poles in dextral and sinistral forms. This case shows that one of the most remarkable and far-reaching forms of variation with which we are acquainted is the result of relatively slight alterations in the localization of germinal substances in the unsegmented egg.

One of the principal difficulties in explaining the origin, on evolutionary grounds, of different phyla has been the dissimilar locations of corresponding organs or parts. These difficulties are well illustrated by the theories which attempt to derive the vertebrates from the annelids, or from any other invertebrate type. If evolution takes place through transformations of the germ rather than of the adult, it is no more difficult to explain the different locations of corresponding parts in these phyla than their different qualities. Changes in the relative positions of parts which would be absolutely impossible in the adult, may be readily accomplished in the unsegmented egg, as is shown by cases of inverse symmetry. The question is here raised whether some similar sudden alteration of germinal organization may not lie at the basis of the origin of new types.

Mutations: THOMAS DWIGHT, Parkman Professor of Anatomy, Harvard Medical School.

It has been clear from the beginning that evolution, if it be a power at all, must work either by minute modifications or by more or less sudden changes. Darwinism is essentially the doctrine of minute modifications increased by selection and controlled by the survival of the fittest. Darwin insisted most strongly on the importance of minute modifications. While holding that 'strongly marked variations' might modify a species without the help of any selection at all, he absolutely denied any sudden changes of importance such as lie at the bottom of the mutation theory. 'Natural selection,' he wrote, 'acts only by the preservation and accumulation of small inherited modifications'; and he asserted that it would 'banish the belief of the continued creation of new organic beings or of any great and sudden modifications of their structure.'

The mutation theory of sudden jumps and, it may be, of long jumps, is far from new; but it is de Vries's merit to be able to show by demonstration what before was only theory. His hypothetical 'pangens' by which the changes are said to be brought about need not be discussed here. A radical difference between the two theories is this: Darwinism pure and simple is essentially fortuitous; it aims in no particular direction, there is no goal; while mutation by producing suddenly a new species, or at least a subspecies, implies the existence of a type and of a law which under certain conditions becomes operative.

In this discussion the anatomist is at a disadvantage to the botanist and the breeder who can experiment. His argument must be largely analogical. He must consider how anatomical observations, both on races and on individuals, are better explained by one theory or the other. Man's body, for I am speaking as an anatomist and leaving aside all consideration of psychology, man's body, as we find it to-day, does not mutate, but varies. What do the variations tell us? Race anatomy offers many instances of fusion of different races and occasionally suggests the occurrence in the past of a race differing sharply from those around it. Perhaps the best example of a race without approximate relations and most indicative of mutation is that of the pigmies. On the whole, race anatomy tells us little. What we call race characters occasionally appear sporadically where one would not expect them. Thus the Mongolian spot on the sacral region of infants has been recently observed on a child in Bavaria. More or less striking features of the disputed Neanderthal race occur among us. The study of anatomical variations in the dissecting rooms of different parts of the world shows that while in all probability there are different tendencies in different races, the variations themselves are of no practical importance. Thus the palmaris longus is absent in 12.7 per cent. at St. Petersburg and in 40.4 per cent. at Strassburg. The 'candelabra' method of division of the carotid artery occurs in 20 per cent. at Strassburg and in 60 per cent. at Breslau. The average absence of the pyramidalis is 12.7 per cent. at Strassburg, 21 per cent. in Massachusetts, while among the Japanese this muscle is wanting in only 3.5 per cent. I have found the psoas minor absent in 60.5 per cent. against 48.7 per cent. at St. Petersburg. The sternalis, so exceptional in Caucasians, occurring at most in from 3 to 4 per cent. was found in nearly 9 per cent. of the Japanese and certain observations on the living seem to show that a larger series would produce even a much larger proportion. This, together with the rare absence of the pyramidalis in the Japanese, points toward specialization in muscle, a feature which strikes one as in keeping with the characteristics of that race. We are familiar with the fact that there seems to be a certain similarity of character among themselves in the fauna of isolated countries, yet it surprises us to find it manifested in the deeper structures. Hrdlicka has observed a form of human tibia, suggesting that of the gorilla, in over 10 per cent. of African bones, which is almost unknown among the whites and not found at all among the Indians. Yet no one seriously believes that the negroes have any special relationship with the gorillas. This phenomenon of similarity, therefore, implies some agency beside selection.

Leaving race anatomy let us see whether the variations, which we continually observe in the dissecting room, point either one way or the other in this discussion. The theory of change by minute variations receives no support from anatomical observations. Precisely what many thought an illustration of Darwinism is its refutation. Huxley foresaw this when he doubted whether variations might not prove a two-edged sword. The fundamental error into which supporters of evolution by selection are logically driven is the unwarranted assumption that similarity of structure can be explained only by descent. Though not formally stated, this is tacitly accepted almost as an axiom.

