GS426

THE

POPULAR SCIENCE MONTHLY.

OCTOBER, 1873.

SILK-WORMS AND SERICULTURE.[1]

By A. DE QUATREFAGES.

TRANSLATED BY ELIZA A. YOUMANS.

GENTLEMEN: When your honorable director invited me to speak before you, I felt much embarrassed. I desired both to interest and instruct you, but the subjects with which I am occupied are of too abstract a nature to offer you much interest. In entering upon them I run the risk of tiring you, and, as people who are tired are little instructed, my aim would be doubly missed.

However, among the animals I have studied, there is one which, I think, will awaken your attention. I mean the silk-worm. Its history is full of serious instruction. It teaches us not to despise a being because, at first, it seems useless; it proves that creatures, in appearance the most humble, may play a part of great importance to the world; it shows us that the most useful things are often slow to attract public attention, but that sooner or later their day of justice arrives. It teaches us, consequently, not to despair when valuable ideas or practical inventions are not at first welcomed as they should be, for, though their triumph is delayed, it is not less sure.

Perhaps, also, in choosing this subject, I have yielded a little to national egotism. I was born in that province which was the first in France to understand the importance of the silk-worm; which owes to this industry, fertilized by study and management, a prosperity rarely equalled, and which, of late cruelly smitten, bears its misfortunes with a firmness worthy of imitation.

We are to speak, then, of industry, of studious care, of perseverance, of courage; I am certain that you will be interested.

Pemit me, at first, to make a supposition—what we call an hypothesis: what would you say if a traveller, coming from some distant

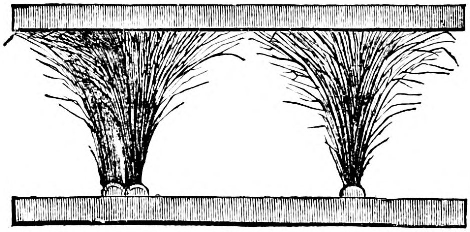
[1] A lecture delivered at the Imperial Asylum at Vincennes.

country, or a philosopher, who had found in some old book forgotten facts, should tell you, "There exists, in a country three or four thousand leagues from here, in the south of Asia, a tree and a caterpillar. The tree produces nothing but leaves which nourish the caterpillar." To a certainty, most of you would say at first, "What of it?"

If the traveller or the man of learning should go on to say: "But this caterpillar is good for something; it produces a species of cocoon, which the inhabitants know how to spin, and which they weave into beautiful and durable fabrics. Would you not like to enter upon the manufacture?" You would infallibly reply: "Have we not wool from which to weave our winter vestments, and hemp, flax, and cotton, for our summer clothing? Why should we cultivate this caterpillar and its cocoons?"

But suppose that the traveller or philosopher, insisting, should add: "We should have to acclimate this tree and this caterpillar. The tree, it is true, bears no fruit, and we must plant thousands of them, for their leaves are to nourish the caterpillar, and it is necessary to raise these caterpillars by the millions. To this end we must build houses expressly for them, enlist and pay men to take care of them—to feed them, watch them, and gather by hand the leaves on which they live. The rooms where these insects are kept must be warmed and ventilated with the greatest care. Well-paid laborers will prepare and serve their repasts, at regular hours. When the moment arrives for the animal to spin his cocoon, he must have a sort of bower of heather (Fig. 1), or branches of some other kind, properly prepared.

Fig. 1.



Sprigs of Heather arranged so that the Silk-worm may mount into them.

And then, at the last day of its life, we must, with the minutest care and the greatest pains, assure its reproduction." Would you not shrug your shoulders and say, "Who, then, is such a madman as to spend so much care and money to raise—what?—some caterpillars!"

Finally, if your interlocutor should add—"We will gather the cocoons spun by these caterpillars, and then the manufacture which spins them will arise, which will call out all the resources of mechanics. Still another new industry would employ this thread in fabricating stuffs. The value of this thread, of these tissues, would be counted by hundreds of millions for France alone; millions that would benefit agriculture, industry, commerce; the producer and the artisan, the laborer in the fields, and the laborer in towns. Our caterpillar and its products will find a place in the elaborate treatises of statesmen; and a time will come when France will think herself happy that the sovereign of a distant empire, some four thousand leagues away, had been pleased to permit her to buy in his states, and pay very dear for, the eggs of this caterpillar"—you would abruptly turn your back and say, "This man is a fool." And you would not be alone: agriculturists, manufacturers, bankers, and officials, could not find sarcasms enough for this poor dreamer.

And yet it is the dreamer who is in the right. He has not traced a picture of fancy. The caterpillar exists, and I do not exaggerate the importance of this humble insect, which plays a part so superior to what seemed to have fallen to it. It is this of which I wish to give you the history.

Let us first rapidly observe this animal, within and without. We call it a silk-worm, but I have told you it was a caterpillar. (Fig. 7.) I add that it has nothing marked in its appearance. It is larger than the caterpillars that habitually prey upon our fruit-trees, but smaller than the magnificent pearl-blue caterpillar so easy to find in the potato-field. Like all caterpillars, it is is transformed into a butterfly. To know the history of this species is to know the history of all others.

Here in these bottles are some adult silk-worms, but here also are some large pictures, where you will more easily follow the details that I shall point out, beginning with the exterior.

At one of the extremities of its long, almost cylindrical body (Fig. 7), we find the small head, provided with two jaws. These jaws do not move up and down, as in man and most animals that surround us, but laterally. All insects present the same arrangement.

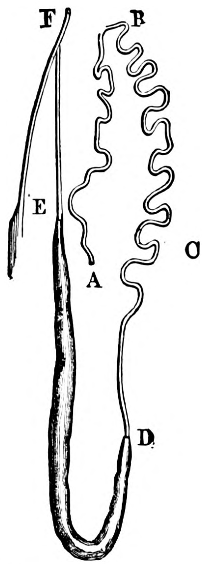
The body is divided into rings, and you see some little black points placed on the side of each of these rings; these are the orifices of respiration. The air enters by these openings, and penetrates the canals that we shall presently find.

The silk-worm has ten pairs of feet. The three first pairs are called the *true feet*, or *scaly feet;* the five last, placed behind, are the *false feet*, or the *membranous feet.* These are destined to disappear at length.

Let us pass to the interior of the body. Here we find, at first, the *digestive tube*, which extends from one extremity to the other. It commences at the *œsophagus*, that which you call the throat. Below you remark an enormous cylindrical sac; it is the *stomach*, which is followed by the very short *intestine.* These canals, slender and tortuous, placed on the side, represent, at the same time, the liver and kidneys. This great yellow cord is the very important organ in which is secreted the silky material (Fig. 2). In proportion as the animal grows, this organ is filled with a liquid which, in passing through the spinners, the orifice of which you see, dries in the air, and forms a thread. This thread constitutes the silk.

The *nervous system* of the animal, placed below the digestive tube, is with insects, as with all animals, of the highest importance. It is the nervous system which seems to animate all the other organs, and particularly the *muscles.* The latter are what we call flesh or meat. They are in reality the organs of movement, with our caterpillar as with man himself. Each of them is formed of elementary fibres that have the property of contracting and relaxing; that is to say, of shortening and lengthening under the influence of the will and of the nervous system. Upon this property depend all the movements executed by any animal whatever.

Fig. 2.



Silk-secreting Apparatus of One Side of a Silk-worm. A, B, C, the part nearest the tail of the worm, where the silk-matter is formed. D, E, enlarged portion—reservoir of silky matter. E, F, capillary tubes proceeding from the two glands, and uniting in one single short canal, F, which opens in the mouth of the worm, at its under lip. Two silk threads are therefore united together, and come out through the orifice with the appearance of a single thread.

I wish you to remark, *à propos* of the caterpillar—of this insect that when crushed seems to be only a formless pulp—that its muscular system is admirably organized. It is superior to that of man himself, at least, in relation to the multiplicity of organs. We count in man 529 muscles; the caterpillar has 1,647, without counting those of the feet and head, which give 1,118 more.

In us, as in most animals, there exists a nourishing liquid *par excellence* that we know under the name of *blood.* This liquid, set in motion by a *heart*, is carried into all parts of the body by *arteries*, and comes back to the heart by *veins*. In making this circuit it finds on its route the lungs filled with air by means of respiration.

In our caterpillar we also find blood and a species of heart, but it has neither arteries nor veins. The blood is diffused throughout the body and bathes the organs in all directions. However, it ought to respire. Here step in the openings of which I have spoken. They lead to a system of ramified canals, of which the last divisions penetrate everywhere, and carry everywhere the air—that fluid essential to the existence of all living beings. In our bodies the air and blood are brought together. In insects the air seeks the blood in all parts of the body.

I have sketched for you a caterpillar when it is full grown. But you well know that living beings are not born in this state. The general law is, small at birth, growth, and death. The caterpillar passes through all these phases.

FIG. 3.

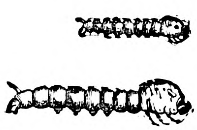
EGG AND FIRST AGE, lasting five days. (An age is the interval between two moultings.)

FIG. 4.

SECOND AGE, lasting six days.

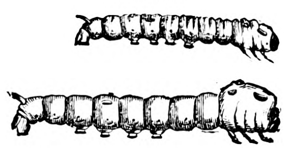
I pass around among you some samples of what we call *seeds of the silk-worm.* These so-called seeds are in reality eggs. The caterpillar comes out of the egg very small; its length at birth is about one-twentieth of an inch. Look at these samples, and you will see how

FIG. 5.



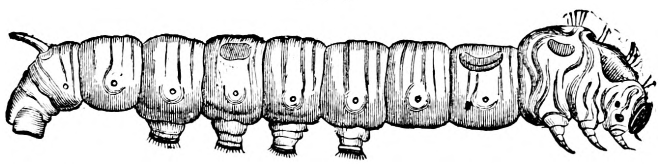
THIRD AGE, lasting six days.

FIG. 6.



FOURTH AGE, lasting six days.

FIG. 7.



FIFTH AGE, lasting nine days. The mature worm near the end of its career, and at the time of its greatest voracity.

great is the difference of size between the worm at birth and the full-grown specimens I have shown you. This difference is much greater than in man. A man weighs about forty times as much as the new-born infant; the caterpillar, when perfectly developed, is 72,000 times heavier than when it first came from the egg.

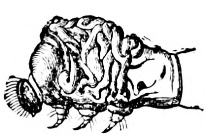
In every thing that relates to the body, there is between men and animals more resemblance than is ordinarily believed. We also come from an egg which essentially resembles all others. That this egg may become a man, it must undergo very great changes, many metamorphoses. But all these changes, all these metamorphoses occur in the bosom of the mother, as they are accomplished within the shell for the chicken. For insects in general, and consequently for the silk-worm, a part of these metamorphoses occur in the open day. Hence they have drawn the attention, excited the curiosity, and provoked for a long time the study of naturalists. Let us say a few words about them.

Scarcely is the caterpillar born than it begins to eat. It has no time to lose in gaining a volume 72,000 times greater than it had at first; so it acquits itself conscientiously of its task, and does nothing but eat, digest, and sleep. At the end of some days this devouring appetite ceases; the little worm becomes almost motionless, hangs itself by the hind-feet, raising and holding a little inclined the anterior of its body.

This repose lasts 24, 36, and even 48 hours, according to the temperature; then the dried-up skin splits open behind the head, and soon along the length of the body. The caterpillar comes out with a new skin, which is formed during this species of sleep.

This singular crisis, during which the animal changes his skin as we change our shirt, is called *moulting*, when it is a question of caterpillars in general. For the silk-worm, we designate it under the name of *sickness*. It is, in fact, for the silk-worm, a grave period, during which it often succumbs, if its health is not perfect.

Fig. 8.



Head of Silk-worm during Moulting; swollen, and skin wrinkled.

Fig. 9.

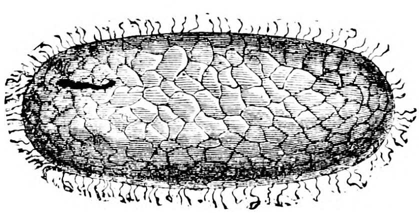
Position of Silk-worm while Moulting.—It remains at rest for from 12 to 24 hours, fasting, but begins to eat an hour after the crisis in which it escapes from the old skin.

The silk-worms change their skin four times. After the fourth moulting comes a redoubled appetite, which permits them to attain their full size in a few days. Then other phenomena appear. The caterpillar ceases to eat, and empties itself entirely; it seems uneasy, wanders here and there, and seeks to climb. Warned by these symptoms, the breeder constructs for it with branches a cradle or bower, into which it mounts. It chooses a convenient place, hangs itself by the hind feet, and soon, through the spinner of which I have spoken (Fig. 2), we see come out a thread of silk. This is at first cast out in any direction, and forms a collection of cords destined to fix the cocoon that is to be spun. Soon the work becomes regular, and the form of the cocoon is outlined. For some hours we can see the worker performing his task across the transparent gauze with which he surrounds himself. By little and little, this gauze thickens, and grows opaque and firm; finally it becomes a cocoon like these I place before you. At the end of about 72 hours the work is done.

Once it has given out its first bit of silk, a worm in good health never stops, and the thread continues without interruption from one end to the other. You see that the cocoon is in reality a ball wound from the outside inward. The thread which forms this ball is 11 miles in length; its thickness is only $\frac{1}{2400}$ of an inch. It is so light that 28 miles of it weigh only $15\frac{1}{3}$ grains. So that $2\frac{1}{5}$ lbs. of silk is more than 2,700 miles long.

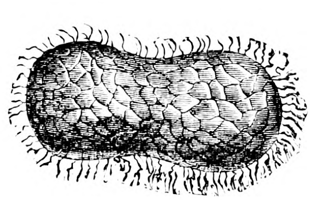
Let me insist a moment on the prodigious activity of the silk-worm while weaving his cocoon. To dispose of its silk when spinning, it moves its head in all directions, and each movement is about one-sixth of an inch. As we know the length of the thread, we can calculate how many movements are made in disposing of the silk in 72 hours. We find in this way that a silk-worm makes nearly 300,000 motions in 24 hours, or 4,166 an hour, or 69 per minute. You see that our insect yields not in activity to any weaver; but we must add that it is beaten by the marvellous machines that the industry of our day has produced.

Fig. 10.



Spherical Cocoon of Bombyx Mori.

Fig. 11.



Cocoon drawn in toward the Middle.

All cocoons are not alike. There exist, in fact, different races of silk-worms, as we have different races of dogs. These differences are less obvious in the animals themselves; they are best seen in the cocoons, which may be either white, yellow, green, or gray; some are round, others oval or depressed in the middle (Figs. 10 and 11). The silk of one is very fine and very strong, that of others is coarse and easily broken. Hence their very different values.

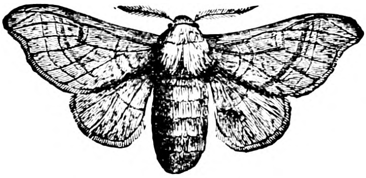
All I have said applies to the silk-worm properly so called—to the silk-worm which feeds on the leaves of the mulberry-tree, the *Bombyx mori* of naturalists. But, some years since, there were introduced into France new species of caterpillars that produce cocoons, and that live upon other leaves than the mulberry. Among these new importations, the two principal ones are the yama-maï worm, which comes from Japan, and feeds upon the leaves of the oak, and the ailanthus worm. The first gives a very beautiful and very fine silk, while that of the second is dull and coarse. But the ailanthus grows very well in unproductive soils, and hence the caterpillar which it nourishes renders an important service.

But let us return to our mulberry caterpillar, or the silk-worm properly so called. We left it at the moment when it disappeared from our eyes enveloped in its cocoon. There, in its mysterious retreat, it becomes torpid once more. It now shortens itself, changes form, and submits to a fifth moulting. But the animal which emerges from the old skin is no longer a caterpillar. It is in some sort a new being; it is what we call a *chrysalis.* This chrysalis scarcely reminds us of the silk-worm. The body is entirely swaddled; we no longer see either head or feet (Fig. 14). The color is changed, and has become a golden yellow. Only by certain obscure movements of the posterior part do we know that it is not a dead body.

This apparent torpor in reality conceals a strange activity in all the organs and all the tissues, which ends in the transformation of the entire being.

In fifteen or seventeen days, according to the temperature, this work is accomplished, and the last crisis arrives. The skin splits on the back; the animal moults for the last time, but the creature that now appears is no longer a caterpillar or a chrysalis: it is a butterfly (Fig. 12).

FIG. 12.



SILK-WORM MOTH (Male).

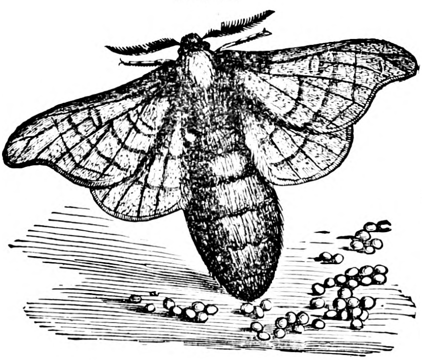
Is it needful to explain the details of this wonderful metamorphosis? The body, before almost all alike, presents now three distinct regions: the *head*, the chest (*thorax*), the belly (*abdomen*). Wings, of which there was not the least vestige, are now developed. In compensation, the hind-feet have disappeared. The fore-feet persist, but you would not know them, they have become so slender, and a fine down covers all the parts.

In the interior, the transformation is also complete. The œsophagus (throat) is no longer a simple reversed funnel; it is a narrow, lengthened tube, with an aërial vessel attached, of which the caterpillar offers no trace. The stomach is strangely shortened. The intestine is elongated, and its different parts, that we found so difficult to distinguish, are very much changed. If we examine in detail all the organs just now indicated, even to the nervous system, we shall find modifications not less striking.

But these are not the strangest changes that have occurred. There are others which still more arrest our attention; they are those which relate to the production of a new generation.

All caterpillars are *neuters*—that is to say, there are no males or females among them. They have no apparatus of reproduction. These organs are developed during the period that follows the formation of the chrysalis while the animal is motionless, and seemingly dead. Marriages occur at the coming out from the cocoon, and, immediately after, the female lays her eggs, averaging about 500 (Fig. 13). This

FIG. 13.



SILK-WORM MOTH (Female).

done, she dies, the male ordinarily dying first. It is a general law for insects; the butterfly of the silk-worm does not escape it. It is even more rigorous for him than for his brethren that we see flying from flower to flower. From the moment of entering the cocoon, the silk-worm takes no nourishment. When it becomes a butterfly, and has assured the perpetuity of the species, its task is accomplished; there is nothing more but to die.

Such, briefly, is the natural history of the silk-worm. It remains to trace rapidly its industrial history.

Whence came this insect? What is its country and that of the mulberry—for the tree and the animal seem to have always travelled side by side? Every thing seems to indicate that China—Northern China—is its point of departure. Chinese annals establish the existence of industries connected with it from those remote and semi-fabulous times when the emperors of the Celestial Empire had, it is said, the head of a tiger, the body of a dragon, and the horns of cattle. They attribute to the Emperor Fo-Hi, 3,400 years before our era, the merit of employing silk in a musical instrument of his own invention. This date carries us back 5,265 years. They are said to have employed the silk of wild caterpillars, and to have spun a sort of floss. At that time they knew nothing of raising the worm or of winding the cocoon into skeins.

This double industry appears to have arisen 2,650 years before our era, or 4,515 years ago, through the efforts of an empress named Si-ling-Chi. To her is attributed the invention of silk stuffs. You will not be surprised to see that the fabrication of silks should have a woman as its inventor.

Si-ling-Chi, in creating this industry, which was to be so immensely developed, enriched her country. Her countrymen seem to have understood the extent of the benefit, and to have been not ungrateful. They placed her among their deities, under the name of Sein-Thsan, two words that, according to M. Stanislas Julien, signify the first who raised the silk-worm. And still, in our time, the empresses of China, with their maids-of-honor, on an appointed day, offer solemn sacrifices to Sien-Thsan. They lay aside their brilliant dress, renounce their sewing, their embroidery, and their habitual work, and devote themselves to raising the silk-worm. In their sphere they imitate the Emperor of China, who, on his part, descends once a year from his throne to trace a furrow with the plough.

The Chinese are an eminently practical race. No sooner did they understand that silk would be to them a source of wealth, than they strove to obtain a monopoly of it. They established guards along their frontier—true custom-house officers—with orders to prevent the going out of seeds of the mulberry or of the silk-worm. Death was pronounced against him who attempted to transport from the country these precious elements which enriched the empire. So, during more than twenty centuries, we were completely ignorant of the source of these marvellous goods—the brilliant tissues manufactured from silk. For a long time we believed them to be a sort of cotton; some supposed even that they were gathered in the fields, and were the webs of certain gigantic spiders. The price of silk continued so high that the Emperor Aurelian, after his victories in the Orient, refused his wife a silken robe, as being an object of immoderate luxury, even for a Roman empress.

A monopoly founded on a secret ought necessarily to come to an end, particularly when the secret is known by several millions of men. But, to export the industry of Si-ling-Chi, it was needful to risk life in deceiving the custom-house officer. It was a woman who undertook this fine contraband stroke. Toward the year 140 before our era, a princess of the dynasty of Han, affianced to a King of Khokan, learned that the country in which she was destined to live had neither the mulberry nor the silk-worm. To renounce the worship of Sein-Thsan, and doubtless also to do without the beautiful stuffs, so dear to the coquette, appeared to her impossible. So she did not hesitate to use the privileges of her rank to violate the laws of the empire. On approaching the frontier, the princess concealed in her hair some mulberry-seed and eggs of the butterfly. The guards dared not put their hands on the head of a "Princess of Heaven;" eggs and seeds passed the officer without disturbance, and prospered well in Khokan, situated near the middle of Asia.

And so commenced that journey which was not to be arrested till the entire world possessed the mulberry and the silk-worm; but it was accomplished slowly and with long halts. That which had occurred in China occurred everywhere, each new state that obtained the precious seeds attempting prohibition.

The silk-worm and mulberry got to Europe in 552, under Justinian. At this time two monks of the order of St. Basil delivered to this emperor the seeds, said to have come from the heart of Asia. To smuggle them, they had taken still greater precautions than the Chinese princess, for they hollowed out their walking-sticks, and filled the interior with the precious material. The Emperor Justinian did not imitate the Asiatic potentates, but sought to propagate and extend the silk-manufacture. Morea, Sicily, and Italy, were the first European countries that accepted and cultivated the new products.

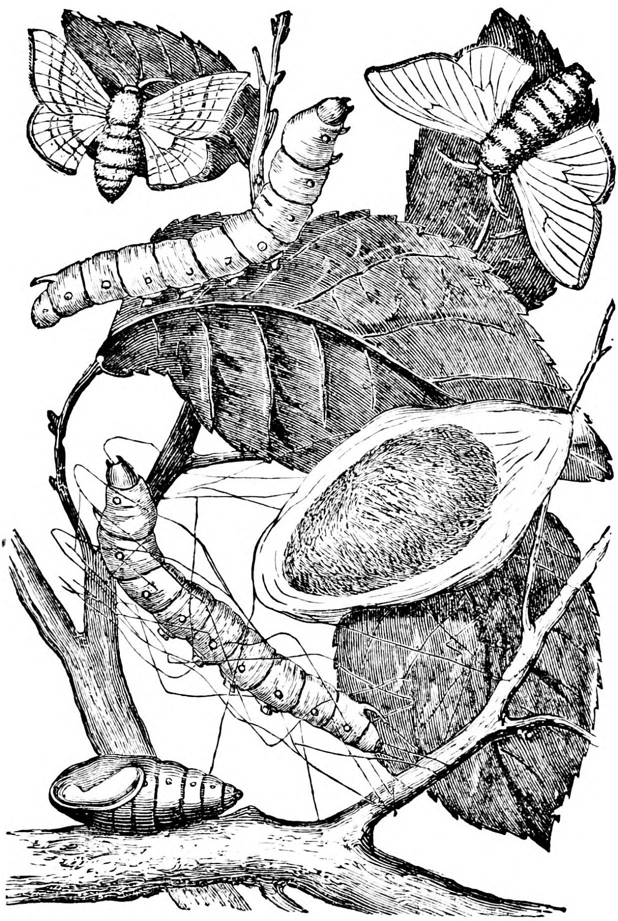
It was not till the twelfth or thirteenth century that the silk-worm penetrated into France. Louis XI. planted mulberry-trees around his Château of Plessis-les-Tours. Besides, he called a Calabrian named Francis to initiate the neighboring population in raising this precious insect, and developing the several industries that are connected with it. Under Henry IV., sericulture received a great impulse, thanks chiefly, perhaps, to a simple gardener of Nîmes named François Traucat. It is always said that this nurseryman distributed throughout the neighboring country more than four million mulberry-sprouts. In enriching the country, Traucat acquired a considerable fortune; but he lost it foolishly. He had heard of treasures buried near a great castle which commanded the town of Nîmes, and which is called the Castle of Magne. He wished to increase the money he had nobly and usefully gained, by this imaginary gold; he bought the great castle and neighboring ground, and dug the earth, which brought him nothing, till he ruined himself.

The minister of Louis XIV., Colbert, sought also to propagate the mulberry. Sully with reluctance had done the same, and sent trees to various parts of the kingdom, some of which were still living when I was a child. They were called by the name of this minister, and I remember to have seen two of them in my father's grounds, which no longer bore leaves, but were piously preserved as *souvenirs* of their origin.

To lead in the development of sericulture, a man was needed who would not hesitate to set an example, and to make considerable sacrifices. This man, I am proud to say, was a modest officer, Captain

François de Carles, my grandfather. Returning from a campaign in Italy, where he had seen how much the culture of the mulberry enriched the population, he resolved to transplant this industry into the heart of Cévennes, where were his estates. He proceeded in this way: He made plantations, and, in order to extend them, he did not hesitate to uproot the chestnuts, those old nourishers of the ancient Cévennols.

Fig. 14.



Larva, Pupa, Cocoon, and Moth, of Silk-worm.

To water the mulberries, he constructed ditches and aqueducts; then he forced, so to say, the peasants to take these improved lands at their own price and on their own conditions. In this way he alienated almost all his land, and singularly diminished his fortune; but he enriched the country. The results speak too distinctly to be misunderstood. You shall judge by the figures.

The little valley where Captain Carles made his experiments, and where I was born, belongs to the Commune of Valleraugue. At the time of which I speak, they harvested scarcely 4,400 lbs. of very poor cocoons, that sold for very little. Recently there were produced, before the malady of which I shall presently speak, 440,000 lbs. of excellent quality, valued on an average at $2\frac{1}{3}$ or $2\frac{1}{2}$ francs per pound. At this price, a million of silver money found its way each year into this little commune of not more than 4,000 inhabitants.

