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CHARLES C. LITTLE AND JAMES BROWN.

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ART. XL.—ENUMERATION OF THE RECENT FRESHWATER MOLLUSCA WHICH ARE COMMON TO NORTH AMERICA AND EUROPE; WITH OBSERVATIONS ON SPECIES AND THEIR DISTRIBUTION. By S. S. HALDEMAN, Professor of Zoology in the Franklin Institute, Philadelphia.

1. *Paludina vivipara*, *Lin.*
2. *P. fasciata*, *Müll.* *achatina*, *Lam.*
3. *Physa hypnorum*, *Lin.* *elongata*, *Say.*
4. *Limnea palustris*, *Müll.* *elodes*, *Say.*
L. stagnalis? *Lin.* *jugularis*, *Say.*
L. truncatulus, *Müller.*
5. *Planorbis albus* “
P. nitidus? “
6. *Cyclas calyculata*, *Draparnaud.*
7. *Pisidium appendiculatum*, *Leach.*
P. amnicum? *Müller.*
8. *Alasmodon margaritiferus*, *Lin.*

Mr. James Sowerby catalogues several other European freshwater species as found by Dr. Richardson, in the upper Canadian lakes; but I am unwilling to adopt these determinations on the present occasion, except that of *Pisidium appendiculatum*, which cannot well be mistaken for another species, and which is mentioned as occurring from Lake Superior to Saskatchewan.

It is difficult to determine the percentage of species common to the two continents, and the result will vary according to the point in which the facts are viewed. *Paludina vivipara* and *fasciata*, for example, appear to be the only European members of the restricted genus, and are found here, where the species are more multiplied. We count Unionidæ and Melaniadæ by the hundred, whilst the former family has but few species, and the latter but two recorded recent *Melania* in Europe.* Under these circumstances, I have deemed it

* I doubt much whether these will not be found, upon a closer scrutiny, to belong to *Melanopsis*, which appears to be as essentially a European—as *Melania* is an

better to leave those families out of the question, which present great discrepancies, and base my conclusions upon the genera nearly equally abundant in species upon both sides, which will be found to be the case in the Physadæ. Thus we have about 50 species of *Physa*, *Limnea*, and *Planorbis*, of which three, or 6 per cent. are European: but with *Ancylus*, the number of Physadæ is raised to 60, reducing the percentage to *five*. If we reject *Ampullaria* as a southern form not admissible into the fauna of Europe, the remaining portions of Lamarck's Peristomata will not cause the result to vary, so that we may safely assume the last named number as indicating the proper proportion.

In preparing lists like the foregoing one, we are met by several difficulties, the most important turning upon the identity or non-identity of certain shells, apparently the same, but found upon different continents; with no apparent existing means of traversing the intervening ocean. The question at issue resolves itself into two propositions: 1. Animals occurring in separate regions, which they could not have attained by crossing the intermediate space, however much they may resemble each other, are *distinct* species.

2. Similar animals, under whatever circumstances they may be found, constitute but *one* species.

At the very onset we are met by the question, What is a species? and sides will be taken according to the answer each one is ready to adopt.* The definition of a species does not

American form. Some conchologists consider the two genera identical, an opinion which the characters and geographical distribution will not justify, notwithstanding the near alliance between some of the species. *Melanopsis* is not an American genus, the species referred to it by M. Deshayes, (*Melania nupera Say.*) constituting with several others a distinct form, of which *Lithasia geniculata Hald.* is the type.

* “We have agreed that a species shall be that distinct form originally so created, and producing, by certain laws of generation, others like itself. There is this inconvenience attending the use of it by naturalists, that it assumes as a fact, that which, in the present state of science, is in many cases a fit subject of inquiry; namely, that species, according to our definition, do exist throughout nature. It is too convenient a term to be dispensed with, even as an assumption; only care should be taken that we do not accept the abstract term for the fact.” Bicheno, *Lin. Trans.* xv, 482. “There is no law whatever hitherto established, by which the limits of variation to a given species can be satisfactorily assigned, and until some such law be discovered,

necessarily include descent from a single pair, because the first male and the first female would, by the definition, be of different species.* If we assume that geographical position is of more value than specific character, it follows that we cannot name *Physa hypnorum*, until we know the locality of the specimen; nor can it ever be ascertained whether this species inhabits both continents, if the very enunciation of the fact calls forth its denial.† Hence tables like that given become useless to the geologist; for should he have reason to infer that certain regions were once united by a chain of islands, for example, he will expect a certain community of animal species; but upon placing his specimens before a zoologist, he is compelled to abandon his view by being informed that no species is common to the two regions; the assertion not being founded upon the positive evidence furnished by the objects themselves, but upon the mere opinion that they would have been unable to traverse the intervening ocean.‡

If the same species may inhabit distant regions, the fact may be accounted for in several ways, as by

we cannot expect precision in the details of systematic botany." Prof. Henslow, *Mag. Zool. Bot.* i. 116.