The student of variations is oppressed by their multiplicity. Those of the biceps, for instance, are bewildering, presenting forms normal in many orders of mammals and which refuse absolutely to be forced into any line of descent. Some, indeed, are mutually contradictory. It is no more

than a truism to say that if an anomaly is to be explained by reversion the structure in question must have been normal in some ancestor; but in view of the vast number of anomalies, we are forced to believe that this ancestor must have been a museum of anatomical curios of the most diverse natures. It is overlooked that if the explanation of a reversion be true it must apply not to one only, but to every possible deviation of structure that is not pathological. Phylogeny must show nothing in the history of the supracondyloid process, for instance, which will not accord with that of the paroccipital process, or with that of the third trochanter, or with that of each and all of the hundreds of variations which the human body may present. Not only has this accord not been shown, but obvious contradictions have been neglected. An explanation has been sought by referring certain peculiarities very far back: even to a hypothetical common vertebrate stem antedating the classes. We admire the learning and the research; but does the explanation explain?

One of the great difficulties of selection has been to account for the appearance of strikingly similar adaptations or arrangements in species from entirely different lines of descent. Analogous to this is the similar irregular appearance of variations. The fossa prænasalis is a deep, sharply marked depression just below the nasal opening, occurring chiefly in low races. It is not to be confounded with the gradual passage of nose into face which is the rule among mammals. I am not aware that it is found among mammals except in the seal, and even there it is less well defined than it may be in man. Here, then, is a sudden change not atavistic and certainly not progressive. The pronator quadratus in man very rarely sends a prolongation to a carpal bone. I have found this as a variation in a chimpanzee, and Macalister in a lion, but in no mammal is it normal. To find it also in turtles and in the *Cryptobranchus japonicus* does not help us much towards an explanation.

It is very suggestive that in certain variations of the platysma by which it enters into various combinations with the facial muscles, in some cases its fibers are in direct continuity with those of muscles which comparative anatomy teaches belong to another layer. It seems as if nature were striving for a certain effect and is absolutely indifferent by just what means it is accomplished.

One of the most significant points of the mutation theory is that it rehabilitates species with its old-time dignity. Though we flounder in our definitions of species, we can not get rid of the thought that it is something, after all. By a strange paradox it is precisely through variations that the tendency towards stability of species is emphasized. In my observations on the human spine I have found that very frequently the effects of a variation in one part are felt in remote parts and, indeed, throughout the spine. Some of these seem directly teleological, others tend to preserve the type. Thus if the last ribs are very small, and this holds good whether they be the normal twelfth pair or the abnormal thirteenth, the rib before the last is usually exceptionally long. In the case of cervical ribs it is common to find the last rib very small, as if the whole thorax had moved up. In cases where there are only eleven thoracic vertebræ not rarely an increase of their size tends to preserve the proper proportions of the thorax. In the lumbar region there are certain striking characteristics in the spread and in the structure of the last three transverse processes which give a definite shape to the whole region. In many cases of numerical variation there is an evident effort to reestablish normal

features as nearly as the modification will permit.

It is to my mind impossible to find any support for a theory of evolution by minute changes from the study of anatomical variations. I should not venture to say, on the other hand, that they give any direct support to the theory of mutation; but at least they are not in disaccord with it.

Systematic Work and Evolution: L. H. Bailey, Director of the College of Agriculture, Cornell University.

Every object of which we take cognizance must be named if we are to record and convey the ideas associated with it. As the names accumulate, it is necessary that we group them, or provide some scheme of arrangement. We classify all categories, even though we do no more than to arrange them alphabetically. Nomenclature and classification are primary intellectual processes.

The number of organisms that we know has come to be legion. These organisms are described in books. The first descriptions accepted the organisms as they are, without serious inquiry of their origins. Definite names have come to be attached to each kind of organism and definite customs have arisen to control the bestowal of the names. Biological nomenclature has become a rigid bibliographical method.

The first object of classification was to afford a perspicuous arrangement of facts. The facts must be pigeon-holed, else they may be lost. Gradually, however, the idea of relationship between the objects has developed, and these ideas have expressed themselves in crystallized schemes of classification. That is to say, classification of organisms is a combination and compromise of bibliographical method and expression of relationships.

Presently, the organisms themselves began to be studied from the physiological side. It was discovered that at least some of the named groups of organisms are not entities. There are all grades of differences, from those peculiar to one individual to those peculiar to many individuals, and to groups of individuals. The organisms are multifarious and elastic, but nomenclatorial and taxonomic systems are editorial and arbitrary.

We are all now committed to the evolution philosophy as a working hypothesis. The greatest problems in the study of organic nature are the determining of the lines of ascent and the means by which they have come about. We study plastic material; at the same time we are making a desperate effort, at least on the botanical side, towards rigidity of nomenclature. Our ideas of what constitutes species and varieties are free and extensible enough, but our methods of designating these ideas still follow the formalism of a century ago —are in fact more inflexible than they were in the time of Linnæus. If nomenclature is inelastic, schemes of classification within the genus or species must likewise be inelastic, for the classification is but an expression of our ideas of the relationships of the objects that we name. Our nomenclature does not express either the knowledge or the point of view of our time.

The Present Status of Systematic Work. —There are three elements in the discussion of systematic work as related to transmutation theories: (1) The idea of a species, (2) the methods of naming and recording, (3) the classificatory schemes themselves.

It would be profitless at this time to enter into a disquisition as to what a species is. The many discussions of this subject are so many admissions that no one knows. The only point I care now to make is that we all recognize the fact that the single word 'species' covers groups of widely different grades of value, of differentiation,