Let me remark that this money went not alone to the rich. The small proprietors, the day-laborers, those even who owned not the least land, had the greatest part. In fact, most of the easy proprietors did not raise their own silk-worms; they contracted for them in this way: The laborer received a certain quantity of eggs of the silk-worm on the condition of giving a fifth of the cocoons for an ounce of eggs; they received, besides, enough mulberry-leaves to nourish all the worms from these eggs, *plus* a certain quantity to boot. All the cocoons above this constituted the wages or gain of the raiser.

You see, we had resolved in our mountains this problem, so often encountered and still unsettled, of the association of capital and labor; and resolved it in the best possible way for both. The interest of the proprietor was, in this case, identical with that of the rearer, and reciprocally; for the success of a good workman would equally benefit both parties, and the poor workman could profit only according to his work.

Now, this labor was in reality of little account. Until after the fourth moulting, when the silk-worm is preparing to make his cocoon, the rearing of the worms can be performed by the women and children while the father pursues his ordinary occupation. Only after the fourth moult is he obliged to interrupt his work, and occupy himself, in his turn, in the gathering of leaves. The rearing ended, an industrious family—and such are not rare with us—will have, on an average, from 250 to 500 francs of profit. This bright silver, added to the resources of the year, this profit obtained without the investment of capital, seconded by the wise conduct of our mountaineer Cévennols, leads rapidly to competency. At the end of a few years, the laborer, who had nothing, possesses a little capital to buy some corner of rock, which, by his intelligent industry, he quickly transforms into fertile soil, and in his turn becomes a proprietor.

What I am telling you is not fancy. I speak of facts that have occurred under my own eyes, and that I well know. In the country, and particularly on the soil of our old mountains, people are not strangers to each other, as in our great cities. Between the gentleman and the peasant there are not the same barriers as between the citizen and the laborer in towns. When a child, I played with all my little neighbors; I knew the most secret nooks of the eight or ten houses composing the modest hamlet which bordered the place where

I was born; I saluted by their names the members of all the families of the valley. And now, when I go to the country, it is always a great pleasure to visit these houses, one by one, and take by the hand those from whom I have been so long separated. But this happiness is always mingled with sorrow; the number of those I knew diminishes with each visit, and those who have come since cannot replace them for me.

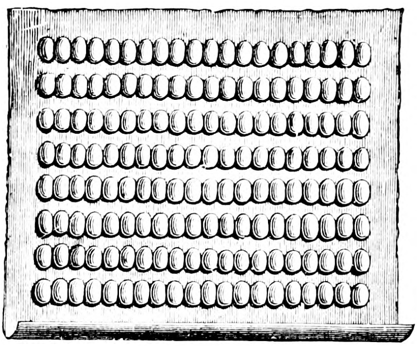
Permit me to give you the history of one of these families. It occurs to me first, as it contrasted with all the others by its miserable dwelling. This was a little thatch-built cottage, standing by itself at the foot of an irregular slope of perfectly bare rocks. It consisted of a single story, with only one room, scarcely larger than one of our bedrooms; the wall, built without mortar, was any thing but regular; the roof consisted of flags of stone, retaining, as well as they were able, a mass of straw and branches. Between the rocks that supported this house and the wall, there was a little place where was kept a pig, the ordinary resource of all Cévennol house-keeping.

This cottage was occupied, when I was eleven or twelve years old, by a man with his wife and four children. The father and mother worked in the field; the eldest child, scarcely of my age, had begun to be useful, particularly in the time of gathering the mulberry-leaves; the smaller ones drove the pig along the road, where it grew and fattened, the best it could, without any expense.

After an absence of ten years, I returned to my mountains, and the first thing was to call upon my old neighbors, those of whom I have spoken among the rest. In approaching, I scarcely knew the place. The rocks that supported the house had disappeared to make way for those *traversiers* of which I shall tell you presently; the house had been rebuilt, it had gained a story, and was of double its former extent; its walls were laid in mortar; its roof covered with beautiful slate. The master of the house was absent, but his wife welcomed me with a glass of wine from a neat walnut table. Then she showed me, with proper pride, a room with two beds at the farther end, the first portion being devoted to the rearing of silk-worms; and, above all, the favorite article of furniture of all good Cévennol housekeeping—an immense cupboard of walnut, crammed with clothing, dresses, and raiment of all sorts. At the same time she gave me news of all the family: the eldest son was a soldier; a daughter was married; the eldest remaining children attended to the business, and, as of old, the younger ones ran about watching the pig. I clasped with pleasure the hand of this brave woman, because this competence was the fruit of good conduct, of industry, of perseverance, and of economy. And what the silk-worm did in ten years for one family it has been doing for nearly a century for the whole region of Cévennes, because among them you generally find the same elements of success.

That you may better understand me, I wish to give you some idea of these valleys. Let me sketch for you the one I know best, the one in which I was born. It is composed of ascents so steep that, when two neighboring houses are placed one above the other, the cellar of the upper one is on the same level as the garret of the lower one. There is not much earth on these declivities, and the rocks stick out everywhere. But it is, as it were, from the rocks themselves that our mountaineers make their mulberry-plantations. They proceed in this way: They first break up the rocks, and with the larger

Fig. 15.



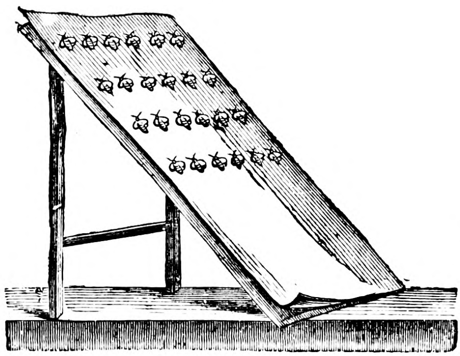
Sheets of Paper, with Rows of Cocoons prepared for the Exit of the Moths designed for laying Eggs.

stones so obtained they raise a wall; then, with the smaller pieces, they fill up the interval between the wall and the mountain. This done, they bring upon their backs, from the bottom of the valley, soil and manure enough entirely to fill the space. This is what is called a *traversier*, and it is in this soil that most of the mulberry-trees are planted. I have seen a bridge built across a mountain-stream expressly to give foothold for two or three of these precious trees. To pay for all this preparation the produce should be very great. The following figures give the average value of ground planted to mulberries for 20 years:

Traversiers not watered . . .	1 acre,	9,800 francs.
Fields watered	1 acre,	12,000 "
Meadows planted with mulberries .	1 acre,	12,400 "

and even then the money yielded five per cent. This price, which some would not believe when I told them, has been officially confirmed by M. de Lavergne, in his remarkable writings upon French agriculture. This value of land, and the way it has been obtained, explain the nature of our country's wealth. With the exception of some families recently enriched by the silk-manufacture and the silk-trade, the level of this wealth, although very high, is more of the nature of general competence than of great fortunes. Industry and economy have produced general well-being, without the growth of offensive differences. I cannot say how it is now, but in my childhood there were no paupers in our commune, except two infirm people who were supported in their misfortunes by voluntary aid.

Fig. 16.



Sheets of Paper stuck into Screens, and inclined for the Reception of Moths.

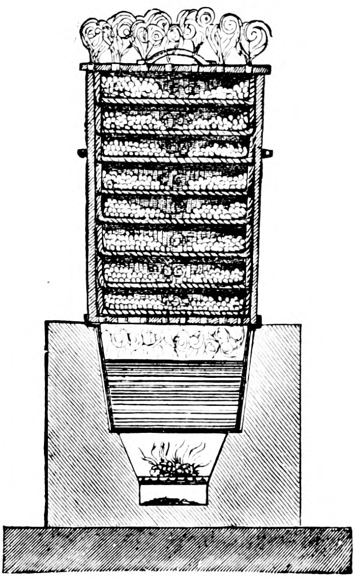
These striking results could not fail to affect the neighboring country. This example of the culture of the mulberry was imitated throughout the south of France, and adopted more or less in other departments. You can judge of the progress made in this culture by the following figures, giving the quantity of cocoons produced annually:

From 1821 to 1830	22,000,000 pounds.
" 1831 " 1840	31,000,000 "
" 1841 " 1845	37,000,000 "
" 1846 " 1852	46,000,000 "
" 1853	56,000,000 "

These 56,000,000 lbs. of cocoons sold at from 2⅓ to 2½ francs per lb., representing a value of about 130,000,000 francs. Now, these millions all went to agriculture, to the first producer; and so they added to the national wealth at its most vital source. If this progress had continued, in a few years we should have been able to supply our own manufactures, and relieve ourselves of the tribute of 60 or 65,000,000 francs that we pay to foreign countries. But, unhappily, at the moment when this culture was most prosperous, when mulberry-plantations were springing up on all sides, fed by the nurseries which were each day more numerous, all this prosperity disappeared before the terrible scourge to which I alluded in the beginning of my discourse.

Like all our domestic animals, the silk-worm is subject to various maladies. One, called the *muscardine*, that for a long time was the terror of breeders, is caused by a species of mould or microscopic mushroom. This mushroom invades the interior of the body of the insect. After affecting all the tissues, this vegetal parasite sometimes suddenly appears upon the outside of the body in the form of a white powder. Each grain of this powder, falling upon a silk-worm, plants the seed of this formidable mushroom, the ravages of which will destroy all the worms of a rearing-chamber in a few hours. Happily, science has found the means of killing these seeds, and of completely disinfecting the locality. At the very moment when this victory was announced, another yet more terrible scourge, the *pébrine*, appeared. The muscardine caused isolated disaster; it had never been so widespread as seriously to injure the general business. Not so this other

FIG. 17.



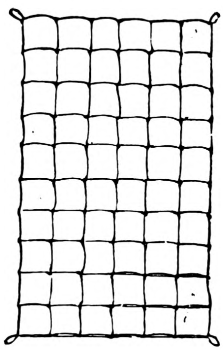
APPARATUS FOR STIFLING THE CHRYSALIS IN THE COCOONS.

malady. It is a true epidemic, which attacks life at its very source in an inexplicable fashion. It is a pestilence like the cholera. Under the influence of this scourge, the chambers of the silk-worm no longer thrive; most of the worms die without producing silk. Those that survive as butterflies give infected eggs, and the next generation is worse than the first. To get healthy eggs, we had to go to the neighboring countries; but other countries have been invaded in their turn. To-day we have to get them in Japan. Even when the egg is healthy, the epidemic bears equally on its product; a great part of the worms always succumb, and when the breeder gets half a crop he is very happy. Upon the whole, the great majority of breeders have worked at a loss since the invasion of this disease.

You understand the consequences of such a state of things, continued since 1849. The people make nothing; they lose, and yet they have to live and cultivate their ground. In this business the profits melt away rapidly, and particularly where the mulberry was the only crop, as at Cévennes, misery has taken the place of comfort. Those who once called themselves rich are to-day scarcely able to get food to eat. Those who used to hire day-laborers to gather their harvest have become day-laborers, and the laborers of former times have emigrated. This will give you an idea of the extremities to which they are reduced, for to uproot a mountaineer of Cévennes he must be dying of hunger.

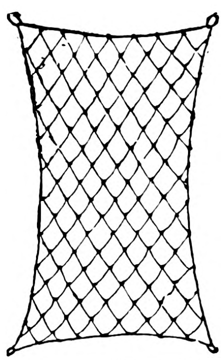
To escape a fatality so heavy, these people have displayed perseverance and courage of the highest kind. They have undertaken distant journeys to get non-infected eggs. More than one has not come back from these journeys, where it was needful to struggle against great fatigue in inhospitable countries. Although they fell not on a field of battle, struck by ball or bullet, they were true soldiers; and, although they did not carry arms, they died in the service of the country.

Fig. 18.



Square Net.

Fig. 19.



Lozenge-shaped Net.

Nets used to separate the worms from their faded and withered leaves. Fresh leaves are spread on these nets, and the worms leave the old food to get on to the new leaves.

During seventeen years this exhaustion has been most aggravated in places chiefly devoted to sericulture. But, if these local sufferings merit all our sympathy, their general consequences still more demand our attention. Confidence in the culture of the silk-worm has diminished wherever it was not the exclusive occupation. Where other crops could replace it, that of the mulberry was easily discouraged. In many countries they have destroyed the tree so lately known as the tree of gold.

As the foregoing interesting discourse was delivered in 1866, the following statement of Prof. Huxley regarding the *pébrine* malady, made in 1870, in his address before the British Association, will be interesting.—[Editor.

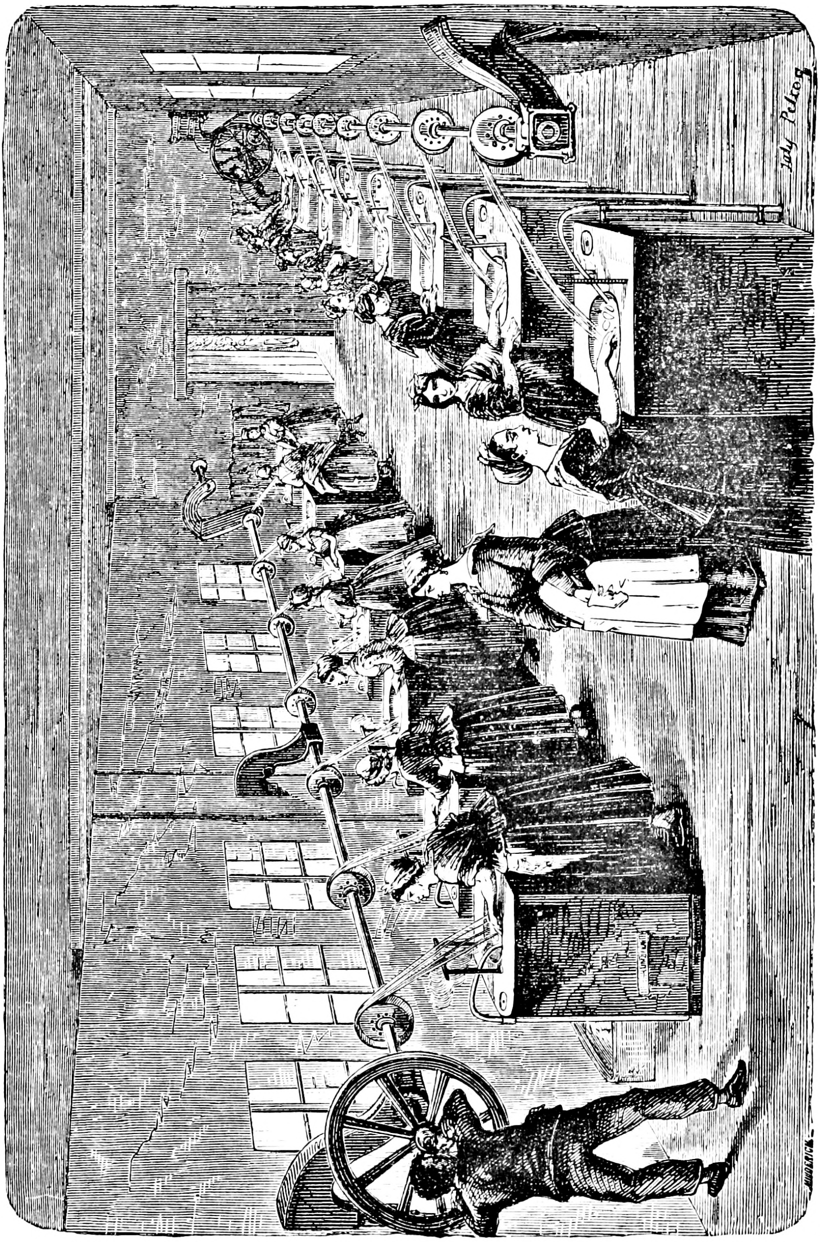


Fig. 20.—Silk-winding Establishment.—Each woman has a copper containing water that she heats by steam, and plunges the cocoons into this hot water to soften the gummy matter that sticks the threads together. She then beats them with a light birch broom to catch hold of the filaments, which she seizes with her fingers and joins into threads, which are drawn out on revolving wheels.

"The Italian naturalist, Filippi, discovered, in the blood of silk-worms affected by this strange disease, *pébrine*, a multitude of cylindrical corpuscles, each of about $\frac{1}{6000}$ of an inch long. These have been carefully studied by Lebert, and named by him *Panhistophyton;* for the reason that, in subjects in which the disease is strongly developed, the corpuscles swarm in every tissue and organ of the body, and even pass into the undeveloped eggs of the female moth. The French Government, alarmed by the continued ravages of the malady and the inefficiency of the remedies which had been suggested, dispatched M. Pasteur to study it, and the question has received its final settlement. It is now certain that this devastating, cholera-like *pébrine* is the effect of the growth and multiplication of the *Panhistophyton* in the silk-worm. It is contagious and infectious, because the corpuscles of the *Panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silk-worms in their neighborhood; it is hereditary, because the corpuscles enter into the egg. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the *pébrine*, but has received its explanation from the fact that the disease is the result of the presence of the microscopic organism *Panhistophyton.* M. Pasteur has devised a method of extirpating the disease, which has proved to be completely successful when properly carried out."

MENTAL SCIENCE AND SOCIOLOGY.

By HERBERT SPENCER.

PROBABLY astonishment would make the reporters drop their pencils, were any member of Parliament to enunciate a psychological principle as justifying his opposition to a proposed measure. That some law of association of ideas, or some trait in emotional development, should be deliberately set forth as a sufficient ground for saying "ay" or "no" to a motion for second reading, would doubtless be too much for the gravity of legislators. And along with laughter from many there would come from a few cries of "question:" the entire irrelevancy to the matter in hand being conspicuous. It is true that during debates the possible behavior of citizens under the suggested arrangements is described. Evasions of this or that provision, difficulties in carrying it out, probabilities of resistance, connivance, corruption, etc., are urged; and these tacitly assert that the mind of man has certain characters, and under the conditions named is likely to act in certain ways. In other words, there is an implied recognition of the truth that the effects of a law will depend on the manner in which human intelligence and human feeling are influenced by it. Experiences of men's conduct which the legislator has gathered, and which lie partially sorted in his memory, furnish him with empirical notions that guide his judgment on each question raised; and he would think it folly to ignore all this unsystematized knowledge about people's characters and actions. But, at the same time, he regards as foolish the proposal to proceed, not on vaguely-generalized facts, but on facts accurately generalized; and, as still more foolish, the proposal to merge these minor definite generalizations in generalizations expressing the ultimate laws of Mind. Guidance by intuition seems to him much more rational.

Of course, I do not mean to say that his intuition is of small value. How should I say this, remembering the immense accumulation of experiences by which his thoughts have been moulded into harmony with things? We all know that when the successful man of business is urged by wife and daughters to get into Parliament, that they may attain a higher social standing, he always replies that his occupations through life have left him no leisure to prepare himself, by collecting and digesting the voluminous evidence respecting the effects of institutions and policies, and that he fears he might do mischief. If the heir to some large estate, or scion of a noble house powerful in the locality, receives a deputation asking him to stand for the county, we constantly read that he pleads inadequate knowledge as a reason for declining: perhaps hinting that, after ten years spent in the needful studies, he may have courage to undertake the heavy responsibilities proposed to him. So, too, we have the familiar fact that, when, at length, men who have gathered vast stores of political information gain the confidence of voters who know how carefully they have thus fitted themselves, it still perpetually happens that after election they find they have entered on their work prematurely. It is true that beforehand they had sought anxiously through the records of the past, that they might avoid legislative errors of multitudinous kinds, like those committed in early times. Nevertheless, when acts are proposed referring to matters dealt with in past generations by acts long since cancelled or obsolete, immense inquiries open before them. Even limiting themselves to the 1,126 acts repealed in 1823–'29, and the further 770 repealed in 1861, they find that to learn what these aimed at, how they worked, why they failed, and whence arose the mischiefs they wrought, is an arduous task, which yet they feel bound to undertake lest they should reinflict these mischiefs; and hence the reason why so many break down under the effort, and retire with health destroyed. Nay, more—on those with constitutions vigorous enough to carry them through such inquiries, there continually presses the duty of making yet further inquiries. Besides tracing the results of abandoned laws in other societies, there is at home, year by year, more futile law-making to be investigated and lessons to be drawn from it; as, for example, from the 134 public acts passed in 1856–'57, of which all but 68 are wholly or partially repealed. And thus it happens that, as every autumn shows us, even the strongest men, finding their lives during the recess overtaxed with the needful study, are obliged so to locate themselves that by an occasional day's hard riding after the hounds, or a long walk over the moors with gun in hand, they may be enabled to bear the excessive strain on their nervous systems. Of course, therefore, I am not so unreasonable as to deny that judgments, even empirical, which are guided by such carefully-amassed experiences, must be of much worth.

But, fully recognizing the vast amount of information which the legislator has laboriously gathered from the accounts of institutions and laws, past and present, here and elsewhere, and admitting that, before thus instructing himself, he would no more think of enforcing a new law than would a medical student think of plunging an operating-knife into the human body before learning where the arteries ran, the remarkable anomaly here demanding our attention is, that he objects to any thing like analysis of these phenomena he has so diligently collected, and has no faith in conclusions drawn from the *ensemble* of them. Not discriminating very correctly between the word "general" and the word "abstract," and regarding as *abstract* principles what are in nearly all cases *general* principles, he speaks contemptuously of these as belonging to the region of theory, and as not concerning the law-maker. Any wide truth that is insisted upon as being implied in many narrow truths, seems to him remote from reality and unimportant for guidance. The results of recent experiments in legislation he thinks worth attending to; and, if any one reminds him of the experiments he has read so much about, that were made in other times and other places, he regards these also, separately taken, as deserving of consideration. But, if, instead of studying special classes of legislative experiments, some one compares many classes together, generalizes the results, and proposes to be guided by the generalization, he shakes his head skeptically. And his skepticism passes into ridicule if it is proposed to affiliate such generalized results on the laws of Mind. To prescribe for society on the strength of countless unclassified observations, appears to him a sensible course; but, to colligate and systematize the observations so as to educe tendencies of human behavior displayed throughout cases of numerous kinds, to trace these tendencies to their sources in the mental natures of men, and thence to draw conclusions for guidance, appears to him a visionary course.

Let us look at some of the fundamental facts he ignores, and at the results of ignoring them.

Rational legislation, based as it can only be on a true theory of conduct, which is derivable only from a true theory of mind, must recognize as a datum the direct connection of action with feeling. That feeling and action bear a constant ratio, is a statement needing qualification; for at the one extreme there are automatic actions which take place without feeling, and at the other extreme there are feelings so intense that, by deranging the vital functions, they impede or arrest action. But, speaking of those activities which life in general presents, it is a law tacitly recognized by all, though not distinctly formulated, that action and feeling vary together in their amounts. Passivity and absence of facial expression, both implying rest of the muscles, are held to show that there is being experienced neither much sensation nor much emotion, while the degree of external demonstration, be it in movements that rise finally to spasms and contortions, or be it in sounds that end in laughter, and shrieks, and groans, is habitually accepted as a measure of the pleasure or pain, sensational or emotional. And so, too, where continued expenditure of energy is seen, be it in a violent struggle to escape, or be it in the persevering pursuit of an object, the quantity of effort is held to show the quantity of feeling.

This truth, undeniable in its generality, whatever qualifications secondary truths make in it, must be joined with the truth that cognition does *not* produce action. If I tread on a pin, or unawares dip my hand into very hot water, I start: the strong sensation produces motion without any thought intervening. Conversely, the proposition that a pin pricks, or that hot water scalds, leaves me quite unmoved. True, if to one of these propositions is joined the idea that a pin is about to pierce my skin, or to the other the idea that some hot water will fall on it, there results a tendency, more or less decided, to shrink. But that which causes shrinking is the ideal pain. The statement that the pin will hurt or the water scald produces no effect, so long as there is nothing beyond a recognition of its meaning: it produces an effect only when the pain verbally asserted becomes a pain actually conceived as impending—only when there rises in consciousness a representation of the pain, which is a faint form of the pain as before felt. That is to say, the cause of movement here, as in other cases, is a feeling and not a cognition. What we see even in these simplest actions, runs through actions of all degrees of complexity. It is never the knowledge which is the moving agent in conduct, but it is always the feeling which goes along with that knowledge, or is excited by it. Though the drunkard knows that after to-day's debauch will come to-morrow's headache, yet he is not deterred by consciousness of this truth, unless the penalty is distinctly represented—unless there rises in his consciousness a vivid idea of the misery to be borne—unless there is excited in him an adequate amount of feeling antagonistic to his desire for drink. Similarly with improvidence in general. If coming evils are imagined with clearness and the threatened sufferings ideally felt, there is a due check on the tendency to take immediate gratifications without stint; but, in the absence of that consciousness of future ills which is constituted by the ideas of pains, distinct or vague, the passing desire is not opposed effectually. The truth that recklessness brings distress, fully acknowledged though it may be, remains inoperative. The mere cognition does not affect conduct—conduct is affected only when the cognition passes out of that intellectual form in which the idea of distress is little more than verbal, into a form in which this term of the proposition is developed into a vivid imagination of distress—a mass of painful feeling. It is thus with conduct of every kind. See this group of persons clustered at the river-side. A boat has upset, and some one is in danger of drowning. The fact, that, in the absence of aid, the youth in the water will shortly die, is known to them all. That by swimming to his assistance his life may be saved, is a proposition denied by none of them. The duty of helping fellow-creatures who are in difficulties, they have been taught all their lives; and they will severally admit that running a risk to prevent a death is praiseworthy. Nevertheless, though sundry of them can swim, they do nothing beyond shouting for assistance or giving advice. But now here comes one who, tearing off his coat, plunges in to the rescue. In what does he differ from the others? Not in knowledge. Their cognitions are equally clear with his. They know as well as he does that death is impending, and know, too, how it may be prevented. In him, however, these cognitions arouse certain correlative emotions more strongly than they are aroused in the rest. Groups of feelings are excited in all; but, whereas in the others the deterrent feelings of fear, etc., preponderate, in him there is a surplus of the feelings excited by sympathy, joined, it may be, with others not of so high a kind. In each case, however, the behavior is not determined by knowledge, but by emotion. Obviously, change in the actions of these passive spectators is not to be effected by making their cognitions clearer, but by making their higher feelings stronger.

Have we not here, then, a cardinal psychological truth, to which any rational system of human discipline must conform? Is it not manifest that a legislation which ignores it and tacitly assumes its opposite will inevitably fail? Yet much of our legislation does this; and we are at present, legislature and nation together, eagerly pushing forward schemes which proceed on the postulate that conduct is determined not by feelings, but by cognitions.