* See *Mag. Nat. Hist. N. S.* ii. 622. Will any one contend that when "grass" was first created, the meadows remained barren until covered by multiplication from a single plant?

† In the same manner, if identity of species in the parents were a just deduction from the occurrence of a prolific offspring, the question touching prolific hybrids could never be settled; and if two precisely similar shells are pronounced distinct merely because they are found in strata of very different ages, we can never determine whether a fossil species can occur in distinct formations. This point will be noticed further on.

‡ The circumstances of the existence of dissimilar forms of a common type are parallel to those of the (*Ovis ammon*) equally found identical or different in Asia, Africa, and the islands of the Mediterranean, which existed anciently in Spain, and at this moment is spread over a great part of western North America. In no case are these animals suspected to have been transported by human intervention, and yet they are located in some places where, without the aid of man, they cannot have migrated, unless we admit of changes on the surface of the earth, since the present zoology was in being, of such magnitude as to include the formation of the Mediterranean — the separation of the British Islands from the continent of Europe — of the Indian Islands from that of Asia — and the formation of a channel to cut America from connection with the Old World. — Col. Hamilton Smith, *Nat. Hist. of Horses*, p. 67.

1. Transportation.
2. Former connection of the regions.
3. Distribution from several original centres.
4. Transmutation.

Of these, the two first are the most obvious, but the third requires to be admitted with great caution, in practice, at least, if not in theory; for, if we suppose that certain physical conditions of the earth's surface require particular organic forms to develop the great ends of creation, we may conceive two distinct regions to exist, with physical characters so nearly alike, as to be better suited for the habitation of some single species than for two distinct ones; and, in the production of a new series of beings, after a geologic convulsion has swept a multitude from existence, it is not impossible that certain exterminated species may be revived, to demonstrate a partial similarity between the two epochs, like that indicated by the plentiful occurrence of genera, the identity of the members of which we seldom have occasion to doubt. But, although many animals are able to live under varying circumstances, such a multitude of conditions are requisite to demand the independent existence of the same species in the most distant localities,* that the probabilities against such an occurrence cannot be less than the proportion of ten thousand to unity.

It remains to notice the hypothesis usually named after Lamarck, certainly not because it owes its origin to him, but doubtless on account of his lucid exposition of it. Mr. Lyell gives a pretty full analysis of these views, for the purpose of disproving them, and he has apparently succeeded in the attempt; but, as several zoological views have been advanced since the publication of this philosopher's "Principles of Geology," which tend to invalidate some of the arguments brought forward, it is impossible to do justice to the hypothesis in question without alluding to them. Thus the cat mummies of Egypt were said to be indetical with the mod-

* See Dr. Weissenborn's remarks, in Charlesworth's *Mag. Nat. Hist.* ii. p. 623.

ern *Felis domestica*; and such was the general opinion, until the discovery, by Dr. Rüppell, of the genuine analogue of the embalmed species, in the *Felis maniculata* of Noubia. I believe Professor Bell to be correct in deciding that *Felis domestica* can neither be referred to this species, nor to the *Felis catus* found wild in the forests of Europe.* Again, great stress has been laid upon the contrasts presented by the so called varieties of the domestic dog; but the hint given by Pallas, that they are merely prolific hybrids,† accounts for the variations in a much more satisfactory manner than to suppose them identical with some single primary form. Col. Hamilton Smith, in his valuable treatise on the Canidæ, ‡ has developed these views in a masterly manner, and, at the same time, has reduced the family to such perfect order, that I do not hesitate to adopt his views.§ The Lamarckian hypothesis of appetency, as he left it, seems clearly untenable, but, in a modified form, affords room for further discussion. Thus it appears impossible that a bird, with detached toes, should ever acquire webbed feet by mere dint of swimming or desire to swim; but, as all animals are formed with a greater or less approximation to certain models, why may not the germ which would result in a swimming organ (as that to which the water dogs owe their partially webbed feet) be present in a terrestrial animal, just as the foetal brain is successively that of a fish, reptile, and bird, before it is that of a perfect mammal? or as the mammary glands have been known to increase in number, and to occur by deviation, in the inguinal region of the human subject? ||

* British Quadrupeds, p. 185.

