to variation, and have been preserved in given directions and repressed in others, by the operation of the law of Natural Selection. (Darwin.)

II. Genera have been produced by a system of retardation or acceleration in the development of individuals; the former on preëstablished, the latter on preconceived lines of direction. Or, in other words, that while nature's series have been projected in accordance with the law of acceleration and retardation, they have been limited, modified, and terminated by the law of natural selection, which may itself have operated in part by the same law.

III. The processes of development of specific and generic characters have not proceeded *pari passu*, transitions of the one kind not being synchronous with transitions of the other; and that, therefore, species may be transferred from one genus to another without losing their specific characters, and genera from order to order without losing their generic characters.

IV. And as the heterologous terms of the peculiar homologous groups present an "inexact parallelism" with each other; and as types related by inexact parallelism are each among themselves exact parallels in separate series, whose earliest members present "exact parallelism" with each other, it follows—

V. That the heterologous terms or genera in the later series are modified descendants of those of the earlier series; in other words, that certain groups higher than genera are produced from others of similar high value by "descent with modification."

VI. That the result of such successional metamorphoses will be expressed in geologic history by more or less abrupt transitions, or expression points, rather than by uniformly gradual successions.

Of course, under the conclusion stated in Proposition II, the genus Homo has been developed by the modification of some preëxistent genus. All his traits which are merely functional have, as a consequence, been produced during the process. Those traits which are not functional, but spiritual, are of course amenable to a different class of laws, which belong to the province of religion.

Variations in Taxodium and Pinus.

BY THOMAS MEEHAN.

In some remarks before the Academy on July 14th, in reference to adnation in the leaves of Coniferæ, I said that the power to branch was the test of vigor; and with increased vigor came proportionately the power of adnation. I pointed out that this was the universal law through all Coniferæ, so far as I had been able to examine them; and that it fully accounted for the specific identity of many forms supposed to be distinct. I went so far as to suggest that *Taxodium distichum*, Richard, and *Glyptostrobus sinensis*, Endl., were no doubt the same thing, because the only difference between the growing plants was in the different degrees of adnation in their foliage; and because with this adnation was the increased power to branch observed in all other cases. The two points, going along together, seemed to indicate that this could not be a solitary exception to so clearly marked a law. I exhibited specimens taken from *Taxodium*, and from *Glyptostrobus*, showing the approach of the two in the manner the theory indicated.

Since then some new facts have come before me confirming this view in a remarkable manner. On the nursery grounds of Mr. Robert Buist, of the Darby Road, near Philadelphia, are a few trees which I supposed to be the *Glyptostrobus*, but which Mr. Buist assured me were many years ago, selected by him from a bed of some thousand *Taxodium* on account of their peculiar appearance. I exhibit specimens from eleven different trees. It will be seen the suppression of the leaf blades or adnation is in exact proportion to vigor, or the power of forming branchlets, and with this increased vigor the *Taxodium* become *Glyptostrobus*, so far as any comparison of leaves and branches can identify anything.

At the conclusion of my paper on the *laws of adnation*, read before the meeting of the American Association for the Advancement of Science, at Chicago, Dr. J. S. Newberry kindly pointed out that, in a fossil state, *Glyptostrobus* and *Taxodium* were often found side by side, but always with so much difference between the scales of the cones that, while assenting to the general principles of the paper, he could not regard these two plants identical. As cones are nothing more than metamorphosed stems and branches, it is not surprising that the sames laws of adnation which might operate in making the *Taxodium Glyptostrobus*, and which make them look so very distinct in the different stages of adnation, should also operate on the fruit, and make it appear, when at the widest point of divergence, as really different. It should in fact do so, and instead of the differences in the cones of these fossils being any proof of their specific distinctness, it must be received as a natural sequence of the law I would evolve.

The specimens I now exhibit show at any rate that the *two plants* are identically the same. This granted, it completely refutes the generally received theory, that no one species of Coniferæ inhabits at once the eastern and western worlds.

In my paper on variations in *Epigæa repens*, presented for publication last May, I endeavored to show that "cultivation" and "external circumstances" would not account for variations in form to the extent they usually received credit for; but that there was rather a regular principle of growth in form, as well as in substance, independent of outward agencies, which agencies were calculated quite as much to preserve as to originate the growing forms.

Those accustomed to study chiefly from herbaria, and little from living specimens, have no idea of the great variations from one type which many species present. These comparative differences are often so insensibly blended, that it is only when we meet with some very extreme forms that they attract our attention, and then only to note their extreme differences. Even when noted they are contemned as obstructing classification, rather than welcomed as invaluable aids in resolving the laws of form.