For what else is the assumption underlying this anxious urging-on of organizations for teaching? What is the root-notion common to Secularists and Denominationalists, but the notion that spread of knowledge is the one thing needful for bettering behavior? Having both swallowed certain statistical fallacies, there has grown up in them the belief that State-education will check ill-doing. In newspapers, they have often met with comparisons between the numbers of criminals who can read and write and the numbers who cannot; and, finding the numbers who cannot greatly exceed the numbers who can, they accept the inference that ignorance is the cause of crime. It does not occur to them to ask whether other statistics, similarly drawn up, would not prove with like conclusiveness that crime is caused by absence of ablutions, or by lack of clean linen, or by bad ventilation, or by want of a separate bedroom. Go through any jail, and ascertain how many prisoners had been in the habit of taking a morning bath, and you would find that criminality habitually went with dirtiness of skin. Count up those who had possessed a second suit of clothes, and a comparison of the figures would show you that but a small percentage of criminals were habitually able to change their garments. Inquire whether they had lived in main streets or down courts, and you would discover that nearly all urban crime comes from holes and corners. Similarly, a fanatical advocate of total abstinence or of sanitary improvement could get equally strong statistical justifications for his belief. But, if, not accepting the random inference presented to you, that ignorance and crime are cause and effect, you consider, as above, whether crime may not with equal reason be ascribed to various other causes, you are led to see that it is really connected with an inferior mode of life, itself usually consequent on original inferiority of nature; and you are led to see that ignorance is simply one of the concomitants, no more to be held the cause of crime than various other concomitants.

But this obvious criticism, and the obvious counter-conclusion it implies, are not simply overlooked, but, when insisted on, seem powerless to affect the belief which has taken possession of men. Disappointment alone will now affect it. A wave of opinion, reaching a certain height, cannot be changed by any evidence or argument, but has to spend itself in the gradual course of things before a reaction of opinion can arise. Otherwise it would be incomprehensible that this confidence in the curative effects of teaching, which men have carelessly allowed to be generated in them by the reiterations of *doctrinaire* politicians, should survive the direct disproofs yielded by daily experience. Is it not the trouble of every mother and every governess, that perpetual insisting on the right and denouncing the wrong do not suffice? Is it not the constant complaint that on many natures reasoning and explanation and the clear demonstration of consequences are scarcely at all operative; that where they are operative there is a more or less marked difference of emotional nature; and that where, having before failed, they begin to succeed, change of feeling rather than difference of apprehension is the cause? Do we not similarly hear from every house-keeper that servants usually pay but little attention to reproofs; that they go on perversely in old habits, regardless of clear evidence of their foolishness; and that their actions are to be altered not by explanations and reasonings, but by either the fear of penalties or the experience of penalties—that is, by the emotions awakened in them? When we turn from domestic life to the life of the outer world, do not like disproofs everywhere meet us? Are not fraudulent bankrupts educated people, and getters-up of bubble-companies, and makers of adulterated goods, and users of false trade-marks, and retailers who have light weights, and owners of unseaworthy ships, and those who cheat insurance-companies, and those who carry on turf-chicaneries, and the great majority of gamblers? Or, to take a more extreme form of turpitude—is there not, among those who have committed murder by poison within our memories, a considerable number of the educated—a number bearing as large a ratio to the educated classes as does the total number of murderers to the total population?

This belief in the moralizing effects of intellectual culture, flatly contradicted by facts, is absurd *a priori.* What imaginable connection is there between the learning that certain clusters of marks on paper stand for certain words and the getting a higher sense of duty? What possible effect can acquirement of facility in making written signs of sounds have in strengthening the desire to do right? How does knowledge of the multiplication-table, or quickness in adding and dividing, so increase the sympathies as to restrain the tendency to trespass against fellow-creatures? In what way can the attainment of accuracy in spelling and parsing, etc., make the sentiment of justice more powerful than it was; or why from stores of geographical information, perseveringly gained, is there likely to come increased regard for truth? The irrelation between such causes and such effects is almost as great as that between exercise of the fingers and strengthening of the legs. One who should by lessons in Latin hope to give a knowledge of geometry, or one who should expect practice in drawing to be followed by expressive rendering of a sonata, would be thought fit for an asylum; and yet he would be scarcely more irrational than are those who by discipline of the intellectual faculties expect to produce better feelings.

This faith in lesson-books and readings is one of the superstitions of the age. Even as appliances to intellectual culture, books are greatly over-estimated. Instead of second-hand knowledge being regarded as of less value than first-hand knowledge, and as a knowledge to be sought only where first-hand knowledge cannot be had, it is actually regarded as of greater value. Something gathered from printed pages is supposed to enter into a course of education; but, if gathered by observation of Life and Nature, is supposed not thus to enter. Reading is seeing by proxy—is learning indirectly through another man's faculties, instead of directly through one's own faculties; and such is the prevailing bias that the indirect learning is thought preferable to the direct learning, and usurps the name of cultivation! We smile when told that savages consider writing as a kind of magic: and we laugh at the story of the negro who hid a letter under a stone, that it might not inform against him when he devoured the fruit he was sent with. Yet the current notions about printed information betray a kindred delusion: a kind of magical efficacy is ascribed to ideas gained through artificial appliances, as compared with ideas otherwise gained. And this delusion, injurious in its effects even on intellectual culture, produces effects still more injurious on moral culture, by generating the assumption that this, too, can be got by reading and the repeating of lessons.

It will, I know, be said that not from intellectual teaching, but from moral teaching, are improvement of conduct and diminution of crime looked for. While, unquestionably, many of those who urge on educational schemes believe in the moralizing effects of knowledge in general, it must be admitted that some hold general knowledge to be inadequate, and contend that rules of right conduct must be taught. Already, however, reasons have been given why the expectations even of these are illusory; proceeding, as they do, on the assumption that the intellectual acceptance of moral precepts will produce conformity to them. Plenty more reasons are forthcoming. I will not dwell on the contradictions to this assumption furnished by the Chinese, to all of whom the high ethical maxims of Confucius are taught, and who yet fail to show us a conduct proportionately exemplary. Nor will I enlarge on the lesson to be derived from the United States, the school-system of which brings up the whole population under the daily influence of chapters which set forth principles of right conduct, and which nevertheless in its political life, and by many of its social occurrences, shows us that conformity to these principles is any thing but complete. It will suffice if I limit myself to evidence supplied by our own society, past and present, which negatives, very decisively, these sanguine expectations. For, what have we been doing all these many centuries by our religious agencies, but preaching right principles to old and young? What has been the aim of services in our ten thousand churches, week after week, but to enforce a code of good conduct by promised rewards and threatened penalties? —the whole population having been for many generations compelled to listen. What have Dissenting chapels, more numerous still, been used for, unless as places where pursuance of right and desistance from wrong have been unceasingly commended to all from childhood upward? And if now it is held that something more must be done—if, notwithstanding perpetual explanations and denunciations and exhortations, the misconduct is so great that society is endangered, why, after all this insistance has failed, is it expected that more insistance will succeed? See here the proposals and the implied beliefs. Teaching by clergymen not having had the desired effect, let us try teaching by school-masters. Bible-reading from a pulpit, with the accompaniment of imposing architecture, painted windows, tombs, and "dim religious light," having proved inadequate, suppose we try bible-reading in rooms with bare walls, relieved only by maps and drawings of animals. Commands and interdicts, uttered by a surpliced priest to minds prepared by chant and organ-peal, not having been obeyed, let us see whether they will be obeyed when mechanically repeated in school-boy sing-song to a threadbare usher, amid the buzz of lesson-learning and clatter of slates. No very hopeful proposals, one would say; proceeding, as they do, upon one or other of the beliefs, that a moral precept will be effective in proportion as it is received without emotional accompaniment, and that its effectiveness will increase in proportion to the number of times it is repeated. Both these beliefs are directly at variance with the results of psychological analysis and of daily experience. Certainly, such influence as may be gained by addressing moral truths to the intellect, is made greater if the accompaniments arouse an appropriate emotional excitement, as a religious service does; while, conversely, there can be no more effectual way of divesting such moral truths of their impressiveness, than associating them with the prosaic and vulgarizing sounds and sights and smells coming from crowded children. And no less certain is it that precepts, often heard and little regarded, lose by repetition the small influence they had. What do public-schools show us?—are the boys rendered merciful to one another by listening to religious injunctions every morning? What do universities show us?—have perpetual chapels habitually made undergraduates behave better than the average of young men? What do cathedral-towns show us?—is there in them a moral tone above that of other towns, or must we from the common saying, "the nearer the church," etc., infer a pervading impression to the contrary? What do clergymen's sons show us?—has constant insistance on right conduct made them conspicuously superior, or do we not rather hear it whispered that something like an opposite effect seems produced. Or, to take one more case, what do religious newspapers show us?—is it that the precepts of Christianity, more familiar to their writers than to other writers, are more clearly to be traced in their articles, or has there not ever been displayed a want of charity in their dealings with opponents, and is it not still displayed? Nowhere do we find that repetition of rules of right, already known but disregarded, produces regard for them; but we find that, contrariwise, it makes the regard for them less than before.

The prevailing assumption is, indeed, as much disproved by analysis as it is contradicted by familiar facts. Already we have seen that the connection is between action and feeling; and hence the corollary, that only by a frequent passing of feeling into action is the tendency to such action strengthened. Just as two ideas often repeated in a certain order become coherent in that order; and just as muscular motions, at first difficult to combine properly with one another and with guiding perceptions, become by practice facile, and at length automatic; so the recurring production of any conduct by its prompting emotion makes that conduct relatively easy. Not by precept, though heard daily; not by example, unless it is followed; but only by action, often caused by the related feeling, can a moral habit be formed. And yet this truth, which Mental Science clearly teaches, and which is in harmony with familiar sayings, is a truth wholly ignored in current educational fanaticisms.

There is ignored, too, the correlative truth; and ignoring it threatens results still more disastrous. While we see an expectation of benefits which the means used cannot achieve, we see no consciousness of injuries which will be entailed by these means. As usually happens with those absorbed in the eager pursuit of some good by governmental action, there is a blindness to the evil reaction on the natures of citizens. Already the natures of citizens have suffered from kindred reactions, due to actions set up centuries ago; and now the mischievous effects are to be increased by further such reactions.

The English people are complained of as improvident. Very few of them lay by in anticipation of times when work is slack; and the general testimony is that higher wages commonly result only in more extravagant living or in drinking to greater excess. As we saw a while since, they neglect opportunities of becoming shareholders in the companies they are engaged under; and those who are most anxious for their welfare despair on finding how little they do to raise themselves when they have the means. This tendency to seize immediate gratification regardless of future penalty is commented on as characteristic of the English people; and, contrasts between them and their Continental neighbors having been drawn, surprise is expressed that such contrasts should exist. Improvidence is spoken of as an inexplicable trait of the race—no regard being paid to the fact that races with which it is compared are allied in blood. The people of Norway are economical and extremely prudent. The Danes, too, are thrifty; and Defoe, commenting on the extravagance of his countrymen, says that a Dutchman gets rich on wages out of which an Englishman but just lives. So, too, if we take the modern Germans. Alike by the complaints of the Americans, that the Germans are ousting them from their own businesses by working hard and living cheaply, and by the success here of German traders and the preference shown for German waiters, we are taught that in other divisions of the Teutonic race there is nothing like this lack of self-control. Nor can we ascribe to such portion of Norman blood as exists among us this peculiar trait: descendants of the Normans in France are industrious and saving. Why, then, should the English people be improvident? If we seek explanation in their remote lineage, we find none; but, if we seek it in the social conditions to which they have been subject, we find a sufficient explanation. The English are improvident because they have been for ages disciplined in improvidence. Extravagance has been made habitual by shielding them from the sharp penalties extravagance brings. Carefulness has been discouraged by continually showing to the careful that those who were careless did as well as, or better than, themselves. Nay, there have been positive penalties on carefulness. Laborers working hard and paying their way have constantly found themselves called on to help in supporting the idle around them ; have had their goods taken under distress-warrants that paupers might be fed ; and eventually have found themselves and their children reduced also to pauperism. Well-conducted poor women, supporting themselves without aid or encouragement, have seen the ill-conducted receiving parish-pay for their illegitimate children. Nay, to such extremes has the process gone, that women with many illegitimate children, getting from the rates a weekly sum for each, have been chosen as wives by men who wanted the sums thus derived ! Generation after generation the honest and independent, not marrying till they had means, and striving to bring up their families without assistance, have been saddled with extra burdens, and hindered from leaving a desirable posterity; while the dissolute and the idle, especially when given to that lying and servility by which those in authority are deluded, have been helped to produce and to rear progeny, characterized, like themselves, by absence of the mental traits needed for good citizenship. And then, after centuries during which we have been breeding the race as much as possible from the improvident, and repressing the multiplication of the provident, we lift our hands and exclaim at the recklessness our people exhibit ! If men, who, for a score of generations, had by preference bred from their worst-tempered horses and their least-sagacious dogs, were then to wonder because their horses were vicious and their dogs stupid, we should think the absurdity of their policy paralleled only by the absurdity of their astonishment ; but human beings instead of inferior animals being in question, no absurdity is seen either in the policy or in the astonishment.

And now something more serious happens than the overlooking of these evils wrought on men's natures by centuries of demoralizing influences. We are deliberately establishing further such influences. Having, as much as we could, suspended the civilizing discipline of an industrial life so carried on as to achieve self-maintenance without injury to others, we now proceed to suspend that civilizing discipline in another direction. Having in successive generations done our best to diminish the sense of responsibility, by warding off evils which disregard of responsibility brings, we now carry the policy further by relieving parents from certain other responsibilities which, in the order of Nature, fall on them. By way of checking recklessness, and discouraging improvident marriages, and raising the conception of duty, we are diffusing the belief that it is not the concern of parents to fit their children for the business of life; but that the nation is bound to do this. Everywhere there is a tacit enunciation of the marvellous doctrine that citizens are not responsible individually for the bringing up each of his own children, but that these same citizens, incorporated into a society, are each of them responsible for the bringing up of everybody else's children! The obligation does not fall upon A in his capacity of father to rear the minds as well as the bodies of his offspring; but in his capacity of citizen there does fall on him the obligation of mentally rearing the offspring of B, C, D, and the rest, who similarly have their direct parental obligations made secondary to their indirect obligations to children not their own! Already it is estimated that, as matters are now being arranged, parents will soon pay in school-fees for their own children only one-sixth of the amount which is paid by them through taxes, rates, and voluntary contributions, for children at large: in terms of money, the claims of children at large to their care will be taken as six times the claim of their own children! And, if, looking back forty years, we observe the growth of the public claim *versus* the private claim, we may infer that the private claim will presently be absorbed wholly. Already the correlative theory is becoming so definite and positive that you meet with the notion, uttered as though it were an unquestionable truth, that criminals are "society's failures." Presently it will be seen that, since good bodily development, as well as good mental development, is a prerequisite to good citizenship (for without it the citizen cannot maintain himself, and so avoid wrong-doing), society is responsible also for the proper feeding and clothing of children: indeed, in school-board discussions, there is already an occasional admission that no logically-defensible halting-place can be found between the two. And so we are progressing toward the wonderful notion, here and there finding tacit expression, that people are to marry when they feel inclined, and other people are to take the consequences!

And this is thought to be the policy conducive to improvement of behavior. Men who have been made improvident by shielding them from many of the evil results of improvidence are now to be made more provident by further shielding them from the evil results of improvidence. Having had their self-control decreased by social arrangements which lessened the need for self-control, other social arrangements are devised which will make self-control still less needful: and it is hoped so to make self-control greater. This expectation is absolutely at variance with the whole order of things. Life of every kind, human included, proceeds on an exactly-opposite principle. All lower types of beings show us that the rearing of offspring affords the highest discipline for the faculties. The parental instinct is everywhere that which calls out the energies most persistently, and in the greatest degree exercises the intelligence. The self-sacrifice and the sagacity which inferior creatures display in the care of their young are often commented upon; and every one may see that parenthood produces a mental exaltation not otherwise producible. That it is so among mankind is daily proved. Continually we remark that men who were random grow steady when they have children to provide for; and vain, thoughtless girls, becoming mothers, begin to show higher feelings, and capacities that were not before drawn out. In both there is a daily discipline in unselfishness, in industry, in foresight. The parental relation strengthens from hour to hour the habit of postponing immediate ease and egoistic pleasure to the altruistic pleasure obtained by furthering the welfare of offspring. There is a frequent subordination of the claims of self to the claims of fellow-beings; and by no other agency can the practice of this subordination be so effectually secured. Not, then, by a decreased, but by an increased, sense of parental responsibility is self-control to be made greater and recklessness to be checked. And yet the policy now so earnestly and undoubtingly pursued is one which will inevitably diminish the sense of parental responsibility. This all-important discipline of parents' emotions is to be weakened that children may get reading, and grammar, and geography, more generally than they would otherwise do. A superficial intellectualization is to be secured at the cost of a deep-seated demoralization.

Few, I suppose, will deliberately assert that information is important and character relatively unimportant. Every one observes from time to time how much more valuable to himself and others is the workman who, though unable to read, is diligent, sober, and honest, than is the well-taught workman who breaks his engagements, spends days in drinking, and neglects his family. And, comparing members of the upper classes, no one doubts that the spendthrift or the gambler, however good his intellectual training, is inferior as a social unit to the man who, not having passed through the approved curriculum, nevertheless prospers by performing well the work he undertakes, and provides for his children instead of leaving them in poverty to the care of relatives. That is to say, looking at the matter in the concrete, all see that, for social welfare, good character is more important than much knowledge. And yet the manifest corollary is not drawn. What effect will be produced on character by artificial appliances for spreading knowledge is not asked. Of the ends to be kept in view by the legislator, all are unimportant compared with the end of character-making; and yet character-making is an end wholly unrecognized.

Let it be seen that the future of a nation depends on the natures of its units; that their natures are inevitably modified in adaptation to the conditions in which they are placed; that the feelings called into play by these conditions will strengthen, while those which have diminished demands on them will dwindle; and it will be seen that the bettering of conduct can be effected, not by insisting on maxims of good conduct, still less by mere intellectual culture, but only by that daily exercise of the higher sentiments and repression of the lower, which results from keeping men subordinate to the requirements of orderly social life—letting them suffer the inevitable penalties of breaking these requirements, and reap the benefits of conforming to them. This alone is national education.

A NATIONAL UNIVERSITY.[1]

By CHARLES W. ELIOT,

PRESIDENT OF HARVARD COLLEGE.

I TURN next to my third topic, the true policy of our government as regards university instruction. In almost all the writings about a nation's university, and of course in the two Senate bills now under discussion, there will be found the implication, if not the express assertion, that it is somehow the duty of our government to maintain a magnificent university. This assumption is the foundation upon which rest the ambitious projects before us, and many similar schemes. Let me try to demonstrate that the foundation is itself unsound.

The general notion that a beneficent government should provide and control an elaborate organization for teaching, just as it maintains an army, a navy, or a post-office, is of European origin, being a legitimate corollary to the theory of government by divine right. It is said that the state is a person having a conscience and a moral responsibility; that the government is the visible representative of a people's civilization, and the guardian of its honor and its morals, and should be the embodiment of all that is high and good in the people's character and aspirations. This moral person, this corporate representative of a Christian nation, has high duties and functions commensurate with its great powers, and none more imperative than that of diffusing knowledge and advancing science.

I desire to state this argument for the conduct of high educational institutions by government, as a matter of abstract duty, with all the force which belongs to it; for, under an endless variety of thin disguises, and with all sorts of amplifications and dilutions, it is a staple commodity with writers upon the relation of government to education. The conception of government upon which this argument is

[1] Closing argument of a report by President Eliot to the National Educational Association at its recent session in Elmira. The first part of the report gives an account of what had been done by the Association about the project of a national university since 1869; and the second part examines the two bills on the subject which were brought before Congress in 1872.

based is obsolescent everywhere. In a free community the government does not hold this parental, or patriarchal—I should better say godlike—position. Our government is a group of servants appointed to do certain difficult and important work. It is not the guardian of the nation's morals; it does not necessarily represent the best virtue of the republic, and is not responsible for the national character, being itself one of the products of that character. The doctrine of state personality and conscience, and the whole argument of the dignity and moral elevation of a Christian nation's government as the basis of government duties, are natural enough under grace-of-God governments, but they find no ground of practical application to modern republican confederations; they have no bearing on governments considered as purely human agencies with defined powers and limited responsibilities. Moreover, for most Americans these arguments prove a great deal too much; for, if they have the least tendency to persuade us that government should direct any part of secular education, with how much greater force do they apply to the conduct by government of the religious education of the people! These propositions are, indeed, the main arguments for an established church. Religion is the supreme human interest, government is the supreme human organization; therefore, government ought to take care for religion, and a Christian government should maintain distinctively Christian religious institutions. This is not theory alone; it is the practice of all Christendom, except in America and Switzerland. Now, we do not admit it to be our duty to establish a national church. We believe not only that our people are more religious than many nations which have established churches, but also that they are far more religious under their own voluntary system than they would be under any government establishment of religion. We do not admit for a moment that establishment or no establishment is synonymous with national piety or impiety. Now, if a beneficent Christian government may rightly leave the people to provide themselves with religious institutions, surely it may leave them to provide suitable universities for the education of their youth. And here again the question of national university or no national university is by no means synonymous with the question, Shall the country have good university education or not? The only question is, Shall we have a university supported and controlled by government, or shall we continue to rely upon universities supported and controlled by other agencies?

There is, then, no foundation whatever for the assumption that it is the duty of our government to establish a national university. I venture to state one broad reason why our government should not establish and maintain a university. If the people of the United States have any special destiny, any peculiar function in the world, it is to try to work out under extraordinarily favorable circumstances the problem of free institutions for a heterogeneous, rich, multitudinous population, spread over a vast territory. We, indeed, want to breed scholars, artists, poets, historians, novelists, engineers, physicians, jurists, theologians, and orators; but, first of all, we want to breed a race of independent, self-reliant freemen, capable of helping, guiding, and governing themselves. Now, the habit of being helped by the government, even if it be to things good in themselves—to churches, universities, and railroads—is a most insidious and irresistible enemy of republicanism; for the very essence of republicanism is self-reliance. With the Continental nations of Europe it is an axiom that the government is to do every thing, and is responsible for every thing. The French have no word for "public spirit," for the reason that the sentiment is unknown to them. This abject dependence on the government is an accursed inheritance from the days of the divine right of kings. Americans, on the contrary, maintain precisely the opposite theory—namely, that government is to do nothing not expressly assigned it to do, that it is to perform no function which any private agency can perform as well, and that it is not to do a public good even, unless that good be otherwise unattainable. It is hardly too much to say that this doctrine is the foundation of our public liberty. So long as the people are really free they will maintain it in theory and in practice. During the war of the rebellion we got accustomed to seeing the government spend vast sums of money and put forth vast efforts, and we asked ourselves, Why should not some of these great resources and powers be applied to works of peace, to creation as well as to destruction? So we subsidized railroads and steamship companies, and agricultural colleges, and now it is proposed to subsidize a university. The fatal objection to this subsidizing process is that it saps the foundations of public liberty. The only adequate securities of public liberty are the national habits, traditions, and character, acquired and accumulated in the practice of liberty and self-control. Interrupt these traditions, break up these habits or cultivate the opposite ones, or poison that national character, and public liberty will suddenly be found defenceless. We deceive ourselves dangerously when we think or speak as if education, whether primary or university, could guarantee republican institutions. Education can do no such thing. A republican people should, indeed, be educated and intelligent; but it by no means follows that an educated and intelligent people will be republican. Do I seem to conjure up imaginary evils to follow from this beneficent establishment of a superb national university? We teachers should be the last people to forget the sound advice—*obsta principiis.* A drop of water will put out a spark which otherwise would have kindled a conflagration that rivers could not quench.

Let us cling fast to the genuine American method—the old Massachusetts method—in the matter of public instruction. The essential features of that system are local taxes for universal elementary education voted by the citizens themselves, local elective boards to spend the money raised by taxation and control the schools, and for the higher grades of instruction permanent endowments administered by incorporated bodies of trustees. This is the American voluntary system, in sharp contrast with the military, despotic organization of public instruction which prevails in Prussia and most other states of Continental Europe. Both systems have peculiar advantages, the crowning advantage of the American method being that it breeds freemen. Our ancestors well understood the principle that, to make a people free and self-reliant, it is necessary to let them take care of themselves, even if they do not take quite as good care of themselves as some superior power might.

And now, finally, let us ask what should make a university at the capital of the United States, established and supported by the General Government, more national than any other American university. It might be larger and richer than any other, and it might not be; but certainly it could not have a monopoly of patriotism or of catholicity, or of literary or scientific enthusiasm. There are an attractive comprehensiveness and a suggestion of public spirit and love of country in the term "national;" but, after all, the adjective only narrows and belittles the noble conception contained in the word "university." Letters, science, art, philosophy, medicine, law, and theology, are larger and more enduring than nations. There is something childish in this uneasy hankering for a big university in America, as there is also in that impatient longing for a distinctive American literature which we so often hear expressed. As American life grows more various and richer in sentiment, passion, thought, and accumulated experience, American literature will become richer and more abounding, and in that better day let us hope that there will be found several universities in America, though by no means one in each State, as free, liberal, rich, national, and glorious, as the warmest advocate of a single crowning university at the national capital could imagine his desired institution to become.

AGASSIZ AND DARWINISM.

By JOHN FISKE,

RECENTLY LECTURER ON PHILOSOPHY AT HARVARD UNIVERSITY.

ONE Friday morning, a few weeks ago, as I was looking over the *Nation*, my eye fell upon an advertisement, inserted by the proprietors of the *New-York Tribune*, announcing the final destruction of Darwinism. What especially riveted my attention was the peculiar style of the announcement: "The Darwinian Theory utterly demolished" (or words to that effect) "by Agassiz Himself!" Whether from accident or design, the type-setter's choice of Roman capitals was very happy. Upon many readers the effect must have been tremendous; and quite possibly there may be some who, without further investigation, will carry to their dying day the opinion that it is all over with the Darwinian theory, since "Agassiz Himself" has refuted it.

Upon me the effect was such as to make me lay down my paper and ask myself: Can it be that we have, after all, a sort of scientific pope among us? Has it come to this, that the dicta of some one "servant and interpreter of Nature" are to be accepted as final, even against the better judgment of the majority of his compeers? In short, who is Agassiz himself, that he should thus single-handed have demolished the stoutest edifice which observation and deduction have reared since the day when Newton built to such good purpose?