† Caldwell's Unity of the Human Race.

‡ Nat. Libr. See, also, Horses, p. 70, where he remarks, "It seems, therefore, more consonant with the distribution of several genera of animals on the earth's surface, to believe that osculating forms existed *ab initio* distinct, circumstanced to accomplish certain ends, such as the service of man, and therefore framed so as to render them fusible into one species." The same view had been previously taken by Mr. Eyton, a British ornithologist, Mag. Nat. Hist. (N. S.) i. 359.

§ If these prevail among zoologists, it will be necessary for the pseudo-Lamarckians to remodel their arguments to some extent, as they will be reduced to such "facts" as are given in the next note but one.

|| In the eyeless fish and crustacean of the Kentucky cave, the germs of visual organs must exist, and only require light to cause the eyes themselves to appear in

Mr. Lyell endeavors to show that all our efforts to remove a species from its type are successful only to a certain extent, and that the maximum deviation is attainable in a few generations.* The Lamarckian contends that the lapse of time and amount of physical revolution are not sufficient to cause any appreciable difference between the embalmed Egyptian remains and the forms now existing; but we are at liberty to step into the field of palæontology, and here, at least, we should be able to find proofs of a gradation of species, as I believe Cuvier remarks. If this view is sustained by facts, it becomes an important argument, and one which is generally regarded as final; but that it should not be received as absolutely conclusive, I will endeavor to show.

Let us, for a moment, suppose a transmutation of species possible, and then attempt to account for the asserted absence of the intermediate links. In the first place, the ability to produce these links would constitute the main ground, (the possibility of hybridity being commonly overlooked) for proving the *identity* of the two previously-admitted species — a mode now in use, and considered of great service, especially when it is well understood that there are distinct species, among the Unionidæ, for example, actually differing less from each other than the known varieties of certain variable species,† which a Lamarckian might suppose to be of so recent an origin as not to have yet become settled in the possession of their proper diagnostic characters.‡ Indeed, notwithstand-

due time. Some insects, which are usually apterous, acquire wings under certain circumstances. Westwood's Introduction, ii. 468, 481.

* Entertaining the opinion that the color of the native American depends upon climate, it was not unusual for authors of the last century to affirm that the descendants of Europeans had already made some progress in a change of color. Azara states that the Russians are black. Marco Polo affirms that some of the distant Orientals had tails. Dr. Prichard asserts that the heads of the white race in the West Indies approach those of the original natives in form, independently, as he seems to suppose, of intermixture; and that the climate of Virginia, which was formerly said to darken the European, now bleaches the skin, and converts the wool of the blacks into hair in a few generations, when they are kept in the families of their employers, whilst the field laborers retain the original color.

† Mr. Lyell admits that, if this should be the case, it would have a tendency to cast an additional doubt upon the definite nature of species.

‡ See my Freshwater Univ. Mollusca. PLANORBIS, p. 26.

ing the assumption to the contrary, by authors who have little practical acquaintance with the details of natural history,* the proper discrimination between species and variety is one of the greatest difficulties which the naturalist has to encounter; and he who is successful in this department is entitled to a rank which comparatively few can attain.†

In the second place, although we may not be able, artificially, to produce a change beyond a definite point, it would be a hasty inference, to suppose that a physical agent, acting gradually for ages could not carry the variation a step or two farther; so that, instead of the original, we will say four varieties, they might amount to six, the sixth being sufficiently unlike the earlier ones to induce a naturalist to consider it distinct.‡ It will now have reached the limit of its ability to exist as the former species, and must be ready either to develop a dormant organic element, or die; if the former is effected, the osculating point is passed, and the species established upon the few individuals that were able to survive the shock. If the physical revolution supposed to be going forward is arrested or recedes, the individuals which had not passed the culminating point remain as a fifth variety, or relapse towards their former station; whilst the few which have crossed the barrier remain permanently beyond it, even under a partial retrogression of the causes to which they owed their newly-developed organization. We may suppose some spe-

* This remark does not apply to Mr. Lyell, whose arguments indicate a knowledge of the subject seldom apparent in the writings of those to whom I allude generally.