In a recent review of part 16 of *Decandolle's Prodromus*, which has lately appeared, with the Coniferæ by Prof Parlatore, the reviewer says: "It must be clear to every one that a great number of so-called species are varieties of one strain, *doubtless produced by localization in different climatal or natural conditions.*" (*Gardener's Chronicle*, page 922, 1868.) As this review is understood to be by one who is himself known as a describer of many Coniferæ, which are doubtless varieties of one strain, it may be worth while to point out, in some Coniferæ, that neither climatal nor any external condition has as much to do with variation in form as an innate power of development, independent of either climatal or local causes.

In one of our commonest pines—*Pinus inops*—a very careful comparative examination will show scarcely any two trees to be exactly alike; the habits of the tree, the shade of color, or the length of the leaves, the size or form of the cone, the scales, or the seeds—in some one point a difference may be found which can easily be described in words. When extremes are brought together the differences are quite as great as characterize different species. By descriptions alone they would be fairly entitled to rank as distinct. The mind fails to unite them. It is only the educated eye which perceives their identity. I exhibit two cones from two trees growing on the banks of the Susquehanna, near Harrisburg. One is very long and narrow—three and a half inches in length, by only three-fourths of an inch wide at the base, and the scarcely projecting scales barely spinescent, the other nearly as wide, but only half the length, and with strongly projecting scales and spines. Unless with previous acquaintance of *Pinus inops* in its natural places of growth, a botanist might well be pardoned for considering these distinct species, yet with the multitude of intermediate forms, all under the same external conditions, how can any "localizations" account for the varieties? I have the same ex-

perience with *Pinus rigida* and *P. pungens*; and it is doubtless true of other species.

I have noted some interesting variations in *Pinus Banksiana*, which in some way do seem to be connected with location, although I have no doubt that ages of geographical travel from a central point conjoined with the principle of inheritance, might find the natural inherent laws of variation sufficient to account for them. Dr. Gray says, in the last edition of his Manual of Botany, it is a shrub or low tree 5 to 20 feet high, giving N. Maine, N. Michigan and Wisconsin, and northward as the localities. I did not collect in northern Illinois, but friends tell me it grows some thirty miles from Chicago, only as a bush. Michaux observes that in Labrador it shows signs of decrepid old age at 3 feet, and in no part of America did he find it over 10 feet. Dr. Richardson, in Franklin's narrative of a journey to the shores of the Polar Seas in 1819—1822, describes it as 40 feet high in favorable situations, but the diameter of its trunk was greater in proportion to its height than in any other pines of the country. Douglass found it to have longer leaves on the Rocky Mountains than elsewhere. In company with Mr. Wm. Canby, I had the opportunity of examining large forests of them growing on the neck of land between Escanauba, on Lake Michigan, and Marquette, on Lake Superior, where we found them just the reverse of Dr. Richardson's experience. Here they were more slender in proportion to their height, not only than any pine of the country, but probably than any pine elsewhere. Most of the trees were from 30 to 40 feet high, remarkably straight, but only from 6 to 12 inches in diameter. We roughly measured one at Escanauba which was about twenty inches in diameter, and perhaps sixty feet high, little shorter than in fact a very fine *Pinus resinosa*, about two and a half feet through, growing near it.

Now these variations have relation to only one particular, that of size; there would no doubt be found others in many respects; but even in this one character no theory of climate or soil will account for them. If a low temperature dwarfs the Labrador specimens, what is to account for the small bushes in Illinois or southern Wisconsin, in lat. 42°? And again, why are these latter in the rich soils of this district so small in comparison with the almost timber trees of a few hundred miles farther north, and in which is usually considered the poorest land of the north-west? Soil and climate may have some influence in aiding variation, but facts show the origin is deeper than these, namely, a native power to change, kept in check only by inheritance and perhaps external circumstances.

I have heretofore reported *Pinus pungens* as growing at Port Clinton; I find it now abundantly on the hills about Harrisburg; so it may be set down as native to the whole interior of the State of Pennsylvania.

Nov. 3d, 1868.

The President, Dr. Hays, in the Chair.

Forty-two members present.

The following paper was presented for publication:

Sixth Contribution to the Herpetologyof Tropical America. By Edw. D. Cope.

Dr. Leidy called attention to two singular specimens presented this evening by Mr. Lamborn.. They were obtained from the Huronian slates near the Dalles of St. Louis River, northern Minnesota. They bear a strong likeness to large coprolites partially imbedded in portions of slate. They not only have the usual form of coprolites, though flattened, but have an apparent spiral arrangement. Taken from the surface slate, the bodies, where exposed to the air have been more readily decomposed than the slate. A broken surface ex-

[Nov.