Prof. Agassiz is a naturalist who is justly world-renowned for his achievements. His contributions to geology, to paleontology, and to systematic zoology, have been such as to place him in a very high rank among contemporary naturalists. Not quite in the highest place, I should say; for, apart from all questions of theory, it is probable that Mr. Darwin's gigantic industry, his wonderful thoroughness and accuracy as an observer, and his unrivalled fertility of suggestion, will cause him in the future to be ranked along with Aristotle, Linnæus, and Cuvier; and upon this high level we cannot place Prof. Agassiz. Leaving Mr. Darwin out of the account, we may say that Prof. Agassiz stands in the first rank of contemporary naturalists. But any exceptional supremacy in this first rank can by no means be claimed for him. Both for learning and for sagacity, the names of Gray, Wyman, Huxley, Hooker, Wallace, Lubbock, Lyell, Vogt, Haeckel, and Gegenbaur, are quite as illustrious as the name of Agassiz; and we may note, in passing, that these are the names of men who openly indorse and defend the Darwinian theory.

Possibly, however, there are some who will not be inclined to accept the estimates made in the foregoing paragraph. No doubt there are many people in this country who have long accustomed themselves to regard Prof. Agassiz not simply as one among a dozen or twenty living naturalists of the highest rank, but as occupying a solitary position as the greatest of all living naturalists—as a kind of second Cuvier, for example. There is, to the popular eye, a halo about the name of Agassiz which there is not about the name of Gray; though, if there is any man now living in America, of whom America might justly boast as her chief ornament and pride, so far as science is concerned, that man is unquestionably Prof. Asa Gray. Now, this greater popular fame of Agassiz is due to the fact that he is a European who cast in his lot with us at a time when we were wont to over-rate foreign importations of whatever sort. As a European, therefore, he outshines such men as Profs. Gray and Wyman, and, as a man whom we know, he outshines other Europeans, like Haeckel and Gegenbaur, whose acquaintance we happen not to have made; just as Rubinstein, whose fame has filled the American newspapers, outshines Bülow (probably his equal as a pianist), who has not yet visited this country. In this way Prof. Agassiz has acquired a reputation in America which is greater than his reputation in Europe, and which is greater than his achievements—admirable as they are—would be able, on trial, to sustain.

And now I come to my first point. Admitting for Prof. Agassiz all the wonderful greatness as a naturalist with which the vague sentiments of the uneducated multitude in this country would accredit him; admitting, in other words, that he is the greatest of naturalists, and not one among a dozen or twenty equals; it must still be asked, why should his rejection of Darwinism be regarded as conclusively fatal to the Darwinian theory? The history of science supplies us with many an instance in which a new and unpopular theory has been vehemently opposed by those whom one would at first suppose most competent to judge of its merits, and has nevertheless gained the victory. Dr. Draper brings a terrible indictment against Bacon for rejecting the Copernican theory, and refusing to profit by the discoveries of Gilbert in magnetism. This should not be allowed to detract from Bacon's real greatness, any more than the rejection of Darwinism should be allowed to detract from the real merit of Agassiz. Great men must be measured by their positive achievements rather than by their negative shortcomings, otherwise they might all have to step down from their pedestals. Leibnitz rejected Newton's law of gravitation; Harvey saw nothing but foolishness in Aselli's discovery of the lacteals; Magendie ridiculed the great work in which the younger Geoffroy Saint-Hilaire began to investigate the conditions of nutrition which determine the birth of monsters; and when Young, Fresnel, and Malus, completed the demonstration of that undulatory theory of light which has made their names immortal, Laplace, nevertheless, the greatest mathematician of the age, persisted until his dying day in heaping contumely upon these eminent men and upon their arguments. Nay, even Cuvier—the teacher whom Prof. Agassiz so justly reveres—did not Cuvier adhere to the last to the grotesque theory of "pre-formation," and reject the true theory of "epigenesis," which C. F. Wolff, even before Baer, had placed upon a scientific basis? Supposing, then, that the Darwinian theory is rejected by Agassiz, this fact is no more decisive against the Darwinian theory than the rejection of Fresnel's theory by Laplace was decisive against Fresnel's theory.

For the facts just cited show that even the wisest and most learned men are not infallible, and that it will not do to have a papacy where scientific questions are concerned. Strange as it may at first seem, nothing is more certain than that a man's opinion may be eminently fallible, even with reference to matters which might appear to come directly within the range of his own specialty. Many people, I presume, think that, because Prof. Agassiz has made a specialty of the study of extinct and living organisms, because he has devoted a long and industrious life to this study, therefore his opinion with reference to the relations of present life upon the globe to past life ought to be at once conclusive. The fallacy of this inference becomes apparent as soon as we recollect that Profs. Gray, Wyman, Huxley, and Haeckel, who are equally well qualified to have an opinion on such matters, have agreed in forming an opinion diametrically opposite to that of Prof. Agassiz. But the fallacy may be shown independently of any such comparison. Even if all the foundations of certainty seem to be shaking beneath us when we say that an expert is not always the best judge of matters pertaining to his own specialty, we must still say it, for facts will bear us out in saying it. I have known excellent mathematicians and astronomers who had not the first word to say about the Nebular Hypothesis: they had never felt interested in it, had never studied it, and consequently did not understand it, and could hardly state it correctly. After a while one ceases to be surprised at such things. It is quite possible for one to study the structure of echinoderms and fishes during a long life, and yet remain unable to offer a satisfactory opinion upon any subject connected with zoology, for the proper treatment of which there are required some power of generalization and some familiarity with large considerations. Indeed, there are many admirable experts in natural history, as well as in other studies, who never pay the slightest heed to questions involving wide-reaching considerations; and who, with all their amazing minuteness of memory concerning the metamorphoses of insects and the changes which the embryo of a white-fish undergoes from fecundation to maturity, are nevertheless unable to see the evidentiary value of the great general facts of geological succession and geographical distribution, even when it is thrust directly before their eyes. To such persons, "science" means the collecting of polyps, the dissecting of mollusks, the vivisection of frogs, the registration of innumerable facts of detail, without regard to the connected story which all these facts, when put together, have it in their power to tell. And all putting together of facts, with a view to elicit this connected story, they are too apt to brand as unscientific speculation; forgetting that if Newton had merely occupied himself with taking observations and measuring celestial distances, instead of propounding an audacious hypothesis, and then patiently verifying it, the law of gravitation might never have been discovered. Herein lies the explanation of the twice-repeated rejection of Mr. Darwin's name by the French Academy of Sciences. The lamentable decline of science in France since the beginning of the Second Empire has been most conspicuously marked by the tendency of scientific inquirers to occupy themselves exclusively with matters of detail, to the neglect of wide-reaching generalizations. And the rejection of Mr. Darwin's name was justified upon the ground, not that he had made unscientific generalizations, but that he had been a *mere* (!) generalizer, instead of a collector of facts. The allegation was, indeed, incorrect; since Mr. Darwin is as eminent for his industry in collecting facts as for his boldness in generalizing. But the form of the allegation well illustrates the truth of what I have been seeking to show—that familiarity with the details of a subject does not enable one to deal with it in the grand style, and elicit new truth from old facts, unless one also possesses some faculty for penetrating into the hidden implications of the facts; or, in other words, some faculty for philosophizing.

Now, I am far from saying of Prof. Agassiz that he is a mere collector of echinoderms and dissector of fishes, with no tact whatever in philosophizing. He does not stand in the position of those who think that the end of scientific research is attained when we have carefully ticketed a few thousand specimens of corals and butterflies, in much the same spirit as that in which a school-girl collects and classifies autographs or postage-stamps. Along with his indefatigable industry as a collector and observer, Prof. Agassiz has a decided inclination toward general views. However lamentably deficient we may think him in his ability to discern the hidden implications of facts, there can be no question that his facts are of little importance to him save as items in a philosophic scheme. He knows very well—perhaps almost too well—that the value of facts lies in the conclusions to which they point. And, accordingly, lack of philosophizing is the last shortcoming with which, as a scientific writer, he can be charged. If he errs on a great scientific question, lying within his own range of investigation, it is not because he refrains steadfastly from all general considerations, but because he philosophizes—and philosophizes on unsound principles. It is because his philosophizing is not a natural outgrowth from the facts of Nature which lie at his disposal, but is made up out of sundry traditions of his youth, which, by dint of playing upon the associations of ideas which are grouped around certain combinations of words, have come to usurp the place of observed facts as a basis for forming conclusions. It is not because he abstains from generalizing that Prof. Agassiz is unable to appreciate the arguments by which Mr. Darwin has established his theory, but it is because he long ago brought his mind to acquiesce in various generalizations, of a thoroughly unscientific or non-scientific character, with the further maintenance of which the acceptance of the Darwinian theory is (or seems to Prof. Agassiz to be) incompatible.

The generalizations which have thus preoccupied Prof. Agassiz's mind are purely theological or mythological in their nature. In estimating the probable soundness of his opinion upon any scientific question, it must always be remembered that he is, above all things, a devotee of what is called "natural theology." In his discussions concerning the character of the relationships between the various members of the animal kingdom, the foreground of his consciousness is always completely occupied by theological considerations, to such an extent that the evidentiary value of scientific facts cannot always get a footing there, and is, consequently, pushed away into the background. One feels, in reading his writings, that, except when he is narrating facts with the pure joyfulness of a specialist exulting in the exposition of his subject (and, when in this mood, he often narrates facts with which his inferences are wholly incompatible), he never makes a point without some regard to its bearings upon theological propositions which his early training has led him to place paramount to all facts of observation whatever. In virtue of this peculiarity of disposition, Prof. Agassiz has become the welcome ally of those zealous but narrow-minded theologians, in whom the rapid progress of the Darwinian theory has awakened the easily explicable but totally groundless fear that the necessary foundations of true religion, or true Christianity, are imperilled. It is not many years since these very persons regarded Prof. Agassiz with dread and abhorrence, because of his flat contradiction of the Bible in his theory of the multiple origin of the human race. But, now that the doctrine of Evolution has come to be the unclean thing above all others to be dreaded and abhorred, this comparatively slight iniquity of Prof. Agassiz has been condoned or forgotten, and, as the great antagonist of Evolution, he is welcomed as the defender of the true Church against her foes.

This preference of theological over scientific considerations once led Prof. Agassiz (if my memory serves me rightly) to use language very unbecoming in a professed student of Nature. Some seven years ago he delivered a course of lectures at the Cooper Union, and in one of these lectures he observed that he *preferred* the theory which makes man out a fallen angel to the theory which makes him out an improved monkey—a remark which was quite naturally greeted with laughter and applause. But the applause was ill-bestowed, for the remark was one of the most degrading which a scientific lecturer could make. A scientific inquirer has no business to have "preferences." Such things are fit only for silly women of society, or for young children who play with facts, instead of making sober use of them. What matters it whether we are pleased with the notion of a monkey-ancestry or not? The end of scientific research is the discovery of truth, and not the satisfaction of our whims or fancies, or even of what we are pleased to call our finer feelings. The proper reason for refusing to accept any doctrine is, that it is inconsistent with observed facts, or with some other doctrine which has been firmly established on a basis of fact. The refusal to entertain a theory because it seems disagreeable or degrading, is a mark of intellectual cowardice and insincerity. In matters of scientific inquiry, it is as grave an offence as the letting one's note go to protest is in matters of business. In saying these things, I do not mean to charge Prof. Agassiz with intellectual cowardice and insincerity, for the remark which I criticise so sharply was not worthy of him, it did not comport with his real character as a student of science, and to judge of him by this utterance alone would be to do him injustice.

It was with the hope of finding some more legitimate objections to the Darwinian theory that I procured the *Tribune's* lecture-sheet containing Prof. Agassiz's twelve lectures on the natural foundations of organic affinity, and diligently searched it from beginning to end. I believe I am truthful in saying that a good staggering objection would have been quite welcome to me, just for the sake of the intellectual stimulus implied in dealing with it, for on this subject my mind was so thoroughly made up thirteen years ago, that the discussion of it, as ordinarily conducted, has long since ceased to have any interest for me. I am just as firmly convinced that the human race is descended from lower animal forms, as I am that the earth revolves in an elliptical orbit about the sun. So completely, indeed, is this proposition wrought in with my whole mental structure, that the negation of it seems to me utterly nonsensical and void of meaning, and I doubt if my mind is capable of shaping such a negation into a proposition which I could intelligently state. To have such deeply-rooted convictions shaken once in a while is, I believe, a very useful and wholesome experiment in mental hygiene. That rigidity of mind which prevents the thorough revising of our opinions is sure, sooner or later, to come upon all of us; but we ought to dread it, as we dread the stagnation of old age or death. For some such reasons as these, I am sure that I should have been glad to find, in the course of Prof. Agassiz's lectures, at least one powerful argument against the interpretation of organic affinities which Mr. Darwin has done so much to establish. I should have been still more glad to find some alternative interpretation proposed which could deserve to be entertained as scientific in character. I am sure no task could be more delightful, or more quickening to one's energies, than that of comparing two alternative theories upon this subject, upon which, thus far, only one has ever been propounded which possesses the marks of a scientific hypothesis. But no such pleasure or profit is in store for any one who studies these twelve lectures of Prof. Agassiz. In all these lectures, there is not a single allusion to Mr. Darwin's name, save once in a citation from another author; there is not the remotest allusion to any of the arguments by which Mr. Darwin has contributed most largely to the establishment of the development theory; nay, there is not a single sentence from which one could learn that Mr. Darwin's books had ever been written, or that the theories which they expound had ever taken shape in the mind of any thinking man. I do not doubt that Prof. Agassiz has, at some time read, or looked over, the "Origin of Species;" but there is not a word in these lectures which might not have been written by one who had never heard of that book, or of the arguments which made the publication of it the beginning of a new epoch in the history of science.

Not only is it that Prof. Agassiz does not attack the Darwinian theory in these lectures; it is also that, until the ninth lecture, he does not allude to the doctrine of Evolution in any way. His first eight lectures consist mostly in an account of the development of the embryo in various animals; and in this we have a pure description of facts with which no one certainly will feel like quarrelling, so far as theories are concerned. He goes to work, very much as Max Müller does, in lecturing about the science of language, when he gives you a maximum of interesting etymologies and a minimum of real philosophizing which goes to the bottom of things. But Prof. Agassiz is not so interesting or so stimulating in his discourse as Max Müller. He does not lead us into pleasant fields of illustration, where we would fain tarry longer, forgetting the main purpose of the discussion in our delight at the unessential matters which occupy our attention. On the contrary, it seems to me that Prof. Agassiz's explanation of the development of eggs is rather tedious and dry, and by no means richly fraught with novel suggestions. The exposition is a commonplace one, such as is good for students in the Museum of Comparative Zoology, who are beginning to study embryology, but there are no features which make it especially interesting or instructive to any one who has already served an apprenticeship in these matters.

In his ninth lecture, Prof. Agassiz begins to make some allusion to the development theory—not to the development theory as it now stands since the publication of the "Origin of Species," but to the development theory as it stood in the days when Prof. Agassiz was a young student, when Cuvier and the elder Geoffroy Saint-Hilaire waged fierce warfare in the French Academy, and when the aged Goethe, sanest and wisest of men, foresaw in the issue of that battle the speedy triumph of the development theory. Beyond this point, I will venture to say, Prof. Agassiz has never travelled. The doctrine of Evolution is still, to him, what it was in those early days; and all the discoveries and reasonings of Mr. Darwin have passed by him unheeded and unnoticed. He arrived too early at that rigidity of mind which prevents us from properly comprehending new theories, and which we should all of us dread.

What, now, is the doctrine which Prof. Agassiz begins to attack, in his ninth lecture, and what is the doctrine which he would propose as a substitute? The doctrine which he attacks is simply this—that all organic beings have come into existence through some natural process of causation; and the doctrine which he defends is just this—that all organic beings, as classed in species, have come into existence at the outset by means of some act of which our ordinary notions of cause and effect can give no account whatever. For every one of the individuals of which a species is made up, he will admit the adequacy of the ordinary process of generation; but for the species as a whole, this process seems to him inadequate, and he flies at once to that refuge of inconsequent and timid minds—*miracle!*

This is really just what Prof. Agassiz's theory of the origin of specific forms amounts to, and this is the reason why, in spite of grave heresy on minor points, he is now regarded by the evangelical Church as one of its chief champions. Instead of the natural process of generation—which is the only process by which we have ever known organic beings to be produced—he would fain set up some unknown mysterious process, the nature of which he is careful not to define, but for which he endeavors to persuade us that we have a fair equivalent in sonorous phrases concerning "creative will," "free action of an intelligent mind," and so on. In thus postponing considerations of pure science to considerations of "natural theology," I have no doubt Prof. Agassiz is actuated by a praiseworthy desire to do something for the glory of that Power of which the phenomenal universe is the perpetual but ever-changing manifestation. But how futile is such an attempt as this! How contrary to common-sense it is to say that a species is produced, *not* by the action of blind natural forces, *but* by an intelligent will! For, although this most prominent of all facts seems to be oftenest overlooked by theologians and others whom it most especially concerns, we are all the time, day by day and year by year, in each and every event of our lives, having experience of the workings of that Divine Power which, whether we attribute to it "intelligent will" or not, is unquestionably the one active agent in all the dynamic phenomena of Nature. Little as we know of the intrinsic nature of this Omnipresent Power, which, in our poor human talk, we call God, we do at least know, by daily and hourly experience, what is the character of its working. The whole experience of our lives teaches us that this Power works after a method which, in our scholastic expression, we call the method of cause and effect, or the method of natural law. Traditions of a barbarous and uncultivated age, in which mere grotesque associations of thoughts were mistaken for facts, have told us that this Power has, at various times in the past, worked in a different way—causing effects to appear without cognizable antecedents, even as Aladdin's palace rose in all its wondrous magnificence, without sound of carpenter's hammer or mason's chisel, in a single night. But about such modes of divine action we know nothing whatever from experience; and the awakening of literary criticism, in modern times, has taught us to distrust all such accounts of divine action which conflict with the lessons we learn from what is ever going on round about us. So far as we know aught concerning the works of God, which are being performed in us, through us, and around us, during every moment of that conscious intelligence which enables us to bear witness to them, we know they are works from which the essential relation of a given effect to its adequate cause is never absent. And for this reason, if we view the matter in pure accordance with experience, we are led to maintain that the antagonism or contrariety which seems to exist in Prof. Agassiz's mind between the action of God and the action of natural forces is nothing but a figment of that ancestral imagination from which the lessons which shaped Prof. Agassiz's ways of thinking were derived. So far as experience can tell us any thing, it tells us that divine action *is* the action of natural forces; for, if we refuse to accept this conclusion, what have we to do but retreat to the confession that we have no experience of divine action whatever, and that the works of God have been made manifest only to those who lived in that unknown time when Aladdin's palaces were built, and when species were created, in a single night, without the intervention of any natural process?

Trusting, then, in this universal teaching of experience, let us for a moment face fairly the problem which the existence of men upon the earth presents to us. Here is actually existing a group of organisms, which we call the human race. Either it has existed eternally, or some combination of circumstances has determined its coming into existence. The first alternative is maintained by no one, and our astronomical knowledge of the past career of our planet is sufficient decisively to exclude it. There is no doubt that at some time in the past the human race did not exist, and that its gradual or sudden coming into existence was determined by some combination of circumstances. Now, when Prof. Agassiz asks us to see, in this origination of mankind, the working of a Divine Power, we acquiesce in all reverence. But when he asks us to see in this origination of mankind the working of a Divine Power, *instead of* the working of natural causes, we do not acquiesce, because, so far as experience has taught us any thing, it has taught us that Divine Power never works except by the way of natural causation. Experience tells us that God causes Aladdin's palaces to come into existence gradually, through the coöperation of countless minute antecedents. And it tells us, most emphatically, that such structures do not come into existence without an adequate array of antecedents, no matter what the Arabian Nights may tell us to the contrary.

Now, when Prof. Agassiz asks us to believe that species have come into existence by means of a special creative fiat, and not through the operation of what are called natural causes, we reply that his request is mere inanity and nonsense. We have no reason to suppose that any creature like a man, or any other vertebrate, or articulate, or mollusk, ever came into existence by any other process than the familar process of physical generation. To ask us to believe in any other process is to ask us to abandon the experience which we have for the chimeras which we had best not seek to acquire. But Prof. Agassiz does not even suggest any other process for our acceptance. He simply retreats upon his empty phrases, "creative will," the "free workings of an intelligent mind," and so on. Now, in his second course of lectures, I hope he will proceed to tell us, not necessarily how "creative will" actually operated in bringing forth a new species, but how it *may* conceivably have operated, save through the process of physical generation, which we know. In his "Essay on Classification," I remember a passage in which he rightly rejects the notion that any species has arisen from a single pair of parents, and propounds the formula: "Pines have originated in forests, heaths in heather, grasses in prairies, bees in hives, herrings in shoals, buffaloes in herds, men in nations." Now, when Prof. Agassiz asserts that men originated in nations, by some other process than that of physical generation, what does he mean? Does he mean that men dropped down from the sky? Does he mean that the untold millions of organic particles which make up a man all rushed together from the four quarters of the compass, and proceeded, spontaneously or by virtue of some divine sorcery, to aggregate themselves into the infinitely complex organs and tissues of the human body, with all their wondrous and well-defined aptitudes? It is time that this question should be faced, by Prof. Agassiz and those who agree with him, without further shirking. Instead of grandiloquent phrases about the "free action of an intelligent mind," let us have something like a candid suggestion of some process, other than that of physical generation, by which a creature like man can even be imagined to have come into existence. When the time comes for answering this question, we shall find that even Prof. Agassiz is utterly dumb and helpless. The sonorous phrase "special creation," in which he has so long taken refuge, is nothing but a synthesis of vocal sounds which covers and, to some minds, conceals a thoroughly idiotic absence of sense or significance. To say that "Abracadabra is not a genial corkscrew," is to make a statement quite as full of meaning as the statement that species have originated by "special creation."

The purely theological (or theologico-metaphysical and at all events unscientific) character of Prof. Agassiz's objections to the development theory is sufficiently shown by the fact that, in the foregoing paragraphs, I have considered whatever of any account there is in his lectures which can be regarded as an objection. *Arguments* against the development theory such objections cannot be called: they are, at their very best, nothing but *expressions of fear and dislike.* The only remark which I have been able to find, worthy of being dignified as an argument, is the following: "We see that fishes are lowest, that reptiles are higher, that birds have a superior organization to both, and that mammals, with man at their head, are highest. The phases of development which a quadruped undergoes, in his embryonic growth, recall this gradation. He has a fish-like, a reptile-like stage before he shows unmistakable mammal-like features. We do not on this account suppose a quadruped grows out of a fish in our time, for this simple reason, that we live among quadrupeds and fishes, and we know that no such thing takes place. But resemblances of the same kind, separated by geological ages, allow play for the imagination, and for inference unchecked by observation."

I do not believe that Prof. Agassiz's worst enemy—if he ever had an enemy—could have been so hard-hearted as to wish for him the direful catastrophe into which this wonderful piece of argument has plunged him irretrievably. For the question must at once suggest itself to every reader at all familiar with the subject, If Prof. Agassiz supposes that the development theory, as held nowadays, implies that a quadruped was ever the direct issue of a fish, of what possible value can his opinion be as regards the development theory in any way?

If I may speak frankly, as I have indeed been doing from the outset, I will say that, as regards the Darwinian theory, Prof. Agassiz seems to me to be hopelessly behind the age. I have never yet come across the first indication that he knows what the Darwinian theory is. Against the development theory, as it was taught him by the discussions of forty years ago, he is fond of uttering, I will not say arguments, but expressions of dislike. With the modern development theory, with the circumstances of variation, heredity, and natural selection, he never, in any of his writings, betrays the slightest acquaintance. Against a mere man of straw of his own devising, he industriously hurls anathemas of a quasi-theological character. But any thing like a scientific examination of the character and limits of the agency of natural selection in modifying the appearance and structure of a species, any thing like such an examination as is to be found in the interesting work of Mr. St. George Mivart, he has never yet brought forth.

Now, when Prof. Agassiz fairly comes to an issue, if he ever does, and undertakes to refute the Darwinian theory, these are some of the questions which he will have to answer: 1. If all organisms are not associated through the bonds of common descent, why is it that the facts of classification are just such as they would have been had they been due to such a common descent? 2. Why does a mammal always begin to develop as if it were going to become a fish, and then, changing its tactics, proceed as if it were going to become a reptile or bird, and only after great delay and circumlocution take the direct road toward mammality? In answer to this, we do not care to be told that a mammal never was the son of a fish, because we know that already; nor do we care to hear any more about the "free manifestations of an intelligent mind," because we have had quite enough of metaphysical phrases which do not contain a description of some actual or imaginable process. We want to know how this state of things can be scientifically interpreted save on the hypothesis of a common ultimate origin for mammals, birds, reptiles, and fishes. 3. What is the meaning of such facts as the homologies which exist between corresponding parts of organisms constructed on the same type? Why does the black salamander retain fully-developed gills which he never uses, and what is the significance of rudimentary and aborted organs in general? Again I say, we do not want to hear about "uniformity of design" and "reminiscences of a plan," and so on, but we wish to know how this state of things was physically brought about, save by community of descent. 4. Why is it that the facts of geological succession and geographical distribution so clearly indicate community of descent, unless there has actually been community of descent? Why have marsupials in Australia followed after other marsupials, and edentata in South America followed after other edentata, with such remarkable regularity, unless the bond which unites present with past ages be the well-known, the only known, and the only imaginable bond of physical generation? Why are the fauna and flora of each geologic epoch in general intermediate in character between the flora and fauna of the epochs immediately preceding and succeeding? And, 5. What are we to do with the great fact of *extinction* if we reject Mr. Darwin's explanations? When a race is extinguished, is it because of a universal deluge, or because of the "free manifestations of an intelligent mind?" For surely Prof. Agassiz will not attribute such a solemn result to such ignoble causes as insufficiency of food or any other of the thousand causes, "blindly mechanical," which conspire to make a species succumb in the struggle for life.

And here the phrase, "struggle for life," reminds me of yet another difficult task which Prof. Agassiz will have before him when he comes to undertake the refutation of Darwinism in earnest. He will have to explain away the enormous multitude of facts which show that there is a struggle for life in which the fittest survive; or he will at any rate have to show in what imaginable way an organic type can remain constant in all its features through countless ages under the influence of such circumstances, unless by taking into the account the Darwinian interpretation of persistent types offered by Prof. Huxley.