† "In very extensive genera, the distinctions of species are so minute, that it requires the most practised eye to separate them; and, indeed, there are some groups, the species of which are so intricately blended together, that no two entomologists are agreed as to their distinctness." — WESTWOOD.

‡ This slight organic change might bring it sufficiently near a cognate species to allow of the production of a prolific hybrid, previously impossible; for, as Mr. Lyell remarks, (Principles, ii. 372, London, 1837,) "Hybrids have sometimes proved prolific, where the disparity was not too great." He asks, (p. 434) "if species in general are of hybrid origin, where are the stocks which combine in themselves the habits, properties, and organs, of which all the intervening species ought to afford us mere modifications?" An answer may be partly found in the complaint of Professor Henslow, (Mag. Zool. Bot. i. 117,) that botanists describe certain species as "duabus prioribus *exactly* intermedia."

cies and individuals to be more able to pass than others, and that many become extinct, from inability to accomplish it. Under this point of view, a hiatus, rather than a regular passage, is required between a species and that whence it is supposed to be derived, just as two crystals may occur, nearly identical in composition, but without an insensible gradation of intermediate forms,* the laws, both of organic and inorganic matter, requiring something definite; whence the rarity of hybrids and monsters, themselves subject to established laws.

My meaning will, perhaps, be better understood by one or two illustrations. We all know that marine and fluviatile mollusca have their peculiar distinctive characters. Let us, then, suppose a species of *Melania* to inhabit a stream into which a salt lake effects a discharge, the saline mixture being, at first, so much diluted as not to occasion the animal much inconvenience, until by the gradual enlargement of the outlet of the lake, the amount of the foreign ingredient is so much increased, that the mollusk finds great difficulty in living, and must eventually perish, unless it can accommodate itself to the saline medium. The form, however, not being marine, the extreme case is presented, of a necessity to change into a different genus — *Fusus*, for example. Now there is a shell confined to the Holston River and its branches, which is, to all appearance, a *Fusus*, so that Say, the leading American conchologist of his day, called it *Fusus fluviatis*. According to the Lamarckian hypothesis, this mollusk may have descended from individuals of the more widely-distributed and nearly-allied *Melania armigera*, Say; and its transmutation (though now a fluviatile species) may have been accomplished by the agency of salt water. Now, although we will not assert that the salt water they inhabit is the cause of the siphonal canal in a large proportion of marine univalves, or even insist that the want of this medium has some connection with its absence in the numerous freshwater species, we are

* The same mineral may crystallize with three, six, or twelve angles, but not with five or seven. Are the phases of organic morphism subject to less definite laws?

acquainted with, yet, when we find but a *single* exception among hundreds of species on both sides, I cannot think it unimportant to inquire whether we are to consider it a mere coincidence, that this anomalous *Fusus fluvialis* should occur in a stream which rises in a salt region.

I have noticed, in another place, the great analogy existing between the aquatic *Paludina subcarinata* and the terrestrial genus *Cyclostoma*. In the former genus, the rim of the aperture lies in close contact with the body of the shell, and the opercle is composed of concentric elements, whilst, in the latter, it has a tendency to disunion, and the opercle is constantly subspiral. In the species in question, "we find a true *Paludina* occasionally rounding its aperture, throwing it off from the body whirl, and not only assuming the physiognomy of *Cyclostoma elegans*, but actually departing so far from the normal character of its genus, as to construct a cyclostomoid opercle, that of the young enlarging spirally. This, however, does not continue; the animal, as a *Paludina*, is not endowed with the elements necessary to produce the entire opercle of a *Cyclostoma*; so that, after continuing it to a certain point, the layers become normal or concentric, apparently indicating the impossibility of a departure from its generic type beyond a certain limit."* When I detected this peculiarity, I considered it as affording an excellent illustration of the theory of "representation;" and, although I still entertain the same opinion, I cannot close my eyes to the fact that representation itself must take its place as an argument in favor of Lamarck's views.

Some authors, having adopted certain views of species and varieties almost at random, and independently of zoological or physiological considerations, call in the Lamarckian principle as far as their notions render its assistance necessary, and chiefly that they may refer the members of the human race to one zoological species, whilst they will admit that the American *Vulpes fulvus* is distinct from its European

* Freshwater Univalve Mollusca, PHVSADÆ, p. 17. It will be remembered that Lamarck, at one period, confounded the genera *Cyclostoma* and *Paludina*,

analogue. This brings us to a result published by a practical zoologist, our associate, Professor Samuel George Morton, well known by his researches in anthropology. Finding it impossible to account for the variations in the races of men on the grounds usually brought forward, he contends that although they constitute one species, the five races were endowed with distinctive characters *ab initio*.* This view places a neutral ground between such as contend for the unity of the human species, and those of a contrary opinion; and is of such a nature that both parties may occupy it without compromising the conclusions each has respectively adopted. Yet it is only satisfactory in the study of the human species, of which we have records and monuments of great antiquity, for it is not very likely that naturalists will be ready to admit that certain shells are referable to a single species, although endowed with distinctive characters at the period of their creation.

The hasty assertion has been sometimes made, that if transmutation of species be possible, the study of natural history becomes useless; that is to say, a science is useless if its principles militate against our view of that science, and *Avena sativa* must not be studied by the botanist because it is asserted to change into *Secale cereale* under peculiar circumstances.† The Lamarckian might with equal propriety insist that this constitutes the chief inducement to study; that without it every species would be isolated in creation; that there would be neither genus, order, nor family; no relation between the wings of a bird and the anterior limbs of a quadruped; and the seven cervical vertebræ, so constant in the mammalia, were accident. He might consider his views as the foundation of comparative anatomy, the key to the theories of representation and types, and the basis of the classification of organized bodies. Nor could he perceive that the study of geology would be affected by it to such a degree as to render the deductions from organic remains less useful than under the sup-

* See Swainson's Geogr. of Animals, (Cab. Cyc.) p. 2.

† See Charlesworth's Mag. Nat. Hist. i. 574 and ii. 670.

position that species alone are permanent, whilst everything else is subject to change; for were they transmutable, it would be in the course of the long periods, and during the progress of the important changes which the geologist notes; so that whether these fragments be regarded as formerly identical, but at present distinct from existing forms, or as distinct at all times, the general result and its applications remain undisturbed.

Mr. Lyell lays great stress (vol. ii. p. 369) upon the fact that Lamarck does not cite the appearance of any *new organ*, but I have endeavored to show, that the theory is not dependent upon the production of any organ not already existing in an undeveloped state. His remarks against appetency are well directed; but the idea, in the extent to which it has been carried, should be discarded as more detrimental than useful to those who maintain the instability of species; because whilst its connection with the subject is very slight, the discussion of it has a tendency to turn the unthinking inquirer aside from the true and philosophical basis of this important question, leading him to believe that if appetency be disproved, transmutation falls with it. The objection (p. 375) that numerous links in the animal series are wanting, has not much force, as there are many causes to remove them, or prevent them from becoming permanently established; and the original creation may have included a multitude of beings of all classes. Moreover, the species of the vertebrata and articulata may be comparatively stationary, and the molluscous division peculiarly liable to transmutation and hybridity. The reason why the lower orders still exist, is to be looked for in the fact that they are fitted for the circumstances under which we find them. The researches of Professor Forbes, in the Grecian archipelago, prove that whilst some species are gradually verging towards extinction, others, belonging to a more recent period, are gradually increasing in numbers.

I pretend not to offer an opinion for or against the Lamarckian hypothesis, being more anxious to show the in-

sufficiency of the standing arguments against it, and the necessity of a thorough revision of them, than to take a decided stand (upon a question which I regard as open to further discussion) before its facts have been carefully observed, or the resulting generalizations properly deduced; so that, whether it be admitted or not, it is entitled to the benefit of all the discoveries which can be brought to bear upon it; and, on this account, I have not hesitated to give a slight sketch of the theory of transmutation, as I conceive it to be modified by some of the results of modern science.*

I had intended to prepare a list of insects common to Europe and North America, to be presented on the present occasion; but, as our collections are not in a condition to admit of such an undertaking, and as such a list would have contained the names of a number of species which have probably been introduced through human intervention, (as *Bruchus pisi* or *Dermestes lardarius*) I resolved to take some family of nearly equal distribution upon both continents, and which had been pretty thoroughly studied. For this purpose, I have chosen the *Brachelytra*, most of the species of which are small, and of such habits as to prevent them from being readily introduced artificially.