But I will desist from further enumeration of the difficulties which surround this task which Prof. Agassiz has not undertaken, and is not likely ever to undertake. For the direct grappling with that complicated array of theorems which the genius of such men as Darwin and Spencer and their companions has established on a firm basis of observation and deduction, Prof. Agassiz seems in these lectures hardly better qualified than a child is qualified for improving the methods of the integral calculus. These questions have begun to occupy earnest thinkers since the period when his mind acquired that rigidity which prevents the revising of one's opinions. The marvellous flexibility of thought with which Sir Charles Lyell so gracefully abandoned his antiquated position, Prof. Agassiz is never likely to show. This is largely because Lyell has always been a thinker of purely scientific habit, while Agassiz has long been accustomed to making profoundly dark metaphysical phrases do the work which properly belongs to observation and deduction. But, however we may best account for these idiosyncrasies, it remains most probable among those facts which are still future, that Prof. Agassiz will never advance any more crushing refutation of the Darwinian theory than the simple expression of his personal dislike for "mechanical agencies," and his belief in the "free manifestations of an intelligent mind." Were he only to be left to himself, such expressions of personal preference could not mar the pleasure with which we often read his exposition of purely scientific truths. But when he is brought before the public as the destroyer of a theory, the elements of which he has never yet given any sign of having mastered, he is placed in a false position, which would be ludicrous could he be supposed to have sought it, and which is, at all events, unworthy of his eminent fame.

THE PRIMARY CONCEPTS OF MODERN PHYSICAL

ERRATUM.

Page 710, line 32, for "impenetrability," read "compenetrability."

German Naturalists and Physicians, ...
Du Bois-Reymond, one of the most noted physicists of the age. "Natural science," says Du Bois-Reymond,[1] "is a reduction of the changes in the material world to motions of atoms caused by central forces independent of time, or a resolution of the phenomena of Nature into atomic mechanics. . . . The resolution of all changes in the material world into motions of atoms caused by their constant central forces would be the completion of natural science."

Obviously, the proposition thus enounced assigns to physical sci-

[1] "Ueber die Grenzen des Naturerkennens. Ein Vortrag in der zweiten öffentlichen Sitzung der 45. Versammlung deutscher Naturforscher und Aerzte zu Leipzig am 14. August 1872, gehalten von Emil Du Bois-Reymond." Leipzig, Veit & Comp., 1872.

ence limits so narrow that all attempts to bring the characteristic phenomena of organic life (not to speak of mental action) within them are utterly hopeless. Nevertheless, it is asserted that organic phenomena are the product of ordinary physical forces alone, and that the assumption of vital agencies, as distinct from the forces of inorganic Nature, is wholly inadmissible. In view of this, it seems strange that the validity of the proposition above referred to has never, so far as I know, been questioned, except in the interest of some metaphysical or theological system. It is my purpose in the following essays to offer a few suggestions in this behalf, in order to ascertain, if possible, whether the prevailing primary notions of physical science can stand, or are in need of revision.

One of the prime postulates of the mechanical theory is the atomic constitution of matter. A discussion of this theory, therefore, at once leads to an examination of the grounds upon which the assumption of atoms, as the ultimate constituents of the physical world, rests.

The doctrine that an exhaustive analysis of a material body into its real elements, if it could be practically effected, would yield an aggregate of indivisible and indestructible particles, is almost coeval with human speculation, and has held its ground more persistently than any other tenet of science or philosophy. It is true that the atomic theory, since its first promulgation by the early Greek philosophers, and its elaborate statement by Lucretius, has been modified and refined. There is probably no one, at this day, who invests the atoms with hooks and loops, or (Lucretius, *De Rerum Natura*, ii., 398, *et seq.*) accounts for the bitter taste of wormwood by the raggedness, and for the sweetness of honey by the smooth roundness of the constituent atoms. But the "atom" of modern science is still of determinate weight, if not of determinate figure, and stands for something more than an abstract unit, even in the view of those who, like Boscovich, Faraday, Ampère, or Fechner, profess to regard it as a mere centre of force. And there is no difficulty in stating the atomic doctrine in terms applicable alike to all the acceptations in which it is now held by scientific men. Whatever diversity of opinion may prevail as to the form, size, etc., of the atoms, all who advance the atomic hypothesis, in any of its varieties, as a physical theory, agree in three propositions, which may be stated as follows:

1. *Atoms are absolutely simple, unchangeable, indestructible; they are physically, if not mathematically, indivisible.*

2. *Matter consists of discrete parts, the constituent atoms being separated by void interstitial spaces. In contrast to the continuity of space stands the discontinuity of matter. The expansion of a body is simply an increase, its contraction a lessening of the spatial intervals between the atoms.*

3. *The atoms composing the different chemical elements are of de-*

terminate specific weights, corresponding to their equivalents of combination.[1]

Confessedly the atomic theory is but an hypothesis. This in itself is not decisive against its value; all physical theories properly so called are hypotheses whose eventual recognition as truths depends upon their consistency with themselves, upon their agreement with the canons of logic, upon their congruence with the facts which they serve to connect and explain, upon their conformity with the ascertained order of Nature, upon the extent to which they approve themselves as reliable anticipations or previsions of facts verified by subsequent observation or experiment, and finally upon their simplicity, or rather their reducing power. The merits of the atomic theory, too, are to be determined by seeing whether or not it satisfactorily and simply accounts for the phenomena as the explanation of which it is propounded, and whether or not it is in harmony with itself and with the known laws of Reason and of Nature.

For what facts, then, is the atomic hypothesis meant to account, and to what degree is the account it offers satisfactory?

It is claimed that the first of the three propositions above enumerated (the proposition which asserts the persistent integrity of atoms, or their unchangeability both in weight and volume) accounts for the indestructibility and impenetrability of matter; that the second of these propositions (relating to the discontinuity of matter) is an indispensable postulate for the explanation of certain physical phenomena, such as the dispersion and polarization of light; and that the third proposition (according to which the atoms composing the chemical elements are of determinate specific gravities) is the necessary general expression of the laws of definite constitution, equivalent proportion, and multiple combination, in chemistry.

In discussing these claims, it is important, first, to verify the facts and to reduce the statements of these facts to exact expression, and then to see how far they are fused by the theory:

1. The indestructibility of matter is an unquestionable truth. But in what sense, and upon what grounds, is this indestructibility predicated of matter? The unanimous answer of the atomists is: Experience teaches that all the changes to which matter is subject are but variations of form, and that amid these variations there is an unvarying constant—the mass or quantity of matter. The constancy of the mass is attested by the balance, which shows that neither fusion nor sublimation, neither generation nor corruption, can add to or detract from the weight of a body subjected to experiment. When a pound of carbon is burned, the balance demonstrates the continuing exist-

[1] To avoid confusion, I purposely ignore the distinction between *molecules* as the ultimate products of the physical division of matter, and *atoms* as the ultimate products of its chemical decomposition, preferring to use the word *atoms* in the sense of the least particles into which bodies are divisible or reducible by any means.

ence of this pound in the carbonic acid, which is the product of combustion, and from which the original weight of carbon may be recovered. The quantity of matter is measured by its weight, and this weight is unchangeable.

Such is the fact, familiar to every one, and its interpretation, equally familiar. To test the correctness of this interpretation, we may be permitted slightly to vary the method of verifying it. Instead of burning the pound of carbon, let us simply carry it to the summit of a mountain, or remove it to a lower latitude; is its weight still the same? Relatively it is; it will still balance the original counterpoise. But the absolute weight is no longer the same. This appears at once, if we give to the balance another form, taking a pendulum instead of a pair of scales. The pendulum on the mountain or near the equator vibrates more slowly than at the foot of the mountain or near the pole, for the reason that it has become specifically lighter by being farther removed from the centre of the earth's attraction, in conformity to the law that the attractions of bodies vary inversely as the squares of their distances.

It is thus evident that the constancy, upon the observation of which the assertion of the indestructibility of matter is based, is simply the constancy of a relation, and that the ordinary statement of the fact is crude and inadequate. Indeed, while it is true that the weight of a body is a measure of its mass, this is but a single case of the more general fact that the masses of bodies are inversely as the velocities imparted to them by the action of the same force, or, more generally still, inversely as the accelerations produced in them by the same force. In the case of gravity, the forces of attraction are directly proportional to the masses, so that the action of the forces (*weight*) is the simplest measure of the relation between any two masses as such; but, in any inquiry relating to the validity of the atomic theory, it is necessary to bear in mind that this weight is not the equivalent, or rather presentation, of an absolute substantive entity in one of the bodies (the body weighed), but the mere expression of a relation between two bodies mutually attracting each other. And it is further necessary to remember that this weight may be indefinitely reduced, without any diminution in the mass of the body weighed, by a mere change of its position in reference to the body between which and the body weighed the relation subsists.[1]

[1] The thoughtlessness with which it is assumed by some of the most eminent mathematicians and physicists that matter is composed of particles which have an absolute primordial weight persisting in all positions, and under all circumstances, is one of the most remarkable facts in the history of science. To cite but one instance: Prof. Rettenbacher, one of the ablest analysts of his day, in his "Dynamidensystem" (Mannheim, Bassermann, 1857), p. 14, says, "The absolute weight of atoms is unknown"—his meaning being, as is evident from the context and from the whole tenor of his discussion, that our ignorance of this absolute weight is due solely to the practical impossibility of insulating an atom, and of contriving instruments delicate enough to weigh it.

Masses find their true and only measure in the action of forces, and the quantitative persistence of the effect of this action is the simple and accurate expression of the fact which is ordinarily described as the indestructibility of matter. It is obvious that this persistence is in no sense explained or accounted for by the atomic hypothesis. It may be that such persistence is an attribute of the minute, insensible particles which are supposed to constitute matter, as well as of sensible masses; but, surely, the hypothetical recurrence of a fact in the atom is no explanation of the actual occurrence of the same fact in the conglomerate mass. Whatever mystery is involved in the phenomenon is as great in the case of the atom as in that of a solar or planetary sphere. Breaking a magnet into fragments, and showing that each fragment is endowed with the magnetic polarity of the integer magnet, is no explanation of the phenomenon of magnetism. A phenomenon is not explained by being dwarfed. A fact is not transformed into a theory by being looked at through an inverted telescope. The hypothesis of ultimate indestructible atoms is not a necessary implication of the persistence of weight, and can at best account for the indestructibility of matter if it can be shown that there is an absolute limit to the compressibility of matter—in other words, that there is an absolutely least volume for every determinate mass. This brings us to the consideration of that general property of matter which probably, in the minds of most men, most urgently requires the assumption of atoms—its impenetrability.

"Two bodies cannot occupy the same space"—such is the familiar statement of the fact in question. Like the indestructibility of matter, it is claimed to be a datum of experience. "*Corpora omnia impenetrabilia esse*," says Sir Isaac Newton (*Phil. Nat. Princ. Math.*, lib. iii., reg. 3), "*non ratione sed sensu colligimus.*" Let us see in what sense and to what extent this claim is legitimate.

The proposition, according to which a space occupied by one body cannot be occupied by another, implies the assumption that space is an absolute, self-measuring entity—an assumption which I may have occasion to examine hereafter—and the further assumption that there is a least space which a given body will absolutely fill so as to exclude any other body. A verification of this proposition by experience, therefore, must amount to proof that there is an absolute limit to the compressibility of all matter whatsoever. Now, does experience authorize us to assign such a limit? Assuredly not. It is true that in the case of solids and liquids there are practical limits beyond which compression by the mechanical means at our command is impossible; but even here we are met by the fact that the volumes of fluids, which effectually resist all efforts at further reduction by external pressure, are readily reduced by mere mixture. Thus, sulphuric acid and water at ordinary temperatures do not sensibly yield to pressure; but, when they are mixed, the resulting volume is materially less than the aggregate volumes of the liquids mixed. But, waiving this, as well as the phenomena which emerge in the processes of solution and chemical action, it must be said that experience does not in any manner vouch for the impenetrability of matter as such in all its states of aggregation. When gases are subjected to pressure, the result is simply an increase of the expansive force in proportion to the pressure exerted, according to the law of Boyle and Mariotte (the modifications of and apparent exceptions to which, as exhibited in the experimental results obtained by Regnault and others, need not here be stated, because they do not affect the argument). A definite experimental limit is reached in the case of those gases only in which the pressure produces liquefaction or solidification. The most significant phenomenon, however, which experience contributes to the testimony on this subject is the diffusion of gases. Whenever two or more gases which do not act upon each other chemically are introduced into a given space, each gas diffuses itself in this space as though it were alone present there; or, as Dalton, the reputed father of the modern atomic theory, expresses it, "Gases are mutually passive, and pass into each other as into vacua."

Whatever reality may correspond to the notion of the impenetrability of matter, this impenetrability is not, in the sense of the atomists, a datum of experience.

Upon the whole, it would seem that the validity of the first proposition of the atomic theory is not sustained by the facts. Even if the assumed unchangeability of the supposed ultimate constituent particles of matter presented itself, upon its own showing, as more than a bare reproduction of an observed fact in the form of an hypothesis, and could be dignified with the name of a generalization or of a theory, it would still be obnoxious to the criticism that it is a generalization from facts crudely observed and imperfectly apprehended.

In this connection it may be observed that the atomic theory has become next to valueless as an explanation of the impenetrability of matter, since it has been pressed into the service of the undulatory theory of light, heat, etc., and assumed the form in which it is now held by the majority of physicists, as we shall presently see. According to this form of the theory, the atoms are either mere points, wholly without extension, or their dimensions are infinitely small as compared with the distances between them, whatever be the state of aggregation of the substances into which they enter. In this view the resistance which a body, i. e., a system of atoms, offers to the intrusion of another body is due, not to the rigidity or unchangeability of volume of the individual atoms, but to the relation between the attractive and repulsive forces with which they are supposed to be endowed. There are physicists holding this view who are of opinion that the atomic constitution of matter is consistent with its impenetrability—among them M. Cauchy, who, in his *Sept Leçons de Phy-*

sique Générale (ed. Moigno, Paris, 1868, p. 38), after defining atoms as "material points without extension," uses this language: "Thus, this property of matter which we call impenetrability is explained, when we consider the atoms as material points exerting on each other attractions and repulsions which vary with the distances that separate them. . . . From this it follows that, if it pleased the author of Nature simply to modify the laws according to which the atoms attract or repel each other, we might instantly see the hardest bodies penetrate each other" (that we might see), "the smallest particles of matter occupy immense spaces, or the largest masses reduce themselves to the smallest volumes, the entire universe concentrating itself, as it were, in a single point."

2. The second fundamental proposition of the modern atomic theory avouches the essential discontinuity of matter. The advocates of the theory affirm that there is a series of physical phenomena which are inexplicable, unless we assume that the constituent particles of matter are separated by void interspaces. The most notable among these phenomena are the dispersion and polarization of light. The grounds upon which the assumption of a discrete molecular structure of matter is deemed indispensable for the explanation of these phenomena may be stated in a few words.

According to the undulatory theory, the dispersion of light, or its separation into spectral colors, by means of refraction, is a consequence of the unequal retardation experienced by the different waves, which produce the different colors, in their transmission through the refracting medium. This unequal retardation presupposes differences in the velocities with which the various-colored rays are transmitted through any medium whatever, and a dependence of these velocities upon the lengths of the waves. But, according to a well-established mechanical theorem, the velocities with which undulations are propagated through a continuous medium depend solely upon the elasticity of the medium as compared with its inertia, and are wholly independent of the length and form of the waves. The correctness of this theorem is attested by experience in the case of sound. Sounds of every pitch travel with the same velocity. If it were otherwise, music heard at a distance would evidently become chaotic; differences of velocity in the propagation of sound would entail a distortion of the rhythm, and, in many cases, a reversal of the order of succession. Now, differences of color are analogous to differences of pitch in sound, both reducing themselves to differences of wave-length. The lengths of the waves increase as we descend the scale of sounds from those of a higher to those of a lower pitch; and similarly, the length of a luminar undulation increases as we descend the spectral scale, from violet to red. It follows, then, that the rays of different color, like the sounds of different pitch, should be propagated with equal velocities, and be equally refracted; that, therefore, no dispersion of light should take place.

This theoretical impossibility of dispersion has always been recognized as one of the most formidable difficulties of the undulatory theory. In order to obviate it, Cauchy, at the suggestion of his friend Coriolis, entered upon a series of analytical investigations, in which he succeeded in showing that the velocities with which the various colored rays are propagated may vary according to the wave-lengths, if it be assumed that the ethereal medium of propagation, instead of being continuous, consists of particles separated by sensible distances.

By means of a similar assumption, Fresnel has sought to remove the difficulties presented by the phenomena of polarization. In ordinary light, the different undulations are supposed to take place in different directions, all transverse to the course or line of propagation, while in polarized light the vibrations, though still transverse to the ray, are parallelized, so as to occur in the same plane. Soon after this hypothesis had been expanded into an elaborate theory of polarization, Poisson observed that, at any considerable distance from the source of the light, all transverse vibrations in a continuous elastic medium must become longitudinal. As in the case of dispersion, this objection was met by the hypothesis of the existence of "definite intervals" between the ethereal particles.

These are the considerations, succinctly stated, which theoretical physics are supposed to bring to the support of the atomic theory. In reference to the cogency of the argument founded upon them, it is to be said, generally, that evidence of the discrete molecular arrangement of matter is by no means proof of the alternation of unchangeable and indivisible atoms with absolute spatial voids. But it is to be feared that the argument in question is not only formally, but also materially, fallacious. It is very questionable whether the assumption of definite intervals between the particles of the luminiferous ether is competent to relieve the undulatory theory of light from its embarrassments. This subject, in one of its aspects, has been thoroughly discussed by E. B. Hunt, in an article on the dispersion of light (*Silliman's Journal*, vol. vii., 2d series, p. 364, *et seq.*), and the suggestions there made appear to me worthy of serious attention. They are briefly these:

M. Cauchy brings the phenomena of dispersion within the dominion of the undulatory theory, by deducing the differences in the velocities of the several chromatic rays from the differences in the corresponding wave-lengths by means of the hypothesis of definite intervals between the particles of the light-bearing medium. He takes it for granted, therefore, that these chromatic rays are propagated with different velocities. But is this the fact? Astronomy affords the means to answer this question.

We experience the sensation of white light, when all the chromatic rays of which it is composed strike the eye simultaneously. The light proceeding from a luminous body will appear colorless, even if the component rays move with unequal velocities, provided all the colored rays, which together make up white light, concur in their action on the retina at a given moment; in ordinary cases it is immaterial whether these rays have left the luminous body successively or together. But it is otherwise when a luminous body becomes visible suddenly, as in the case of the satellites of Jupiter, or Saturn, after their eclipses. At certain periods, more than 49 minutes are requisite for the transmission of light from Jupiter to the earth. Now, at the moment when one of Jupiter's satellites, which has been eclipsed by that planet, emerges from the shadow, the red rays, if their velocity were the greatest, would evidently reach the eye first, the orange next, and so on through the chromatic scale, until finally the complement of colors would be filled by the arrival of the violet ray, whose velocity is supposed to be the least. The satellite, immediately after its emersion, would appear red, and gradually, in proportion to the arrival of the other rays, pass into white. Conversely, at the beginning of the eclipse, the violet rays would continue to arrive after the red and other intervening rays, and the satellite, up to the moment of its total disappearance, will gradually shade into violet.

Unfortunately for Cauchy's hypothesis, the most careful observation of the eclipses in question has failed to reveal any such variations of color, either before immersion, or after emersion, the transition between light and darkness taking place instantaneously, and without chromatic gradations.

If it be said that these chromatic gradations escape our vision by reason of the inappreciability of the differences under discussion, astronomy points to other phenomena no less subversive of the doctrine of unequal velocities in the movements of the chromatic undulations. Fixed stars beyond the parallactic limit, whose light must travel more than three years before it reaches us, are subject to great periodical variations of splendor; and yet these variations are unaccompanied by variations of color. Again, the assumption of different velocities for the different chromatic rays is discountenanced by the theory of aberration. Aberration is due to the fact that, in all cases where the orbit of the planet, on which the observer is stationed, forms an angle with the direction of the luminar ray, a composition takes place between the motion of the light and the motion of the planet, so that the direction in which the light meets the eye is a resultant of the two component directions—the direction of the ray and that of the observer's motion. If the several rays of color moved with different velocities there would evidently be several resultants, and each star would appear as a colored spectrum longitudinally parallel to the direction of the earth's motion.

The alleged dependence of the velocity of the undulatory movements, which correspond to, or produce, the different colors, upon the length of the waves, is thus at variance with observed fact. The hypothesis of definite intervals is unavailable as a supplement to the undulatory theory; other methods will have to be resorted to in order to free this theory from its difficulties.[1]

3. The third proposition of the atomic hypothesis assigns to the atoms, which are said to compose the different chemical elements, determinate weights corresponding to their equivalents of combination, and is supposed to be necessary to account for the facts whose enumeration and theory constitute the science of chemistry. The proper verification of these facts is of great difficulty, because they have generally been observed through the lenses of the atomic theory, and stated in its doctrinal terms. Thus the differentiation and integration of bodies are invariably described as decomposition and composition; the equivalents of combination are designated as atomic weights or volumes, and the greater part of chemical nomenclature is a systematic reproduction of the assumptions of atomism. Nearly all the facts to be verified are in need of preparatory enucleation from the envelops of this theory.

The phenomena usually described as chemical composition and decomposition present themselves to observation thus: A number of heterogeneous bodies concur in definite proportions of weight or volume; they interact; they disappear, and give rise to a new body possessing properties which are neither the sum nor the mean of the properties of the bodies concurring and interacting (excepting the weight which is the aggregate of the weights of the interacting bodies), and this conversion of several bodies into one is accompanied, in most cases, by changes of volume, and in all cases by the evolution or involution of heat, or light, or of both. Conversely, a single homogeneous body gives rise to heterogeneous bodies, between which and the body out of which they originate the persistence of weight is the only relation of identity.

For the sake of convenience, these phenomena may be distributed into three classes, of which the first embraces the persistence of weight and the combination in definite proportions; the second, the changes of volume and the evolution of light and heat; and the third, the emergence of a wholly new complement of chemical properties.

Obviously, the atomic hypothesis is in no sense an explanation of the phenomena of the second class. It is clearly and confessedly in-

[1] Cauchy's theory of dispersion is subject to another difficulty, of which no note is taken by Hunt: it does not account for the different refracting powers of different substances. Indeed, according to Cauchy's formulæ (whose terms are expressive simply of the distances between the ethereal particles and their hypothetical forces of attraction and repulsion), the refracting powers of all substances whatever must be the same, unless each substance is provided with a peculiar ether of its own. If this be the case, the assemblage of atoms in a given body is certainly a very motley affair, especially if it be true, as W. A. Norton and several other physicists assert, that there is an electric ether distinct from the luminiferous ether. Rettenbacher ("Dynamidensystem," p. 130, *et seq.*) attempts to overcome the difficulty by the hypothesis of mutual action between the corpuscular and ethereal atoms.

competent to account for changes of volume or of temperature. And, with the phenomena of the third class, it is apparently incompatible. For, in the light of the atomic hypothesis, chemical compositions and decompositions are in their nature nothing more than aggregations and segregations of masses whose integrity remains inviolate. But the radical change of chemical properties, which is the result of all true chemical action, and serves to distinguish it from mere mechanical mixture or separation, evinces a thorough destruction of that integrity. It may be that the appearance of this incompatibility can be obliterated by the device of ancillary hypotheses; but that leads to an abandonment of the simplicity of the atomic hypothesis itself, and thus to a surrender of its claims to merit as a theory.

At best, then, the hypothesis of atoms of definite and different weights can be offered as an explanation of the phenomena of the first class. Does it explain them in the sense of generalizing them, of reducing many facts to one? Not at all; it accounts for them, as it professed to account for the indestructibility and impenetrability of matter, by simply iterating the observed fact in the form of an hypothesis. It is another case (to borrow a scholastic phrase) of illustrating *idem per idem.* It says: The large masses combine in definitely-proportionate weights because the small masses, the atoms of which they are multiples, are of definitely-proportionate weight. It pulverizes the fact, and claims thereby to have sublimated it into a theory.

Upon closer examination, moreover, the assumption of atoms of different specific gravities proves to be, not only futile, but absurd. Its manifest theoretical ineptitude is found to mask the most fatal inconsistencies. According to the mechanical conception which underlies the whole atomic hypothesis, differences of weight are differences of density; and differences of density are differences of distance between the particles contained in a given space. Now, in the atom there is no multiplicity of particles, and no void space; hence differences of density or weight are impossible in the case of atoms.

It is to be observed that the attribution of different weights to different atoms is an indispensable feature of the atomic theory in chemistry, especially in view of the combination of gases in simple ratios of volume, so as to give rise to gaseous products bearing a simple ratio to the volumes of its constituents, and in view of the law of Ampère and Clausius, according to which all gases, of whatever nature or weight, contain equal numbers of molecules in equal volumes.

The inadequacy of the atomic hypothesis as a theory of chemical changes has been repeatedly pointed out by men of the highest scientific authority, such as Grove (*Correlation of Physical Forces*, in Youmans's "Correlation and Conservation of Forces," p. 164, *et seq.*), and is becoming more apparent from day to day. I shall have occasion to inquire, hereafter, what promise there is, in the present state of chemical science, of a true generalization of the phenomena of combination in definite proportions, both of weight and volume, which is independent of the atomic doctrine, and will serve to connect a number of concomitant facts for which this doctrine is utterly incompetent to account.

It is not infrequently asserted by the advocates of the atomic theory that there is a number of other phenomena, in addition to those of combination in definite proportions, which are strongly indicative of the truth of the atomic theory. Among these phenomena are isomerism, polymerism, and allotropy. But it is very doubtful whether this theory is countenanced by the phenomena in question. The existence of different allotropic states, in an elementary body said to consist of but one kind of atoms, is explicable by the atomic hypothesis in no other way than by deducing these different states from diversities in the grouping of the different atoms. But this explanation applies to solids only, and fails in the cases of liquids and gases. The same remark applies to isomerism and polymerism.

From the foregoing considerations, I take it to be clear that the atomic hypothesis mistakes many of the facts which it seeks to explain; that it accounts imperfectly or not at all for a number of other facts which are correctly apprehended; and that there are cases in which it appears to be in irreconcilable conflict with the data of experience. As a physical theory, it is barren and useless, inasmuch as it lacks the first requisite of a true theory—that of being a generalization, a reduction of several facts to one; it is essentially one of those spurious figments of the brain, based upon an ever-increasing *multiplicatio entium praeter necessitatem*, which are characteristic of the prescientific epochs of human intelligence, and against which the whole spirit of modern science is an emphatic protest. Moreover, in its logical and psychological aspect, as we shall hereafter see more clearly, it is the clumsiest attempt ever made to transcend the sphere of relations in which all objective reality, as well as all thought, has its being, and to grasp the absolute "*ens per sese, finitum, reale, totum.*"

I do not speak here of a number of other difficulties which emerge upon a minute examination of the atomic hypothesis in its two principal varieties, the atoms being regarded by some physicists as extended and figured masses, and by others as mere centres of force. In the former case the assumption of physical indivisibility becomes gratuitous, and that of mathematical indivisibility absurd; while in the latter case the whole basis of the relation between force and mass, or rather force and inertia, without which the conception of either term of the relation is impossible, is destroyed. Some of these difficulties are frankly admitted by leading men of science—for instance, by Du Bois-Reymond, in the lecture above cited. Nevertheless, it is asserted that the atomic, or at least molecular, constitution of matter is the only form of material existence which can be realized in thought. In what sense, and to what extent, this assertion is well founded, will be my next subject of examination.