Of this family, Erichson, the highest authority, enumerates 220 North American (exclusive of Mexican) species, of which 12, or five and half per cent., inhabit Europe. But recent discoveries raise our species to 250, of which none of the additional ones have been identified as European, (perhaps from inability to institute comparisons); on the strength of which, we may reduce the percentage to *five*, when a remarkable coincidence is apparent with that already assigned to the *Physadæ*. Of the genus *Philonthus*, 22 American species

* These views have been held by men of learning and piety at various times. Thus Ray and Swammerdam believed that the parasitic hymenoptera, to which a dead caterpillar apparently gives birth, may result from the division of its vitality into numerous portions, and no one finds fault with the "evil tendency" or "absurdity" of the idea. Now, the objection appears to rest not so much against the opinion as the source whence it comes; in other words, Ray was an Englishman, whilst Lamarck was a Frenchman.

are enumerated, (or 33, including Mexico,) of which no less than eight occur in Europe; and, of these eight, common to Europe and North America, but one has been detected in the West Indies. Erichson cites three species of *Brachelytra*, common to Europe, Asia, and America; another, as common to these countries and South Africa; three, as found in Europe and South America; and but a single species as appertaining to both divisions of the American continent.* The last fact is a curious one, and shows how very distinct the faunas of approximate regions may be. The author, to whom I have just alluded, remarks that northern species may be found in the mountains of temperate regions,† and cites *Tachinus elongatus* as occurring in Sweden, Unalashka, and the mountains of Switzerland.

Mr. Westwood states that a large species of *Cybister* (an aquatic coleopter) occurs at Senegal, Guinea, Cape Good Hope, Madagascar, Mauritius, and in the Indian archipelago; and Mr. Kirby, that *Sphodrus terricola* inhabits Great Britain and Valparaiso,‡ *Carabus victinghovii*, Siberia and America, and a species of *Isopleurus*, India and the Rocky Mountains.§ M. Laporte affirms || that *Nogrus griseus* is found in the four quarters of the globe; and M. Guérin Méneville, that a crustacean, (*Pontonia custos*) found parasitic in bivalve shells at Rio Janeiro, is identical with individuals from the Morea. Probably *Libinia cana*, *liculata Say*, and certainly *Cancer irroratus, Say*, inhabit the Atlantic and Pacific coasts of North America; ¶ and, according to Professor Ehrenberg,** two thirds of the microscopic protozoa hitherto detected on our continent, inhabit Europe also.

Of the species indicated in the list at the head of this paper, *Paludina vivipara* is found in Ireland, England, Sweden, Germany, and Sicily; *Physa hypnorum* in the same,

* North and South America present us with entirely distinct groups of many animals, as in the order Rodentia.

† See Dr. Pickering's botanical map, in the Amer. Phil. Trans., vol. iv.

‡ Bridgw. Tr., Am. ed., p. 29.

§ *Ib.* p. 494, note.

|| Ann. Ent. Soc. Fr. i. 397.

¶ Journ. Acad. N. S. viii. 103, 116.

** Am. Journ. Sci. xlv. 309.

except Sicily; *Pisidium appendiculatum* in England and Sweden; and *Limnea palustris* in Ireland, England, Scotland, Mona, Germany, Sweden, and Corsica.*

After the authorities I have cited in favor of a community of species between distant regions, I think it indicates a considerable degree of boldness to deny the identity of objects never seen, on the strength of a preconceived opinion, which, if true, (and I am far from making the remark as argument against it) will destroy much of the interest with which geographic zoology and botany are invested, and originate a more untenable theory than that of appetency; for, according to the latter, after a fish would have become a bird, it would be a bird; but, according to the former, it would still be a fish; † *Cynthia cardui* would be from three to seven species, according to the extent of country required to constitute a zoological region, as it inhabits North America, South America, Europe, Northern, Western, and Southern Africa, the Islands of Madagascar and Bourbon, Bengal, China, Java, and Australia; and *Colias edusa*, another frail butterfly, would, in like manner, become four species, on account of its occurrence in North America, Europe, Caffraria, and Nipal.