FINDING THE WAY AT SEA.

By R. A. PROCTOR.

THE wreck of the Atlantic, followed closely by that of the City of Washington nearly on the same spot, has led many to inquire into the circumstances on which depends a captain's knowledge of the position of his ship. In each case, though not in the same way, the ship was supposed to be far from land, when in reality quite close to it. In each case, in fact, the ship had oversailed her reckoning. A slight exaggeration of what travellers so much desire—a rapid passage—proved the destruction of the ship, and in one case occasioned a fearful loss of life. And, although such events are fortunately infrequent in Atlantic voyages, yet the bare possibility that, besides ordinary sea-risks, a ship is exposed to danger from simply losing her way, suggests unpleasant apprehensions as to the general reliability of the methods in use for determining where a ship is, and her progress from day to day.

I propose to give a brief sketch of the methods in use for finding the way at sea, in order that the general principles on which safety depends may be recognized by the general reader.

It is known, of course, to every one, that a ship's course and rate of sailing are carefully noted throughout her voyage. Every change of her course is taken account of, as well as every change in her rate of advance, whether under sail or steam, or both combined. If all this could be quite accurately managed, the position of the ship at any hour could be known, because it would be easy to mark down on a chart the successive stages of her journey, from the moment when she left port. But a variety of circumstances renders this impossible.

To begin with: the *exact* course of a ship cannot be known, because there is only the ship's compass to determine her course by, and a ship's compass is not an instrument affording perfectly exact indications. Let any one on a sea-voyage observe the compass for a short time, being careful not to break the good old rule which forbids speech to the "man at the wheel," and he will presently become aware of the fact that the ship is not kept rigidly to one course, even for a short time. The steersman keeps her as near as he can to a particular course, but she is continually deviating, now a little on one side, now a little on the other, of the intended direction; and even the general accuracy with which that course is followed is a matter of estimation, and depends on the skill of the individual steersman. Looking at the compass-card, in steady weather, a course may seem very closely followed; perhaps the needle's end may not be a hundredth part of an inch (on the average) from the position it should have. But a hundredth part of an inch on the circumference of the compass-card would correspond to a considerable deviation in the course of a run of twenty or thirty knots; and there is nothing to prevent the errors so arising from accumulating in a long journey until a ship might be thirty or forty miles from her estimated place. To this may be added the circumstance that the direction of the needle is different in different parts of the earth. In some places it points to the east of the north, in others to the west. And, although the actual "variation of the compass," as this peculiarity is called, is known in a general way for all parts of the earth, yet such knowledge has no claim to actual exactness. There is also an important danger, as recent instances have shown, in the possible change of the position of the ship's compass, on account of iron in her cargo.

But a far more important cause of error, in determinations merely depending on the log-book, is that arising from uncertainty as to the ship's rate of progress. The log-line gives only a rough idea of the ship's rate at the time when the log is cast;[1] and, of course, a ship's rate does not remain constant, even when she is under steam alone. Then, again, currents carry the ship along sometimes with considerable rapidity; and the log-line affords no indication of their action: while no reliance can be placed on the estimated rates, even of known currents. Thus the distance made on any course may differ considerably from the estimated distance; and, when several days' sailing are dealt with, an error of large amount may readily accumulate.

For these and other reasons, a ship's captain places little reliance on what is called "the day's work"—that is, the change in the ship's position from noon to noon as estimated from the compass-courses entered in the log-book, and the distances supposed to be run on these courses. It is absolutely essential that such estimates should be carefully made, because, under favorable conditions of weather, there may be no other means of guessing at the ship's position. But the only really reliable way of determining a ship's place is by astronomical observations. It is on this account that the almanac published by the Admiralty, in which the position and apparent motions of the celestial bodies are indicated, four or five years in advance, is called, *par excel-*

[1] The log is a flat piece of wood of quadrantal shape, so loaded at the rim as to float with the point (that is, the centre of the quadrant) uppermost. To this a line about 300 yards long is fastened. The log is thrown overboard, and comes almost immediately to rest on the surface of the sea, the line being suffered to run freely out. By marks on the log-line divided into equal spaces, called *knots*, of known length, and by observing how many of these run out, while the sand in a half-minute hour-glass is running, the ship's rate of motion is roughly inferred. The whole process is necessarily rough, since the line cannot even be straightened.

lence, the *Nautical Almanac.* The astronomer, in his fixed observatory, finds this almanac essential to the prosecution of his observations; the student of theoretical astronomy has continual occasion to refer to it; but, to the sea-captain, the *Nautical Almanac* has a far more important use. The lives of sailors and passengers are dependent upon its accuracy. It is, again, chiefly for the sailor that our great nautical observatories have been erected, and that our astronomer-royal and his officers are engaged. What other work they may do is subsidiary, and, as it were, incidental. Their chief work is to time this great clock, our earth, and so to trace the motions of those celestial indices, which afford our fundamental time-measures, as to insure as far as possible the safety of our navy, royal and mercantile.[1]

Let us see how this is brought about, not, indeed, by inquiring into the processes by which, at the Greenwich Observatory, the elements of safety are obtained, but by considering the method by which a seaman makes use of these elements.

In the measures heretofore considered, the captain of a ship in reality relies on terrestrial measurements. He reasons that, being on such and such a day in a given place, and having in the interval sailed so many miles in such and such directions, he must at the time being be in such and such a place. This is called "navigation." In the processes next to be considered, which constitute a part of the science of nautical astronomy, the seaman trusts to celestial observations independently of all terrestrial measurements.

The points to be determined by the voyager are his latitude and longitude. The latitude is the distance north or south of the equator, and is measured always from the equator in degrees, the distance from equator to pole being divided into ninety equal parts, each of which is a degree.[2] The longitude is the distance east or west of Greenwich (in English usage, but other nations employ a different starting-point for measuring longitudes from). Longitude is not measured in miles, but in degrees. The way of measuring is not very

[1] This consideration has been altogether lost sight of in certain recent propositions for extending government aid to astronomical inquiries of another sort. It may be a most desirable thing that government should find means for inquiring into the physical condition of sun and moon, planets and comets, stars and all the various orders of star-clusters. But, if such matters are to be studied at government expense, it should be understood that the inquiry is undertaken with the sole purpose of advancing our knowledge of these interesting subjects, and should not be brought into comparison with the utilitarian labors for which our Royal Observatory was founded.

[2] Throughout this explanation all minuter details are neglected. In reality, in consequence of the flattening of the earth's globe, the degrees of latitude are not equal, being larger the farther we go from the equator. Moreover, strictly speaking, it is incorrect to speak of distances being divided into degrees, or to say that a degree of latitude or longitude contains so many miles; yet it is so exceedingly inconvenient to employ any other way of speaking in popular description, that I trust any astronomers or mathematicians who may read this article will forgive the solecism.

readily explained without a globe or diagrams, but may be thus indicated: Suppose a circle to run completely round the earth, through Greenwich and both the poles; now, if this circle be supposed free to turn upon the polar axis, or on the poles as pivots, and the half which crosses Greenwich be carried (the nearest way round) till it crosses some other station, then the arc through which it is carried is called the longitude of the station, and the longitude is easterly or westerly according as this half-circle has to be shifted toward the east or west. A complete half-turn is 180°, and, by taking such a half-turn either eastwardly or westwardly, the whole surface of the earth is included. Points which are 180° east of Greenwich are thus also 180° west of Greenwich.

So much is premised in the way of explanation to make the present paper complete; but ten minutes' inspection of an ordinary terrestrial globe will show the true meaning of latitude and longitude more clearly (to those who happen to have forgotten what they learned at school on these points) than any verbal description.

Now, it is sufficiently easy for a sea-captain in fine weather to determine his latitude. For places in different latitudes have different celestial scenery, if one may so describe the aspect of the stellar heavens by night and the course traversed by the sun by day. The height of the pole-star above the horizon, for instance, at once indicates the latitude very closely, and would indicate the latitude exactly if the pole-star were exactly at the pole instead of being merely close to it. But the height of any known star when due south also gives the latitude. For, at every place in a given latitude, a star rises to a given greatest height when due south; if we travel farther south, the star will be higher when due south; if we travel farther north, it will be lower; and thus its observed height shows just how far north of the equator any northerly station is, while, if the traveller is in the Southern Hemisphere, corresponding observations show how far to the south of the equator he is.

But commonly the seaman trusts to observation of the sun to give him his latitude. The observation is made at noon, when the sun is highest above the horizon. The actual height is determined by means of the instrument called the sextant. This instrument need not be here described; but thus much may be mentioned to explain that process of taking the sun's meridian altitude which, no doubt, every one has witnessed who has taken a long sea-journey. The sextant is so devised that the observer can see two objects at once, one directly and the other after reflection of its light; and the amount by which he has to move a certain bar carrying the reflecting arrangement, in order to bring the two objects into view in the same direction, shows him the real divergence of lines drawn from his eye to the two objects. To take the sun's altitude, then, with this instrument, the observer takes the sun as one object and the horizon directly below the sun as the other: he brings them into view together, and then, looking at the sextant to see how much he has had to move the swinging arm which carries the reflecting glasses, he learns how high the sun is. This being done at noon, with proper arrangements to insure that the greatest height then reached by the sun is observed, at once indicates the latitude of the observer. Suppose, for example, he finds the sun to be 40° above the horizon, and the *Nautical Almanac* tells him that, at the time the sun is 10° north of the celestial equator, then he knows that the celestial equator is 30° above the southern horizon. The pole of the heavens is, therefore, 60° above the northern horizon, and the voyager is in 60° north latitude. Of course, in all ordinary cases, the number of degrees is not exact, as I have here for simplicity supposed, and there are some niceties of observation which would have to be taken into account in real work. But the principle of the method is sufficiently indicated by what has been said, and no useful purpose could be served by considering minutiæ.

Unfortunately, the longitude is not determined so readily. The very circumstance which makes the determination of the latitude so simple introduces the great difficulty which exists in finding the longitude. I have said that all places in the same latitude have the same celestial scenery; and precisely for this reason it is difficult to distinguish one such place from another, that is, to find on what part of its particular latitude-circle any place may lie.

If we consider, however, how longitude is measured, and what it really means, we shall readily see where a solution of the difficulty is to be sought. The latitude of a station means how far toward either pole the station is; its longitude means how far *round* the station is from some fixed longitude. But it is by turning round on her axis that the earth causes the changes which we call day and night; and therefore these must happen at different times in places at different distances round. For example, it is clear that, if it is noon at one station, it must be midnight at a station half-way round from the former. And if any one at one station could telegraph to a person at another, "It is exactly noon here," while this latter person knew from his clock or watch that it was exactly midnight where *he* was, then he would know that he was half-way round exactly. He would, in fact, know his longitude from the other station. And so with smaller differences. The earth turns, we know, from west to east—that is, a place lying due west of another is so carried as presently to occupy the place which its easterly neighbor had before occupied, while this last place has gone farther east yet. Let us suppose an hour is the time required to carry a westerly station to the position which had been occupied by a station to the east of it. Then manifestly every celestial phenomenon depending on the earth's turning will occur an hour later at the westerly station. Sunrise and sunset are phenomena of this kind. If I telegraph to a friend at some station far to the west, but in the same latitude, "The sun is rising here," and he finds that he has to wait exactly an hour before the sun rises there, then he knows that he is one hour west of me in longitude, a most inexact yet very convenient and unmistakable way of speaking. As there are twenty-four hours in the day, while a complete circle running through my station and his (and everywhere in the same latitude) is supposed to be divided into 360°, he is 15° (a 24th part of 360) west of me; and, if my station is Greenwich, he is in what we, in England, call 15° west longitude.[1]

But what is true of sunrise and sunset in the same latitudes and different longitudes, is true of noon whatever the latitude may be. And of course it is true of the southing of any known star. Only unfortunately one cannot tell the exact instant when either the sun or a star is due south or at its highest above the horizon. Still, speaking generally, and for the moment limiting our attention to noon, every station toward the west has noon later, while every station toward the east has noon earlier, than Greenwich (or whatever reference station is employed).

I shall presently return to the question how the longitude is to be determined with sufficient exactness for safety in sea-voyages. But I may digress here to note what happens in sea-voyages where the longitude changes. If a voyage is made toward the west, as from England to America, it is manifest that a watch set to Greenwich time will be in advance of the local time as the ship proceeds westward, and will be more and more in advance the farther the ship travels in that direction. For instance, suppose a watch shows Greenwich time; then when it is noon at Greenwich the watch will point to twelve, but it will be an hour before noon at a place 15° west of Greenwich, two hours before noon at a place 30° west, and so on: that is, the watch will point to twelve when it is only eleven o'clock, ten o'clock, and so on, of local time. On arrival at New York, the traveller would find that his watch was nearly five hours fast. Of course the reverse happens in a voyage toward the east. For instance, a watch set to New-York time would be found to be nearly five hours slow, for Greenwich time, when the traveller arrived in England.

In the following passage these effects are humorously illustrated by Mark Twain:

"Young Mr. Blucher, who is from the Far West, and on his first voyage" (from New York to Europe) "was a good deal worried by the constantly-changing 'ship-time.' He was proud of his new watch at first, and used to drag it out promptly when eight bells struck at noon, but he came to look after a while as if he were losing confi-

[1] In this case, he is "at sea" (which, I trust, will not be the case with the reader), and, we may suppose, connected with Greenwich by submarine telegraph in course of being laid. In fact, the position of the Great Eastern throughout her cable-laying journeys, was determined by a method analogous to that sketched above.

dence in it. Seven days out from New York he came on deck, and said with great decision, 'This thing's a swindle!' 'What's a swindle?' 'Why, this watch. I bought her out in Illinois—gave $150 for her, and I thought she was good. And, by George, she *is* good on shore, but somehow she don't keep up her lick here on the water—gets sea-sick, may be. She skips; she runs along regular enough, till half-past eleven, and then all of a sudden she lets down. I've set that old regulator up faster and faster, till I've shoved it clear round, but it don't do any good; she just distances every watch in the ship,[1] and clatters along in a way that's astonishing till it's noon, but them "eight bells" always gets in about ten minutes ahead of her any way. I don't know what to do with her now. She's doing all she can; she's going her best gait, but it won't save her. Now, don't you know there ain't a watch in the ship that's making better time than she is; but what does it signify? When you hear them "eight bells," you'll find her just ten minutes short of her score—sure.' The ship was gaining a full hour every three days, and this fellow was trying to make his watch go fast enough to keep up to her. But, as he had said, he had pushed the regulator up as far as it would go, and the watch was 'on its best gait,' and so nothing was left him but to fold his hands and see the ship beat in the race. We sent him to the captain, and he explained to him the mystery of 'ship-time,' and set his troubled mind at rest. This young man," proceeds Mr. Clemens, *à propos des bottes*, "had asked a great many questions about sea-sickness before we left, and wanted to know what its characteristics were, and how he was to tell when he had it. He found out."

I cannot leave Mark Twain's narrative, however, without gently criticising a passage in which he has allowed his imagination to invent effects of longitude which assuredly were never perceived in any voyage since the ship Argo set out after the Golden Fleece. "We had the phenomenon of a full moon," he says, "located just in the same spot in the heavens, at the same hour every night. The reason of this singular conduct on the part of the moon did not occur to us at first, but it did afterward, when we reflected that we were gaining about twenty minutes every day; because we were going east so fast, we gained just about enough every day to keep along with the moon. It was becoming an old moon to the friends we had left behind us, but to us Joshuas it stood still in the same place, and remained always the same." O Mr. Clemens, Mr. Clemens! In a work of imagination (as the "Innocents Abroad" must, I suppose, be to a great extent considered), a mistake such as that here made is perhaps not a very serious matter; but, suppose some unfortunate compiler of astronomical works should happen to remember this passage, and to state (as a

[1] Because *set* to go "fast." Of course, the other watches on board would be left to go at their usual rate, and simply put forward at noon each day by so many minutes as corresponded to the run eastward since the preceding noon.

compiler would be tolerably sure to do, unless he had a mathematical friend at his elbow) that, by voyaging eastward at such and such a rate, a traveller can always have the moon "full" at night, in what an unpleasant predicament would the mistake have placed him! Such things happen, unfortunately; nay, I have even seen works, in which precisely such mistakes have been made, in use positively as text-books for examinations. On this account, our fiction writers must be careful in introducing science details, lest peradventure science-teachers (save the mark!) be led astray.

It need scarcely be said that no amount of eastwardly voyaging would cause the moon to remain always "full" as seen by the voyager. The moon's phase is the same from whatever part of the earth she may be seen, and she will become "new," that is, pass between the earth and the sun, no matter what voyages may be undertaken by the inhabitants of earth. Mr. Clemens has confounded the monthly motion of the moon with her daily motion. A traveller who could only go fast enough eastward might keep the moon always due south. To do this he would have to travel completely round the earth in a day and (roughly) about 50½ minutes. If he continued this for a whole month, the moon would never leave the southern heavens; but she would not continue "full." In fact, we see that the hour of the day (local time) would be continually changing—since the traveller would not go round once in twenty-four hours (which would be following the sun, and would cause the hour of the day to remain always the same), but in twenty-four hours and the best part of another hour; so that the day would seem to pass on, though very slowly, lasting a lunar month instead of a common day.

Every one who makes a long sea-voyage must have noted the importance attached to moon observations; and many are misled into the supposition that these observations are directly intended for the determination of the longitude (or, which is the same thing in effect, for determining true ship-time). This, however, is a mistake. The latitude can be determined at noon, as we have seen. A rough approximation to the local time can be obtained also, and is commonly obtained, by noting when the sun begins to dip after reaching the highest part of his course above the horizon. But this is necessarily *only* a rough approximation, and quite unsuited for determining the ship's longitude. For the sun's elevation changes very slowly at noon, and no dip can be certainly recognized, even from *terra firma*, far less from a ship, within a few minutes of true noon. A determination of time effected in this way serves very well for the ship's "watches," and accordingly when the sun, so observed, begins to dip, they strike "eight bells" and "make it noon." But it would be a serious matter for the crew if that was made the noon for working the ship's place; for an error of many miles would be inevitable.

The following passage from "Foul Play" illustrates the way in which mistakes have arisen on this point: The hero, who, being a clergyman and a university man, is, of course, a master of every branch of science, is about to distinguish himself before the heroine by working out the position of the ship Proserpine, whose captain is senselessly drunk. After ten days' murky weather, "the sky suddenly cleared, and a rare opportunity occurred to take an observation. Hazel suggested to Wylie, the mate, the propriety of taking advantage of the moment, as the fog-bank out of which they had just emerged would soon envelop them again, and they had not more than an hour or so of such observation available. The man gave a shuffling answer. So he sought the captain in his cabin. He found him in bed. He was dead drunk. On a shelf lay the instruments. These Hazel took, and then looked round for the chronometers. They were safely locked in their cases. He carried the instruments on deck, together with a book of tables, and quietly began to make preparations, at which Wylie, arresting his walk, gazed with utter astonishment" (as well he might).

"'Now, Mr. Wylie, I want the key of the chronometer-cases.'

"'Here is a chronometer, Mr. Hazel,' said Helen, very innocently, 'if that is all you want.'

"Hazel smiled, and explained that a ship's clock is made to keep the most exact time; that he did not require the time of the spot where they were, but Greenwich time. He took the watch, however. It was a large one for a lady to carry; but it was one of Frodsham's masterpieces.

"'Why, Miss Rolleston,' said he, 'this watch must be two hours slow. It marks ten o'clock; it is now nearly mid-day. Ah, I see,' he added, with a smile, 'you have wound it regularly every day, but you have forgotten to set it daily. Indeed, you may be right; it would be a useless trouble, since we change our longitude hourly. Well, let us suppose that this watch shows the exact time at Sydney, as I presume it does, I can work the ship's reckoning from that meridian, instead of that of Greenwich.' And he set about doing it." Wylie, after some angry words with Hazel, brings the chronometers and the charts. Hazel "verified Miss Rolleston's chronometer, and, allowing for difference of time, found it to be accurate. He returned it to her, and proceeded to work on the chart. The men looked on; so did Wylie. After a few moments, Hazel read as follows: 'West longitude 146° 53′ 18″. South latitude 35° 24′. The island of Oparo [1] and the Four Crowns distant 420 miles on the N. N. E.,'" and so on. And, of course, "Miss Rolleston fixed her large, soft eyes on the young clergyman with the undisguised admiration a woman is apt to feel for what she does not understand."

[1] The island fixes the longitude at about 147°, otherwise I should have thought the 4 was a misprint for 7. In longitude 177° west, Sydney time would be about 2 hours slow, but about 4 hours slow in longitude 147° west.

The scene here described corresponds pretty closely, I have little doubt, with one actually witnessed by the novelist, except only that the captain or chief officer made the observations, and that either there had not been ten days' murky weather, or else that in the forenoon, several hours at least before noon, an observation of the sun had been made. The noon observation would give the latitude, and, combined with a forenoon observation, would give the longitude, but *alone* would be practically useless for that purpose. It is curious that the novelist sets the longitude as assigned much more closely than the latitude, and the value given would imply that the ship's time was known within less than a second. This would in any case be impracticable; but, from noon observations, the time could not be learned within a minute at the least. The real fact is, that, to determine true time, the seaman selects, not noon, as is commonly supposed, but a time when the sun is nearly due east or due west. For then the sun's elevation changes most rapidly, and so gives the surest means of determining the time. The reader can easily see the *rationale* of this by considering the case of an ordinary clock-hand. Suppose our only means of telling the time was by noting how high the end of the minute-hand was: then, clearly, we should be apt to make a greater mistake in estimating the time, when the hand was near XII., than at any other time, because then its end changes very slowly in height, and a minute more or less makes very little difference. On the contrary, when the hand was near III. and IX., we could in a very few seconds note any change of the height of its extremity. In one case we could not tell the time within a minute or two; in the other, we could tell it within a few seconds.

But the noon observation would be wanted to complete the determination of the longitude; for, until the latitude was known, the captain would not be aware what apparent path the sun was describing in the heavens, and therefore would not know the time corresponding to any particular solar observation. So that a passenger, curious in watching the captain's work, would be apt to infer that the noon observations gave the longitude, since he would perceive that from them the captain worked out both the longitude and the latitude.

It is curious that another and critical portion of the same entertaining novel is affected by the mistake of the novelist on this subject. After the scuttling of the Proserpine, and other events, Hazel and Miss Rolleston are alone on an island in the Pacific. Hazel seeks to determine their position, as one step toward escape. Now, "you must know that Hazel, as he lay on his back in the boat, had often, in a half-drowsy way, watched the effect of the sun upon the boat's mast: it now stood, a bare pole, and at certain hours acted like the needle of a dial by casting a shadow on the sands. Above all, he could see pretty well, by means of this pole and its shadow, when the sun attained its greatest elevation. He now asked Miss Rolleston to assist him in making this observation exactly. She obeyed his instructions, and, the moment the shadow reached its highest angle and showed the minutest symptom of declension, she said 'Now,' and Hazel called out in a loud voice" (why did he do that?) "'Noon!' 'And forty nine minutes past eight at Sydney,' said Helen, holding out her chronometer; for she had been sharp enough to get it ready of her own accord. Hazel looked at her and at the watch with amazement and incredulity. 'What?' said he. 'Impossible! You can't have kept Sydney time all this while.' 'And pray why not?' said Helen. 'Have you forgotten that some one praised me for keeping Sydney time? it helped you somehow or other to know where we were.'" After some discussion, in which she shows how natural it was that she should have wound up her watch every night, even when "neither of them expected to see the morning," she asks to be praised. "'Praised!' cried Hazel, excitedly, 'worshipped, you mean. Why, we have got the longitude by means of your chronometer. It is wonderful! It is providential. It is the finger of Heaven. Pen and ink, and let me work it out.'" He was "soon busy calculating the longitude of Godsend Island." What follows is even more curiously erroneous. "'There,' said he. 'Now, the latitude I must guess at by certain combinations. In the first place the slight variation in the length of the days. Then I must try and make a rough calculation of the sun's parallax.'" (It would have been equally to the purpose to have calculated how many cows' tails would reach to the moon.) "'And then my botany will help me a little; spices furnish a clew; there are one or two that will not grow outside the tropic,'" and so on. He finally sets the latitude between the 26th and 33d parallels, a range of nearly 500 miles. The longitude, however, which is much more closely assigned, is wrong altogether, being set at $103\frac{1}{2}°$ *west*, as the rest of the story requires. For Godsend Island is within not many days' sail of Valparaiso. The mistake has probably arisen from setting Sydney in *west* longitude instead of *east* longitude, 151° 14′; for the difference of time, 3h. 11m., corresponds within a minute to the difference of longitude between 151° 14′ west and $103\frac{1}{2}°$ west.

Mere mistakes of calculation, however, matter little in such cases. They do not affect the interest of a story even in such extreme cases as in "Ivanhoe," where a full century is dropped in such sort that one of Richard I.'s knights holds converse with a contemporary of the Conqueror, who, if my memory deceives me not, was Cœur de Lion's great-great-grandfather. It is a pity, however, that a novelist or indeed any writer should attempt to sketch scientific *methods* with which he is not familiar. No discredit can attach to any person, not an astronomer, who does not understand the astronomical processes for determining latitude and longitude, any more than to one who, not being a lawyer, is unfamiliar with the rules of conveyancing. But, when an attempt is made by a writer of fiction to give an exact description of any technical matter, it is as well to secure correctness by submitting the description to some friend acquainted with the principles of the subject. For, singularly enough, people pay much more attention to these descriptions when met with in novels, than when given in text-books of science, and they thus come to remember thoroughly well precisely what they ought to forget. I think, for instance, that it may not improbably have been some recollection of "Foul Play" which led Mr. Lockyer to make the surprising statement that longitude is determined at sea by comparing chronometer time with local time, which is found "at noon by observing, with the aid of a sextant, when the sun is at the highest point of its path." Our novelists really must not lead the students of astronomy astray in this manner.