In connection with this subject, I wish to protest against the practice of making distinct species of similar organic remains, merely because they are found in different strata or formations — a proceeding which must end in the admission that a genus can contain more species than distinct forms, (which involves a contradiction) and, indeed, an infinite number of species. There must, however, be a limit to species, and the limit must vary with the genera; because an insect, having more organs than a worm, affords a greater

* Gray's Torton, p. 30. Mr. Lea has a beautiful and extensive series of *Alasmodon margaritifera*, from various parts of Europe and America, including Columbia River.

† This inference is fairly deducible from the writings of Dr. Prichard, and others of the same school, some of whom are continually making attacks upon Lamarck, for the general or uncritical reader, in a manner ingeniously calculated to keep the merits of the question out of view. It is a matter of congratulation to the friends of the French philosopher, that he never produced anything approaching this in absurdity.

number of distinguishing points. Moreover, whilst we admit that there may be 500 species of *Helix*, we cannot allow the possibility of 200 *Ancylus*, with the usual amount of character, because the shell is of the most simple form, and deprived of a number of parts and modifications which afford good specific characters in the former genus. It may, indeed, be insisted, that, for all we know to the contrary, one, two, or fifty, *species* of *Ancylus* may be hidden under each distinct *form*, and that, in this manner, there may be as many *Ancylus* as *Helix*. To assert that two monomorphic animals, belonging to one of the families having uniform tints, are distinct, because, if we could metamorphose them into members of another family, having varying colors, the tints of the two (not being subject to the law of uniformity) might present a specific* variation, is to imagine an organic iso-morphism † of which we have hitherto had no evidence. Admitting, however, that, in an imaginary metamorphosis, two individuals (none of which are absolutely alike in every point) of a species of *Melania*, should result in two distinct species of *Conus*, of different markings and colors, I conceive that we would not be justified, either in assuming the specific difference of the former, or the identity of the latter, which we derive from them; because, although, as *Conus*, they would be distinct from the possession of a different modification of character, as *Melania*, they must be alike, the characters being identical. Let us now suppose our metamorphosis to take the opposite direction, the subjects being two species of *Helix* of the same size, form, color, and texture, of shell and ani-

* "SPECIFIC, that makes a thing of the species of which it is; designating the peculiar property or properties of a thing, which constitute its species, and distinguish it from other things. SPECIFIC CHARACTER, a circumstance or circumstances distinguishing one species from every other species of the same genus." — Webster's Dict. It is evident, from these definitions, that, where there is no specific character, there can be no distinct species; so that, to prevent confusion, it will be necessary for those whose views I am opposing, to adopt some other term, instead of *species*, to designate what they contend for.

† This term may be introduced into zoology to indicate the resemblance which a parasitic insect bears to another, into whose nest it must be able to penetrate unobserved.

mal; one with a columellar fold, a thickened outer lip, armed with teeth, and a wide umbilical depression; the other without a thickened lip, teeth, fold, or depression. In converting these dissimilar shells into *Ancylus*, they must produce but one species, notwithstanding the extent of their disagreement, because the latter genus never has a thickened lip nor teeth, and cannot possibly have the remaining characters which *Helix* presents. The same reasoning has a bearing upon higher divisions, and prevents us from setting aside a genus like *Helicina*, because there cannot be a corresponding genus allied to *Patella*; and it is fatal to the quinary, or any other numerical arrangement, because, in proportion to the simplicity of the form, and the reduction of the organs, must the number of groups and aggregates diminish, of whatever value or denomination they may be.* It appears needless to assert that the absence of characters upon which generic and specific distinction would depend, renders two forms identical; as, in natural history, we cannot admit "a distinction without a difference."