It will be clear to the reader, by this time, that the great point in determining the longitude is, to have the true time of Greenwich or some other reference station, in order that, by comparing this time with ship-time, the longitude east or west of the reference station may be ascertained. Ship-time can always be determined by a morning or afternoon observation of the sun, or by observing a known star when toward the east or west, at which time the diurnal motion raises or depresses it most rapidly. The latitude being known, the time of day (any given day) at which the sun or a star should have any particular altitude is known also, and, therefore, conversely, when the altitude of the sun or a star has been noted, the seaman has learned the time of day. But to find Greenwich time is another matter; and, without Greenwich time, ship-time teaches nothing as to the longitude. How is the voyager at sea or in desert places to know the exact time at Greenwich or some other fixed station? We have seen that chronometers are used for this purpose; and chronometers are now made so marvellously perfect in construction that they can be trusted to show true time within a few seconds, under ordinary conditions. But it must not be overlooked that in long voyages a chronometer, however perfect its construction, is more liable to get wrong than at a fixed station. That it is continually tossed and shaken is something, but is not the chief trial to which it is exposed. The great changes of temperature endured, when a ship passes from the temperate latitudes across the torrid zone to the temperate zone again, try a chronometer far more severely than any ordinary form of motion. And then it is to be noted that a very insignificant time-error corresponds to a difference of longitude quite sufficient to occasion a serious error in the ship's estimated position. For this reason and for others, it is desirable to have some means of determining Greenwich time independently of chronometers.

This, in fact, is the famous problem for the solution of which such high rewards were offered and have been given.[1] It was to solve this

[1] For invention of the chronometer, Harrison (a Yorkshire carpenter, and the son of

problem that Whiston, the same who fondly imagined Newton was afraid of him,[1] suggested the use of bombs and mortars; for which Hogarth pilloried him in the celebrated mad-house scene of the Rake's Progress. Of course Whiston had perceived the essential feature of all methods intended for determining the longitude. Any signal which is *recognizable*, no matter by eye or ear, or in whatsoever way, at both stations, the reference station and the station whose longitude is required, must necessarily suffice to convey the time of one station to the other. The absurdity of Whiston's scheme lay in the implied supposition that any form of ordnance could propel rocket-signals far enough to be seen or heard in mid-ocean. Manifestly the only signals available, when telegraphic communication is impossible, are signals in the celestial spaces, for these alone can be discerned simultaneously from widely-distant parts of the earth. It has been to such signals, then, that men of science have turned for the required means of determining longitude.

Galileo was the first to point out that the satellites of Jupiter supply a series of signals which might serve to determine the longitude. When one of these bodies is eclipsed in Jupiter's shadow, or passes out of sight behind Jupiter's disk, or reappears from eclipse or occultation, the phenomenon is one which can be seen from a whole hemisphere of the earth's surface. It is as truly a signal as the appearance or disappearance of a light in ordinary night-signalling. If it can be calculated beforehand that one of these events will take place at any given hour of Greenwich time, then, from whatever spot the phenomenon is observed, it is known there that the Greenwich hour is that indicated. Theoretically, this is a solution of the famous problem; and Galileo, the discoverer of Jupiter's four satellites, thought he had found the means of determining the longitude with great accuracy. Unfortunately, these hopes have not been realized. At sea, indeed, except in the calmest weather, it is impossible to observe the phenomena of Jupiter's satellites, simply because the telescope cannot be directed steadily upon the planet. But even on land Jupiter's satellites afford but imperfect means of guessing at the longitude. For, at present, their motions have not been thoroughly mastered by astronomers, and though the *Nautical Almanac* gives the estimated epochs for the various phenomena of the four satellites,

a carpenter) received £20,000. This sum had been offered for a marine chronometer which would stand the test of two voyages of assigned length. Harrison labored fifty years before he succeeded in meeting the required condition.

[1] Newton, for excellent reasons, had opposed Whiston's election to the Royal Society. Like most small men, Whiston was eager to secure a distinction which, unless spontaneously offered to him, could have conferred no real honor. Accordingly he was amusingly indignant with Newton for opposing him. "Newton perceived," he wrote, "that I could not do as his other darling friends did, that is, learn of him without contradicting him when I differed in opinion from him: he could not in his old age bear such contradiction, and so he was afraid of me the last thirteen years of his life."

yet, owing to the imperfection of the tables, these epochs are often found to be appreciably in error. There is yet another difficulty. The satellites are not mere points, but, being in reality also as large as or larger than our moon, they have disks of appreciable though small dimensions. Accordingly, they do not vanish or reappear instantaneously, but gradually, the process lasting in reality several seconds (a longer or shorter time, according to the particular satellites considered), and the estimated moment of the phenomenon thus comes to depend on the power of the telescope employed, or the skill or the visual powers of the observer, or the condition of the atmosphere, and so on. Accordingly, very little reliance could be placed on such observations as a mean for determining the longitude with any considerable degree of exactness.

No other celestial phenomena present themselves except those depending on the moon's motions.[1] All the planets, as well as the sun and moon, traverse at various rates and in different paths the sphere of the fixed stars. But the moon alone moves with sufficient

[1] If but one star or a few would periodically (and quite regularly) "go out" for a few moments, the intervals between such vanishings being long enough to insure that one would not be mistaken in point of time for the next or following one, then it would be possible to determine Greenwich or other reference time with great exactness. And here one cannot but recognize an argument against the singular theory that the stars were intended simply as lights to adorn our heavens and to be of use to mankind. The teleologists who have adopted this strange view can hardly show how the theory is consistent with the fact that quite readily the stars (or a few of them) might have been so contrived as to give man the means of travelling with much more security over the length and breadth of his domain than is at present possible. In this connection I venture to quote a passage in which Sir John Herschel has touched on the *usefulness* of the stars, in terms which, were they not corrected by other and better-known passages in his writings, might suggest that he had adopted the theory I have just mentioned: "The stars," he said, in an address to the Astronomical Society, in 1827, "are landmarks of the universe; and, amid the endless and complicated fluctuations of our system, seem placed by its Creator as guides and records, not merely to elevate our minds by the contemplation of what is vast, but to teach us to direct our actions by reference to what is immutable in his works. It is indeed hardly possible to over-appreciate their value in this point of view. Every well-determined star, from the moment its place is registered, becomes to the astronomer, the geographer, the navigator, the surveyor, a point of departure which can never deceive or fail him—the same forever and in all places, of a delicacy so extreme as to be a test for every instrument yet invented by man, yet equally adapted for the most ordinary purposes; as available for regulating a town-clock as for conducting a navy to the Indies; as effective for mapping down the intricacies of a petty barony as for adjusting the boundaries of transatlantic empires. When once its place has been thoroughly ascertained, and carefully recorded, the brazen circle with which the useful work was done may moulder, the marble pillar may totter on its base, and the astronomer himself survive only in the gratitude of posterity; but the record remains, and transfuses all its own exactness into every determination which takes it for a groundwork, giving to inferior instruments, nay, even to temporary contrivances, and to the observations of a few weeks or days, all the precision attained originally at the cost of so much time, labor, and expense." It is only necessary, as a corrective to the erroneous ideas which might otherwise be suggested by this somewhat high-flown passage, to quote the following remarks from the work which represented Sir John Her-

rapidity to act as a time indicator for terrestrial voyagers. It is hardly necessary to explain why rapidity of motion is important; but the following illustration may be given for the purpose. The hour-hand of a clock does in reality indicate the minute as well as the hour; yet, owing to the slowness of its motion, we regard the hour-hand as an unsatisfactory time-indicator, and only consider it as showing what hour is in progress. So with the more slowly-moving celestial bodies. They would serve well enough, at least some among them would, to show the *day of the year*, if we could only imagine that such information were ever required from celestial bodies. But it would be hopeless to attempt to ascertain the true time with any degree of accuracy from their motions. Now, the moon really moves with considerable rapidity among the stars.[1] She completes the circuit of the celestial sphere in 27⅓ days (a period less than the common lunation), so that in one day she traverses about 13°, or her own diameter (which is rather more than half a degree), in about an hour. This, astronomically speaking, is very rapid motion; and, as it can be detected in a few seconds by telescopic comparison of the moon's place with that of some fixed star, it serves to show the time within a few seconds, which is precisely what is required by the seaman. Theoretically, all he has to do is, to take the moon's apparent distance from a known star, and also her height and the star's height above the horizon. Thence he can calculate what would be the moon's distance from the star at the moment of observation, if the observer were at the earth's centre. But the *Nautical Almanac* informs him of the precise instant of Greenwich time corresponding to this calculated distance. So he has, what he requires, the true Greenwich time.

It will be manifest that all methods of finding the way at sea, except the rough processes depending on the log and compass, require that the celestial bodies, or some of them, should be seen. Hence it is that cloudy weather, for any considerable length of time, occasions danger, and sometimes leads to shipwreck and loss of life. Of course the captain of a ship proceeds with extreme caution when the weather has long been cloudy, especially if, according to his reckoning, he is drawing near shore. Then the lead comes into play, that by soundings, if possible, the approach to shore may be indicated.

schel's more matured views, his well-known "Outlines of Astronomy:" "For what purpose are we to suppose such magnificent bodies scattered through the abyss of space? Surely not to illuminate our nights, which an additional moon of the thousandth part of the size of our own world would do much better; nor to sparkle as a pageant void of meaning and reality, and bewilder us among vain conjectures. Useful, it is true, they are to man as points of exact and permanent reference, but he must have studied astronomy to little purpose, who can suppose man to be the only object of his Creator's care; or who does not see, in the vast and wonderful apparatus around us, provision for other races of animated beings."

[1] It was this doubtless which led to the distinction recognized in the book of Job, where the moon is described as "walking in brightness."

Then, also, by day and night, a careful watch is kept for the signs of land. But it sometimes happens that, despite all such precautions, a ship is lost; for there are conditions of weather which, occurring when a ship is nearing shore, render the most careful lookout futile. These conditions may be regarded as included among ordinary sea-risks, by which term are understood all such dangers as would leave a captain blameless if shipwreck occurred. It would be well if no ships were ever lost save from ordinary sea-risks; but, unfortunately, ships are sometimes cast ashore for want of care; either in maintaining due watch as the shore is approached, or taking advantage of opportunities, which may be few and far between, for observing sun, or moon, or stars, as the voyage proceeds. It may safely be said that the greater number of avoidable shipwrecks have been occasioned by the neglect of due care in finding the way at sea.

SECULAR PROPHECY.

ALTHOUGH prophecy is usually supposed to be the special gift of inspiration, nothing comes more glibly from secular pens. Half of the leading articles in the daily newspapers are more or less disguised predictions. The prophecies of the *Times* are more numerous, more confident, and more explicit, than those of Jeremiah or Isaiah. "Secular Prophecy fulfilled" would be a good title for a book written after the model of those old and half-educated divines who zealously looked through Isaiah, Jeremiah, Daniel, and the Apocalypse, for shadowy hints that Hildebrand would enforce celibacy on the clergy of the Latin Church; that Luther would cut up the Christianity of the West into two sections; that Cromwell would sign the death-warrant of Charles I.; and that the Stuarts would become wanderers over the face of the earth. There are still, we believe, devout, mystical, and studious sectaries, who find such events as the disestablishment of the Irish Church and the meeting of the Vatican Council plainly foretold in the book of Revelation. They also find Mr. Gladstone's name written in letters of fire by inspired pens that left their record while the captivity of Babylon was a recent memory, or while Nero was the scourge of the Church. Nay, Dr. Cumming, who is as different from those mystical interpreters as a smart Yankee trader is from Parson Adams, sees that the Prophet Daniel and St. John had a still more minute acquaintance with the home and Continental politics of these latter days. But "Secular Prophecy fulfilled" would show a much more wonderful series of glimpses into the future than we find in the interpretations of Dr. Cumming, and it would certainly bring together a strange set of soothsayers.

Arthur Young, Lord Chesterfield, and William Cobbett, are not exactly the kind of men whom we should expect to find among the prophets. Arthur Young was a shrewd traveller, with a keen eye for leading facts, and a remarkable power of describing what he saw in plain, homely words. Chesterfield was a literary and philosophical dandy, who, richly furnished with the small coin of wisdom, and fearing nothing so much as indecorum, would have been a great teacher if the earth had been a drawing-room. Cobbett was a coarse, rough English farmer, with an extraordinary power of reasoning at the dictate of his prejudices, and with such a faculty of writing racy, vigorous English as excites the admiration and the despair of scholars. It seems almost ludicrous to speak of such men as prophets. And yet Arthur Young foretold the coming of the French Revolution at a time when the foremost men of France did not dream that the greatest of political convulsions was soon to lay low the proudest of monarchies. And the dandified morality of Lord Chesterfield did not prevent him from making a similar prediction. Cobbett made a guess which was still more notable; for, at the beginning of the present century, he foretold the secession of the Southern States. But the most remarkable of all the secular prophets who have spoken to our time is Heine. He might seem indeed to have been a living irony on the very name of prophet, for he read backward all the sanctities of religion and all the commands of the moral law. Essentially a humorist, to whom life seemed now the saddest of mysteries, and now the most laughable of jokes, he made sport of every thing that he touched. His most fervid English devotee, Mr. Matthew Arnold, is forced to admit that he was profoundly disrespectable. He quarrelled with his best friends for frivolously petty reasons, and he repaid their kindness by writing lampoons which are masterpieces at once of literary skill and of malignity. Neither Voltaire nor Pope scattered calumnies with such a lack of scruple, and Byron himself was not a more persistent or more systematic voluptuary. Yet Heine was so true a prophet that his predictions might have been accounted the work of inspiration if he had been as famed for piety or purity as he was notorious for irreligion and profligacy. He predicted that Germany and France would fight, and that France would be utterly put down. He predicted that the line of fortifications which M. Thiers was then building round Paris would draw to the capital a great hostile army, and that they would crush the city as if they were a contracting iron shroud. He predicted that the Communists would some day get the upper hand in Paris, that they would strike in a spirit of fiendish rage at the statues, the beautiful buildings, and all the other tangible marks of the civilization which they sought to destroy; that they would throw down the Vendôme Column in their hate of the man who had made France the foe of every other people; and that they would further show their execration for his memory by taking his ashes from the Invalides and flinging them into the Seine. All these predictions, save the last, have been fulfilled to the letter, and it would need a bolder prophet than even Heine himself to say that the last will not be verified also. For nothing is more remarkable in France than the success with which the International is teaching the artisans that the first as well as the third Napoleon was the worst enemy of their class. Although they still regard his achievements with pride, they fervently believe that he was the foe of their order, and the acts of the Commune showed their eagerness to insult his name. And there may be another Commune. Intrepid prophets would say that there certainly will be another. If that should happen, it is quite possible that the fanatics of the International may fling the ashes of the great soldier into the Seine to mark their abhorrence of military glory.

Prevost-Paradol was as different from Heine as a gifted voluptuary can be from a polished, fastidious, and decorous gentleman. Yet the refined, reserved, satirical Orleanist, who seemed to be uncomfortable when his hands were not encased in kid gloves, and who was a master of all the literary resources of innuendo, would be as much out of place among the Hebrew prophets as Heine himself. He would find a place, nevertheless, in "Secular Prophecy fulfilled," by reason of the startling exactness with which he foretold the outbreak of the war between his own country and Germany. In a passage which promises to become classic, he said that the two nations were like two trains which, starting from opposite points, and placed on the same line of rails, were driven toward each other at full speed. There must be a collision. The only doubt was, where it would happen, and when, and with what results. De Tocqueville better fulfilled the traditionary idea of a prophet, and there is a startling accuracy in some of the predictions as to the future of France which he flung forth in talking with his friends, and of which we find a partial record in the journal of Mr. Nassau Senior. Eighteen years before the fall of the empire, he predicted that it would wreck itself "in some extravagant foreign enterprise." "War," he added, "would assuredly be its death, but its death would perhaps cost dear." M. Renan also aspires to a place among the prophets, and he has made a prediction which may be a subject of some curiosity when the next pope shall be elected. The Church of Rome will not, he says, be split up by disputes about doctrine. But he does look for a schism, and it will come, he thinks, when some papal election shall be deemed invalid; when there shall be two competing pontiffs, and Europe shall see a renewal of the strife between Rome and Avignon.

It may be said, no doubt, that the verified predictions which we have cited are only stray hits; that the oracles make still more remarkable misses; and that, since guesses about the future are shot off every hour of the day, it would be a marvel if the bull's-eye were not struck sometimes. Such a theory might suffice to account for the hits, if the prophecies were let off in the dark and at random; but that is not the case. It is easy to trace the path along which the mind of Heine or De Tocqueville travelled to the results of the future, and their predictions betray nothing more wonderful than a rare power of drawing correct inferences from confused facts. A set of general rules might be laid down as a guide to prophecy. In the first place, we might give the negative caution that the analogy of past events is misleading, because the same set of conditions does not appear at two different times, and an almost unseen element might suffice to determine an all-important event. Forgetting this fact, Archbishop Manning has ventured into the field of prophecy with the argument that Catholics should not be made uneasy because the pope has lost his temporal power, for they should remember that he has again and again suffered worse calamities, and has then won back all his old authority. Between 1378 and 1418 the Church witnessed the scandal of a schism, in which there were rival popes, and in which Rome and Avignon competed for the mastery. That calamity is worse than any which has come to the Church in our days, yet the Papacy regained its old power and glory. So late as within the present century the temporal power was reduced to nullity by the first Napoleon, and Pius IX. himself had to flee from Rome in the beginning of his reign. Why, then, should not the robber-band of Victor Emmanuel be paralyzed in turn, and the Papacy once more regain its old splendor? Not being ambitious to play the part of prophets, we do not undertake to say whether the Papacy will or will not again climb or be flung into its ancient place, but it is not the less certain that Archbishop Manning's prophecy is a conspicuous example of a false inference. When he argues that a pope in the nineteenth century will again be the temporal ruler of Rome because a pope triumphed over the schism of Avignon in the fifteenth, he forgets that the lapse of centuries has wrought a vast change of conditions. At the end of the fourteenth century a keen onlooker, a Heine or a De Tocqueville, might have confidently foretold that a pope of unquestioned authority would soon govern the historic city of the Papacy, because the political and the social interests of Europe, no less than the piety or superstition of the times, required that the pope should be powerful and free. The current of the age, if we may use the philosophical slang, was running from Avignon to Rome in the fourteenth and fifteenth centuries, and now the current of the age is not less distinctly running against the temporal power. The very reasons which would have led a prophet in 1400 to predict that Rome would again be the unquestioned seat of the Papacy would lead the same soothsayer to affirm in 1873 that the temporal power has been shattered forever.

It is in general causes that we find the guide of prophecy. Mr. Buckle attached so much importance to the physical conditions of a country, the food of a people, the air they breathe, the occupations which they are forced to follow, and the habits of thought which they display, that he undertook to tell the end of a nation from the beginning. Spain was no mystery to him when he remembered that it had originally been a country of volcanoes; that the people had consequently been filled with a dread of the unseen and inscrutable power which reveals itself in convulsions of the earth; that their diseased fear of shadowy influences made them resent the teachings of science, and hence left them an easy prey to the Holy Office and Ignatius Loyola when Luther, Calvin, and Zwingle, drew away from sacerdotalism all the Christianity of Northern Europe. There can be no doubt that Buckle's theory did rest on a basis of truth, and that it erred simply by trying to account for every thing. In fact, it is not specially his doctrine, but simply the rigid and systematized application of a principle which is as old as speculative curiosity. We apply it every day of our lives. If a family go into a badly-drained house, we say the chances are that they will have typhus, diarrhœa, or cholera. If a rich and foolish young man bets largely on the turf, the probability is that he will be ruined. And the statistician comes to help us with a set of tables which throw uncomfortable light on the mechanical character of those mental and moral processes which might seem to be determined by the unprompted bidding of our own wills. Mr. Buckle was no doubt beguiled by a mere dream when he fancied that we could account for every turn and winding in the history of a country if we had only a large knowledge of its general conditions, such as the temperature of the land, the qualities of the soil, the food of the people, and their relations to their neighbors. He paid too little heed to subtle qualities of race, and he did not make sufficient allowance for the disturbing force of men gifted with extraordinary power of brain and will. Still it is a mere truism that the more correctly and fully we know the general condition of a country, the more does mystery vanish from its history, and the successive events tend to take their place in orderly sequence.

It is impossible, however, to prophesy by rule, and such system-mongers as Mr. Buckle would be the most treacherous of all oracles. Their hard and fast canons will not bend into the subtle crevices of human life. Men who are so ostentatiously logical that they cannot do a bit of thinking without the aid of a huge apparatus of sharply-cut principles always lack a keen scent for truth. They blunder by rule when less showy people find their way by mother-wit. Hence they are the worst of all prophets. It was not by counting up how many things tell in one way, and how many tell in another, that Heine and De Tocqueville were able to guess correctly what was coming, but by watching the chief currents of the age, or, as more homely folk would say, by finding out which way the wind was blowing. They had to decide which among many social, religious, or political forces were the strongest, and which would be the most lasting. They had to give a correct decision as to the stability of particular institutions and the strength of popular passions. General rules could not be of much avail, and they had to rely on their knowledge of human nature, their acquaintance with the forces which have been at work in history, and their own sagacity. Most likely Heine could not have given such an explanation of the grounds on which he made his predictions as would have satisfied any average jury of historical students. But he could have said that he knew the working-men of Paris; that his power of poetic sympathy enabled him to see how their minds veered toward socialism, and he also knew what forces were on the side of order; and that a mental comparison of the two made him look with certainty to a ferocious outbreak of democratic passion. Being thus sure that the storm would come, he had next to ask himself which points the lightning would strike, and he looked for the most prominent symbols of kingship, wealth, refinement, and military glory. The Tuileries would be a mark for the fury of the mob, because that was the palace of the man who had destroyed the populace. The public offices must go, because they represented what the *bourgeois* called order and the workmen called tyranny. The Louvre must go, for the mere sake of maddening rich people who took a delight in art. And the Vendôme Column must go, because it glorified a man who was the incarnation of the war-spirit, and who was consequently the worst foe of the working-classes. To a select committee of the House of Commons such reasons would have seemed the dreams of a moon-struck visionary, and they certainly did not admit of being logically defended. No prophecy does. The power of predicting events is the power of guessing, and those guess best who are least dependent on rules, and most gifted with the mother-wit which works with the quietude and unconsciousness of instinct.—*Saturday Review.*

SYMPATHETIC VIBRATIONS IN MACHINERY.[1]

BY PROF. J. LOVERING,

OF HARVARD COLLEGE.

AT the meeting of this Association in Burlington, I showed some experiments in illustration of the *optical method* of making sensible the vibrations of the column of air in an organ-pipe. At the Chicago meeting I demonstrated the way in which the vibrations of strings could be studied by the eye in place of the ear, when these strings were attached to tuning-forks with which they could vibrate in sympathy; substituting for the small forks, originally used by Melde,

[1] From the Proceedings of the Twenty-first Meeting of the American Association for the Advancement of Science.

a colossal tuning-fork, the prongs of which were placed between the poles of a powerful electro-magnet. This fork, which interrupted the battery current, at the proper time, by its own motion, was able to put a heavy cord, thirty feet in length, in the most energetic vibration, and for an indefinite time. I propose, at the present time, to speak of those sympathetic vibrations which are pitched so low as not to come within the limits of human ears, but which are felt rather than heard, and to show how they may be seen as well as felt.

All structures, large or small, simple or complex, have a definite rate of vibration, depending on their materials, size, and shape, and as fixed as the fundamental note of a musical cord. They may also vibrate *in parts*, as the cord does, and thus be capable of various increasing rates of vibration, which constitute their harmonics. If one body vibrates, all others in the neighborhood will respond, if the rate of vibration in the first agrees with their own principal or secondary rates of vibration, even when no more substantial bond than the air unites a body with its neighbors. In this way, mechanical disturbances, harmless in their origin, assume a troublesome and perhaps a dangerous character, when they enter bodies all too ready to move at the required rate, and sometimes beyond the sphere of their stability.

When the bridge at Colebrooke Dale (the first iron bridge in the world) was building, a fiddler came along and said to the workmen that he could fiddle their bridge down. The builders thought this boast a fiddle-de-dee, and invited the itinerant musician to fiddle away to his heart's content. One note after another was struck upon the strings until one was found with which the bridge was in sympathy. When the bridge began to shake violently, the incredulous workmen were alarmed at the unexpected result, and ordered the fiddler to stop.

At one time, considerable annoyance was experienced in one of the mills in Lowell, because the walls of the building and the floors were violently shaken by the machinery: so much so that, on certain days, a pail of water would be nearly emptied of its contents, while on other days all was quiet. Upon investigation it appeared that the building shook in response to the motion of the machinery only when that moved at a particular rate, coinciding with one of the harmonics of the structure; and the simple remedy for the trouble consisted in making the machinery move at a little more or a little less speed, so as to put it out of time with the building.

We can easily believe that, in many cases, these violent vibrations will loosen the cement and derange the parts of a building, so that it may afterward fall under the pressure of a weight which otherwise it was fully able to bear, and at a time, possibly, when the machinery is not in motion; and this may have something to do with such accidents as that which happened to the Pemberton Mills in Lawrence. Large trees are uprooted in powerful gales, because the wind comes in gusts; and, if these gusts happen to be timed in accordance with the natural swing of the tree, the effect is irresistible. The slow vibrations which proceed from the largest pipes of a large organ, and which are below the range of musical sounds, are able to shake the walls and floors of a building so as to be felt, if not heard, thereby furnishing a background of noise on which the true musical sounds may be projected.

We have here the reason of the rule observed by marching armies when they cross a bridge; viz., to stop the music, break step, and open column, lest the measured cadence of a condensed mass of men should urge the bridge to vibrate beyond its sphere of cohesion. A neglect of this rule has led to serious accidents. The Broughton bridge, near Manchester, gave way beneath the measured tread of only sixty men who were marching over it. The celebrated engineer, Robert Stephenson, has remarked [1] that there is not so much danger to a bridge, when it is crowded with men or cattle, or if cavalry are passing over it, as when men go over it in marching order. A chain-bridge crosses the river Dordogne on the road to Bordeaux. One of the Stephensons passed over it in 1845, and was so much struck with its defects, although it had been recently erected, that he notified the authorities in regard to them. A few years afterward it gave way when troops were marching over it.[2]

A few years ago, a terrible disaster befell a battalion of French infantry, while crossing the suspension-bridge at Angers, in France. Reiterated warnings were given to the troops to break into sections, as is usually done. But the rain was falling heavily, and, in the hurry of the moment, the orders were disregarded. The bridge, which was only twelve years old, and which had been repaired the year before at a cost of $7,000, fell, and 280 dead bodies were found, besides many who were wounded. Among the killed or drowned were the chief of battalion and four other officers. Many of the guns were bent double, and one musket pierced completely through the body of a soldier. The wholesale slaughter at the bridge of Beresina, in Russia, when Napoleon was retreating from Moscow, in 1812, and his troops crowded upon the bridge and broke it, furnishes a fitting parallel to this great calamity.