I am aware that species from distant localities, long confounded, have eventually been found to present distinctive characters; but, whilst this ought to teach us caution, it should not lead us to pronounce all such objects distinct, until similar diagnostic characters be detected, or we adopt a rule in the one case which we reject in the other; namely, that objects are distinct which appear identical, under the operation of a peculiar law, only when remote localities are concerned, which exerts no influence upon animals of the same region. Thus, if *Cicada septendecim* were to occur in Australia, it would be considered a distinct species, whilst the seventeen broods of our own country constitute but one; although it admits of a doubt whether all are the descendants of a single pair. The same remark will probably apply to all the species of this genus, the period required to bring them to maturity being unknown. Moreover, the very fact of a species living

* This idea is borne out by the fact that an ornithologist is the proposer of the quinary, and an entomologist of the septenary arrangement.

under modified habits, in distant localities, may produce a variation which, under the circumstances, would be considered specific, no intervening links being possible; so that, after all, such species may be founded upon *quasi* characters,* just as two mollusks have been described under separate names, because one lives in rough water, and has a strong shell, and the other in smooth water, with a thin shell, when the varying texture of the shells has depended upon the locality.† Now, as most of the species named in my list do not present appreciable distinctions, although found in regions widely separated, it remains for those who contend that localities indicate species, to inform us how far west of its present position Ireland should have been placed, to induce them to believe its *Paludina vivipara* another species; and, in case this island were to occupy the middle of the Atlantic, whether the analogous shell should be considered as appertaining to the American, the European, or to an entirely distinct species. The same question might be raised with respect to *Rhinoceros sondiacus*, found equally in Sumatra and Java; and *Elephas maximus*, because it inhabits some of the Asiatic islands, as well as the continent. Mr. Jenyns is inclined to doubt the validity of "many of the species found in distant latitudes, which, although presenting *the closest affinity* to some in Europe, have been considered distinct by naturalists, principally on the ground of their inhabiting such different geographical positions." — *Mag. Zool. Bot.*, i. 25.

* They may be what Dr. Engelmann calls *geographical varieties*, "where no specific distinction can be discovered the natives of both continents, but where the American and European variety can always be distinguished by some points of minor importance." — *Am. J. Sci.*, vol. xlvi. p. 94. According to the same botanist, ten out of twelve American species of *Equisetum* are found in Europe. — *Ib.* p. 81.

† "We often regard a different habitation as a proof of a difference in species, while it may be the cause of the variation." — *Gray's Turton*, p. 240. Schlegel, a German herpetologist, after stating his belief that the American *Rana halecina* and *sylvatica* are identical with European species, remarks that it is "d'un intérêt infini-ment plus puissant, de savoir que nos deux espèces de grenouilles communes d'Europe sont répandues à peu près sur le même degré de latitude dans tout l'hémisphère boréal, et qu'elles présentent dans des contrées aussi distantes et de nature diverse, comme le sont l'Europe, le nord de l'Afrique, l'Asie tempérée, le Japon et l'Amérique septentrionale des différences si peu notables, que les naturalistes auront de la peine à les signaler, et qu'ils ne viendront pas à bout d'en énumérer de constantes." — *Revue Zool.*, 1838, p. 321.

ART. XLI.—DESCRIPTIONS AND NOTICES OF SOME OF THE LAND SHELLS OF CUBA. By A. A. GOULD, M. D.

WITHIN the last two years, I have received several parcels of shells, many of them containing the living animal, collected by Mr. John Bartlett, on the island of Cuba, near Matanzas, accompanied by careful memoranda of the habits and stations of the animals. A few of them were briefly noticed on the cover of the first number of the present volume; most of these I have since ascertained to have been described by Dr. Pfeiffer, in "Wiegmann's Archiv für Naturgeschichte," for 1839–40, a work which had not then reached this country. As however, the study of conchology no longer consists in observing the external configuration and marking of shells, but embraces the study of the animals producing them also, it may not be amiss if I give a list of the shells, with such observations as were noted by Mr. Bartlett, and such as I have made upon the animals which reached me alive; all that relates to them as existing in Cuba, is, of course, derived from him.

The principal localities mentioned are La Carolina, Retiro, Mount Vernon, St. Cecilia, and Santa Ana, which are coffee estates, (cafetales) in the district of Sumidero, and in the neighborhood of Coliseo post office, a distance of twenty to twenty-five miles south east of Matanzas. San Jorge is a sugar plantation on Sagua la Grande river, about one hundred and fifty miles east of Matanzas.

Coffee trees are planted about six feet apart, and on a portion of most estates plantain trees grow in the spaces. When too exuberant many of the smaller ones are cut down, and when the fruit is mature the old trees are cut close to the ground; being full of sap they moisten the earth where they lie. Under these are found some of the *Helices*, and most of *Helicina* and *Glandina*, attracted, no doubt, by the shelter and moisture thus afforded.