When Galileo set a pendulum in strong vibration by blowing on it whenever it was moving away from his mouth, he gave a good illustration of the way in which small but regularly-repeated disturbances grow into consequence. Tyndall tells us that the Swiss muleteers tie up the bells of the mules, for fear that the tinkle should bring an avalanche down. The breaking of a drinking-glass by the human voice, when its fundamental note is sounded, is a well-authenticated feat; and Chladni mentions an innkeeper who frequently repeated the

[1] *Edinburgh Philosophical Journal*, vol. v., p. 255.

[2] Smiles's "Life of Stephenson," p. 390.

experiment for the entertainment of his guests and his own profit. The nightingale is said to kill by the power of its notes. The bark of a dog is able to call forth a response from certain strings of the piano. And a curious passage has been pointed out in the Talmud, which discusses the indemnity to be claimed when a vessel is broken by the voice of a domestic animal. If we enter the domain of music, there is no end to the illustrations which might be given of these sympathetic vibrations. They play a conspicuous part in most musical instruments, and the sounds which these instruments produce would be meagre and ineffective without them.

In the case of vibrations which are simply mechanical, without being audible, or at any rate musical, the following ocular demonstration may be given: A train of wheels, set in motion by a strong spring wound up in a drum, causes an horizontal spindle to revolve with great velocity. Two pieces of apparatus like this are placed at the opposite sides of a room. On the ends of the spindles which face one another are attached buttons about an inch in diameter. The two ends of a piece of white tape are fastened to the rims of these buttons. When the spindles, with the attached buttons, revolve, the two ends of the tape revolve, and in such directions as to prevent the tape from twisting, unless the velocities are different. Even if the two trains of wheels move with unequal velocities, when independent of each other, the motions tend to uniformity when the two spindles are connected by the tape. Now, by moving slightly the apparatus at one end of the room, the tape may be tightened or loosened. If the tape is tightened, its rate of vibration is increased, and, at the same time, the velocity of the spindles is diminished on account of the greater resistance. If the tape is slackened, its rate of vibration is less, and the velocity of the spindles is greater. By this change we can readily bring the fundamental vibration of the tape into unison with the machinery, and then the tape responds by a vibration of great amplitude, visible to all beholders. If we begin gradually to loosen the tape, it soon ceases to respond, on account of the twofold effect already described, until the time comes when the velocity of the machinery accords with the first harmonic of the tape, and the latter divides beautifully into two vibrating segments with a node at the middle. As the tension slowly diminishes, the different harmonics are successively developed, until finally the tape is broken up into numerous segments only an inch or two in length. The eye is as much delighted by this visible music as the ear could be if the vibrations were audible; and the optical demonstration has this advantage, that all may see, while few have musical ears. A tape is preferred to a cord in this experiment, because it is better seen, and any accidental twist it may acquire is less troublesome.

SPECULATION IN SCIENCE.[1]

By Prof. J. LAWRENCE SMITH.

I NOW pass to the second part of my discourse. It is in reference to the methods of modern science—the caution to be observed in pursuing it, if we do not wish to pervert its end by too confident assertions and deductions.

It is a very common attempt, nowadays, for scientists to transcend the limits of their legitimate studies, and in doing this they run into speculations apparently the most unphilosophical, wild, and absurd; quitting the true basis of inductive philosophy, and building up the most curious theories on little else than assertion; speculating upon the merest analogy; adopting the curious views of some metaphysicians, as Edward von Hartmann; striving to work out speculative results by the inductive method of natural science.

And such an example as this is of great value to the reflective mind, teaching caution, and demonstrating the fact that, while the rules by which we are guided in scientific research are far in advance of those of ancient days, we must not conclude that they are perfect by any means. In our modern method of investigation how many conspicuous examples of deception we have had in pursuing even the best method of investigation! Take, for instance, the science of geology, from the time of Werner to the present day. While we always thought we had the true interpretation of the structural phenomena of the globe, as we progressed from year to year, yet how vastly different are our interpretations of the present day from what they were in the time of Werner! In chemistry, the same thing is true. How clearly were all things explained to the chemist of the last century by Phlogiston, which, in the present century, receive no credence, and chemical phenomena are now viewed in an entirely different light!

Lavoisier, in the latter part of the last century, elucidated the phenomena of respiration and the production of animal heat by one of the most beautiful theories, based, to all appearances, upon well-observed facts; yet, at the present day, more delicate observations, and the discovery of the want of balance between the inhaled oxygen and exhaled carbonic acid, subverted that beautiful theory, and we are left entirely without one. It is true we have collated a number of facts in regard to respiration, molecular changes in the tissues, etc., all of which are recognized as having something to do with animal heat; still it is acknowledged that we are incapable of giving any concrete expression to the phenomena of respiration and animal heat as Lavoisier did eighty or ninety years ago.

[1] Abstract of the address before the American Association for the Advancement of Science, at its late meeting in Portland, Me., by the retiring president.

Electricity is the same now as it has ever been, yet it was once spoken of as a fluid, then as a force, now as an energy readily convertible into caloric or mechanical energy; and in what light it will be considered fifty years hence no one can predict.

Now, what I desire to enforce here is, that amid all these changes and revolutions of theories, so called, it is simply man, the interpreter, that has erred, and not Nature; her laws are the same; we simply have not been able to read them correctly, and perhaps never will be.

What, it may be asked, are we to do, then? Must we cease theorizing? Not at all. The lesson to be learned from this is to be more modest in our generalizations; to generalize as far as our carefully-made-out facts will permit us, and no further; check the imagination, and let it not run riot and shipwreck us upon some metaphysical quicksand.

The fact is, it becomes a question whether there is such a thing as pure theory in science. No true scientific theory deserves the name that is not based on verified hypothesis; in fact, it is but a concise interpretation of the deductions of scientific facts. Dumas has well said that theories are like crutches, the strength of them is, to be tested by attempting to walk with them. And I might further add, that very often scientists, who are without sure-footed facts to carry them along, take to these crutches.

It is common to speak of the theory of gravitation, when there is nothing purely hypothetical in connection with the manner in which it was studied; in it we only see a clear generalization of observed laws which govern the mutual attraction of bodies. If at any time Newton did assume an hypothesis, it was only for the purpose of facilitating his calculations: "Newton's passage from the falling of an apple to the falling of a moon was at the outset a leap of the imagination;" but it was this hypothesis, verified by mathematics, which gave to the so-called theory of gravitation its present status.

In regard to light, we are in the habit of connecting with it a pure hypothesis, viz., the impressions of light being produced by emission from luminous bodies, or by the undulation of an all-pervading, attenuated medium; and these hypotheses are to be regarded as probable so long as the phenomena of light are explained by them, and no longer. The failure to explain one single well-observed fact is sufficient to cast doubt upon or subvert any pure hypothesis, as has been the case with the emission theory of light, and may be the fate of the undulatory theory, which, however, up to the present time, serves in all cases.

It is not my object to criticise the speculations of any one or more of the modern scientists who have carried their investigations into the world of the imagination; in fact, it could not be done in a discourse so limited as this, and one only intended as a prologue to the present meeting. But, in order to illustrate this subject of method more fully, I will refer to Darwin, whose name has become synonymous with progressive development and natural selection, which we had thought had died out with Lamarck fifty years ago. In Darwin we have one of those philosophers whose great knowledge of animal and vegetable life is only transcended by his imagination. In fact, he is to be regarded more as a metaphysician with a highly-wrought imagination than as a scientist, although a man having a most wonderful knowledge of the facts of natural history. In England and America we find scientific men of the profoundest intellects differing completely in regard to his logic, analogies, and deductions; and in Germany and France the same thing—in the former of these countries some speculators saying that "his theory is our starting-point," and in France many of her best scientific men not ranking the labors of Darwin with those of pure science. Darwin takes up the law of life, and runs it into progressive development. In doing this, he seems to me to increase the embarrassment which surrounds us on looking into the mysteries of creation. He is not satisfied to leave the laws of life where he finds them, or to pursue their study by logical and inductive reasoning. His method of reasoning will not allow him to remain at rest; he must be moving onward in his unification of the universe. He started with the lower order of animals, and brought them through their various stages of progressive development until he supposed he had touched the confines of man; he then seems to have recoiled, and hesitated to pass the boundary which separated man from the lower order of animals; but he saw that all his previous logic was bad if he stopped there, so man was made from the ape (with which no one can find fault, if the descent be legitimate). This stubborn logic pushes him still further, and he must find some connecting link between that most remarkable property of the human face called expression; so his ingenuity has given us a very curious and readable treatise on that subject. Yet still another step must be taken in this linking together man and the lower order of animals; it is in connection with language; and before long it is not unreasonable to expect another production from that most wonderful and ingenious intellect on the connection between the language of man and the brute creation.

Let us see for a moment what this reasoning from analogy would lead us to. The chemist has as much right to revel in the imaginary formation of sodium from potassium, or iodine and bromine from chlorine, by a process of development, and call it science, as for the naturalist to revel in many of his wild speculations, or for the physicist who studies the stellar space to imagine it permeated by mind as well as light—mind such as has formed the poet, the statesman, or the philosopher. Yet any chemist who would quit his method of investigation, of marking every foot of his advance by some indelible imprint, and go back to the speculations of Albertus Magnus, Roger

Bacon, and other alchemists of former ages, would soon be dropped from the list of chemists and ranked with dreamers and speculators.

What I have said is, in my humble opinion, warranted by the departure Darwin and others have made from true science in their purely speculative studies; and neither he nor any other searcher after truth expects to hazard great and startling opinions without at the same time courting and desiring criticism; yet dissension from his views in no way proves him wrong—it only shows how his ideas impress the minds of other men. And just here let me contrast the daring of Darwin with the position assumed by one of the great French naturalists of the present day, Prof. Quatrefages, in a recent discourse of his on the physical character of the human race. In referring to the question of the first origin of man, he says distinctly that, in his opinion, it is one that belongs not to science; these questions are treated by theologians and philosophers: "Neither here nor at the Museum am I, nor do I wish to be, either a theologian or a philosopher. I am simply a man of science; and it is in the name of comparative physiology, of botanical and zoological geography, of geology and paleontology, in the name of the laws which govern man as well as animals and plants, that I have always spoken." And, studying man as a scientist, he goes on to say: "It is established that man has two grand faculties, of which we find not even a trace among animals. He alone has the moral sentiment of good and evil; he alone believes in a future existence succeeding this natural life; he alone believes in beings superior to himself, that he has never seen, and that are capable of influencing his life for good or evil; in other words, man alone is endowed with morality and religion." Our own distinguished naturalist and associate, Prof. Agassiz, reverts to this theory of evolution in the same positive manner, and with such earnestness and warmth as to call forth severe editorial criticisms, by his speaking of it as a "mere mine of assertions," and the "danger of stretching inferences from a few observations to a wide field;" and he is called upon to collect "real observations to disprove the evolution hypothesis." I would here remark, in defence of my distinguished friend, that scientific investigation will assume a curious phase when its votaries are required to occupy time in looking up facts, and seriously attempting to disprove any and every hypothesis based upon proof, some of it not even rising to the dignity of circumstantial evidence.

I now come to the last point to which I wish to call the attention of the members of the Association in the pursuit of their investigations, and the speculations that these give rise to in their minds. Reference has already been made to the tendency of quitting the physical to revel in the metaphysical, which, however, is not peculiar to this age, for it belonged as well to the times of Plato and Aristotle as it does to ours. More special reference will be made here to the proclivity of the present epoch among philosophers and theologians to be parading science and religion side by side, talking of reconciling science and religion, as if they have ever been unreconciled. Scientists and theologians may have quarrelled, but never science and religion. At dinners they are toasted in the same breath, and calls made on clergymen to respond, who, for fear of giving offence, or lacking the fire and firmness of St. Paul, utter a vast amount of platitudes about the beauty of science and the truth of religion, trembling in their shoes all the time, fearing that science falsely so called may take away their professional calling, instead of uttering in a voice of thunder, like the Boanerges of the Gospel, that the "world by wisdom knew not God." And it never will. Our religion is made so plain by the light of faith that the wayfaring man, though a fool, cannot err therein.

No, gentlemen, I firmly believe that there is less connection between science and religion than there is between jurisprudence and astronomy, and the sooner this is understood the better it will be for both. Religion is based upon revelations as given to us in a book, the contents of which are never changed, and of which there have been no revised or corrected editions since it was first given, except so far as man has interpolated; a book more or less perfectly understood by mankind, but clear and unequivocal in all essential points concerning the relation of man to his Creator; a book that affords practical directions, but no theory; a book of facts, and not of arguments; a book that has been damaged more by theologians than by all the pantheists and atheists that have ever lived and turned their invectives against it—and no one source of mischief on the part of theologians is greater than that of admitting the profound mystery of many parts of it, and almost in the next breath attempting some sort of explanation of these mysteries. The book is just what Richard Whately says it is, viz., "Not the philosophy of the human mind, nor yet the philosophy of the divine nature in itself, but (that which is properly religion) the relation and connection of the two beings—what God is to us, what he has done and will do for us, and what we are to be in regard to him." . . . Let us stick to science, pure, unadulterated science, and leave to religion things which pertain to it; for science and religion are like two mighty rivers flowing toward the same ocean, and, before reaching it, they will meet and mingle their pure streams, and flow together into that vast ocean of truth which encircles the throne of the great Author of all truth, whether pertaining to science or to religion. And I will here, in defence of science, assert that there is a greater proportion of its votaries who now revere and honor religion in its broadest sense, as understood by the Christian world, than that of any other of the learned secular pursuits.

But, before concluding, I cannot refrain from referring to one great event in the history of American science during the past year, as it will doubtless mark an epoch in the development of science in this country. I refer to the noble gift of a noble foreigner to encourage the poor but worthy student of pure science in this country. It is needless for me to insist on the estimation in which Prof. John Tyndall is held among us. We know him to be a man whose heart is as large as his head, both contributing to the cause of science. We regard him as one of the ablest physicists of the time, and one of the most level-headed philosophers that England has ever produced—a man whose intellect is as symmetrical as the circle, with its every point equidistant from the centre. We have been the recipient of former endowments from that land which, we thank God, was our mother-country, for from it we have drawn our language, our liberty, our laws, our literature, our science, and our energy, and without whose wealth our material development would not be what it is at the present day. Count Rumford, the founder of the Royal Society of London, in earlier years endowed a scientific chair in one of our larger universities, and Smithson transferred his fortune to our shores to promote the diffusion of science. Now, while these are noble gifts, yet Count Rumford was giving to his own countrymen—for he was an American—and they were posthumous gifts from men of large fortune. But the one I now refer to was from a man who ranks not with the wealthy, and he laid his offering upon the altar of science in this country with his own hands; and it has been both consecrated and blest by noble words from his own lips; all of which makes the gift a rich treasure to American science; and I think we can assure him that, as the same Anglo-Saxon blood flows in our veins as does in his (tempered, 'tis true, with the Celtic, Teutonic, Latin, etc.), he may expect much from the American student in pure science as the offspring of his gift and his example.

THE GLACIERS AND THEIR INVESTIGATORS.

By Prof. JOHN TYNDALL.

SOON after my return from America, I learned with great concern that a little book of mine, published prior to my departure, had given grave offence to some of the friends and relatives of the late Principal Forbes; and I was specially grieved when informed that the chastisement considered due to this offence was to be administered by gentlemen between whom and myself I had hoped mutual respect and amity would forever reign. We had, it is true, met in conflict on another field; but hostilities had honorably ceased, old wounds had, to all appearance, been healed, and I had no misgiving as to the permanence of the peace established between us.

The genesis of the book referred to is this: At Christmas, 1871, it fell to my lot to give the brief course of "Juvenile Lectures" to which

Faraday for many years before his death lent such an inexpressible charm. The subject of glaciers, which I had never previously treated in a course of lectures, might, it was thought, be rendered pleasant and profitable to a youthful audience. The sight of young people wandering over the glaciers of the Alps with closed eyes, desiring knowledge, but not always finding it, had been a familiar one to me, and I thought it no unworthy task to respond to this desire, and to give such of my young hearers as might visit the Alps an intelligent interest in glacier phenomena.

The course was, therefore, resolved upon; and, to render its value more permanent, I wrote out copious "Notes," had them bound together, and distributed among the boys and girls. Knowing the damage which elementary books, wearily and confusedly written, had done to my own young mind, I tried, to the best of my ability, to confer upon these "Notes" clearness, thoroughness, and life. It was my particular desire that the imaginary pupil chosen for my companion in the Alps, and for whom, odd as it may sound, I entertained a real affection, should rise from the study of the "Notes" with no other feeling than one of attachment and respect for those who had worked upon the glaciers. I therefore avoided all allusion to those sore personal dissensions which, to the detriment of science and of men, had begun fifteen years prior to my connection with the glaciers, and which have been unhappily continued to the present time.

Prof. Youmans, of New York, was then in London, organizing the "International Scientific Series," with which his name and energy are identified. To prove my sympathy for his work, I had given him permission to use my name as one of his probable contributors, the date of my contribution being understood to belong to the distant, and indeed indefinite, future. He, however, read the "Notes," liked them, urged me to expand them a little, and to permit him to publish them as the first volume of his series. His request was aided by that of another friend, and I acceded to it—hence the little book, entitled the "Forms of Water," which the friends and relatives of Principal Forbes have read with so much discontent.

That modest volume has, we are informed, caused an uncontemplated addition to be made to the Life of Principal Forbes, lately published under the triple auspices of Principal Shairp, the successor of Principal Forbes in the College of St. Andrew's, Mr. Adams-Reilly, and Prof. Tait. "It had been our hope," says Principal Shairp, in his preface, "that we might have been allowed to tell our story without reverting to controversies which, we had thought, had been long since extinguished. But, after most of these sheets were in press, a book appeared, in which many of the old charges against Principal Forbes in the matter of the glaciers were, if not openly repeated, not obscurely indicated. Neither the interests of truth, nor justice to the dead, could suffer such remarks to pass unchallenged. How it has been thought best for the present to meet them, I must leave my friend and fellow-laborer, Prof. Tait, to tell."

The book here referred to is the unpretending volume whose blameless advent I have just described.

I have not the honor of knowing Principal Shairp personally, but he will, I trust, permit me to assure him of two things: Firstly, that, in writing my book, I had no notion of rekindling an extinct fire, or of treating with any thing but tenderness the memory of his friend. Secondly, that, had such been my intention, the negative attribute, "not obscure," is hardly the one which he would have chosen to describe the words that I should have employed. But the fact is, the fire was not extinct: the anger of former combats, which I thought spent, was still potential, and my little book was but the finger which pulled the trigger of an already-loaded gun.

Let the book speak for itself. I reproduce here *in extenso* the references to Principal Forbes, which have been translated into "charges" against him by Principal Shairp. Having, in section 20, mentioned the early measurements of glaciers made by Hugi and Agassiz, I continue thus:

"We now approach an epoch in the scientific history of glaciers. Had the first observers been practically acquainted with the instruments of precision used in surveying, accurate measurements of the motion of glaciers would probably have been earlier executed. We are now on the point of seeing such instruments introduced almost simultaneously by M. Agassiz on the glacier of the Unteraar, and by Prof. Forbes on the Mer de Glace. Attempts had been made by M. Escher de la Linth to determine the motion of a series of wooden stakes driven into the Aletsch Glacier, but the melting was so rapid that the stakes soon fell. To remedy this, M. Agassiz, in 1841, undertook the great labor of carrying boring-tools to his 'hotel,' and piercing the Unteraar Glacier at six different places to a depth of ten feet, in a straight line across the glacier. Into the holes six piles were so firmly driven that they remained in the glacier for a year, and, in 1842, the displacements of all six were determined. They were found to be 160 feet, 225 feet, 269 feet, 245 feet, 210 feet, and 125 feet, respectively.

"A great step is here gained. You notice that the middle numbers are the largest. They correspond to the central portion of the glacier. Hence, these measurements conclusively establish, not only the fact of glacier motion, but that the *centre of the glacier, like that of a river, moves more rapidly than the sides.*

"With the aid of trained engineers, M. Agassiz followed up these measurements in subsequent years. His researches are recorded in a work entitled 'Système Glaciaire,' which is accompanied by a very noble Atlas of the Glacier of the Unteraar, published in 1847.

"These determinations were made by means of a theodolite, of which I will give you some notion immediately. The same instrument was employed the same year by the late Principal Forbes upon the Mer de Glace. He established independently the greater central motion. He showed, moreover, that it is not necessary to wait a year, or even a week, to determine the motion of a glacier; with a correctly-adjusted theodolite he was able to determine the motion of various points of the Mer de Glace from day to day. He affirmed, and with truth, that the motion of the glacier might be determined from hour to hour. We shall prove this farther on. Prof. Forbes also triangulated the Mer de Glace, and laid down an excellent map of it. His first observations and his survey are recorded in a celebrated book published in 1843, and entitled 'Travels in the Alps.'

"These observations were also followed up in subsequent years, the results being recorded in a series of detached letters and essays of great interest. These were subsequently collected in a volume entitled 'Occasional Papers on the Theory of Glaciers,' published in 1859. The labors of Agassiz and Forbes are the two chief sources of our knowledge of glacier phenomena."

It would be difficult for an unbiassed person to find in these words any semblance of a "charge" against Principal Forbes. His friends and relatives may be dissatisfied to see the name of M. Agassiz placed first in relation to the question of the quicker central flow of glaciers; but in giving it this position I was guided by the printed data which are open to any writer upon this subject.

I have checked this brief historic statement by consulting again the proper authorities, and this is the result: In 1841 Principal Forbes became the guest of M. Agassiz on the glacier of the Aar; and in a very able article, published some time subsequently in the *Edinburgh Review*, he speaks of "the noble ardor, the generous friendship, the unvarying good temper, the true hospitality" of his host. In order to explain the subsequent action of Principal Forbes, it is necessary to say that the kindly feeling implied in the foregoing words did not continue long to subsist between him and M. Agassiz. I am dealing, however, for the moment with scientific facts, not with personal differences; and, as a matter of indisputable fact, M. Agassiz did, in 1841, incur the labor of boring six holes in a straight line across the glacier of the Aar, of fixing in these holes a series of piles, and of measuring, in 1842, the distance through which the motion of the glacier had carried them. This measurement was made on July 20th; some results of it were communicated to the Academy of Science in Paris on August 1st, and they stand in the "Comptes Rendus" of the Academy as an unquestionable record, from which date can be taken.

But the friends quarrelled. Who was to blame I will not venture here to intimate; but the assumption that M. Agassiz was wholly in the wrong would, I am bound to say, be required to justify the subsequent conduct of Principal Forbes. He was, I gather from the Life, acquainted with the use of surveying instruments; and knowing roughly the annual rate of glacier-motion, he would also know that through the precision attainable with a theodolite, a single day's—probably a single hour's motion—especially in summer, must be discernible. With such knowledge in his possession, as early as June, 1842, and without deeming it necessary to give his host of the Aar any notice of his intention, Principal Forbes repaired to the Mer de Glace, made in the first instance a few rapid measurements at the Montanvert, and in a letter dated from Courmayeur, on July 4th, communicated them to the editor of the Edinburgh *New Philosophical Journal.*

He did not at that time give any numbers expressing the ratio of the side to the central motion of the glacier, but contented himself with announcing the result in these terms: "The central portion of the Mer de Glace moves past the edges in a very considerable proportion, quite contrary to the opinion generally entertained." This communication, as I have said, bears the date of July 4th; but it was first published in the October number of the journal to which it was addressed. My reason, therefore, for mentioning Agassiz first in the "Forms of Water" is, that, apart from all personal complications, his experiment was begun ten months prior to that of his rival, and that he had also two months' priority of publication.

Neither in his "Travels in the Alps," nor in his "Occasional Papers," does Principal Forbes, to my knowledge, make any reference to this communication of Agassiz. I am far from charging him with conscious wrong, or doubting that he justified this reticence to his own mind. But my duty at present lies with objective facts, and not with subjective judgments. And the fact is that, for eighteen years subsequent to this campaign of 1842, Agassiz, as far as the glaciers are concerned, was practically extinguished in England. The labors of the following years failed to gain for him any recognition. His early mistake regarding the quicker motion of the sides of a glacier, and other weaknesses, were duly kept in view; but his positive measurements, and his Atlas, which prove the observations upon the glacier of the Aar to be far more complete than those made upon any other glacier, were never permitted to yield the slightest credit to their author. I am no partisan of Agassiz, but I desire to be just.

Here, then, my case ends as regards the first reference to Principal Forbes, in section 20 of the "Forms of Water."

In section 48 I describe the dirt-bands of the Mer de Glace, and ascribe the discovery of them to Principal Forbes. There can be no thought of a "charge" here.

The next reference that has any bearing upon this discussion occurs in sections 59 and 60 of the "Forms of Water." I quote it fully:

"By none of these writers is the property of viscosity or plasticity ascribed to glacier-ice; the appearances of many glaciers are, however, so suggestive of this idea that we may be sure it would have found more frequent expression were it not in such apparent contradiction with our every-day experience of ice.

"Still the idea found its advocates. In a little book, published in 1773, and entitled 'Picturesque Journey to the Glaciers of Savoy,' Bordier, of Geneva, wrote thus: 'It is now time to look at all these objects with the eyes of reason; to study, in the first place, the position and the progression of glaciers, and to seek the solution of their principal phenomena. At the first aspect of the ice-mountains an observation presents itself, which appears sufficient to explain all. It is that the entire mass of ice is connected together, and presses from above downward after the manner of fluids. Let us, then, regard the ice, not as a mass entirely rigid and immobile, but as a heap of coagulated matter, or as softened wax, flexible and ductile to a certain point.' Here probably for the first time the quality of plasticity is ascribed to the ice of glaciers.

"To us, familiar with the aspect of the glaciers, it must seem strange that this idea once expressed did not at once receive recognition and development. But in those early days explorers were few, and the 'Picturesque Journey' probably but little known, so that the notion of plasticity lay dormant for more than half a century. But Bordier was at length succeeded by a man of far greater scientific grasp and insight than himself. This was Rendu, a Catholic priest and canon when he wrote, and afterward Bishop of Annecy. In 1841 Rendu laid before the Academy of Sciences of Savoy his 'Theory of the Glaciers of Savoy,' a contribution forever memorable in relation to this subject.

"Rendu seized the idea of glacier plasticity with great power and clearness, and followed it resolutely to its consequences. It is not known that he had ever seen the work of Bordier; probably not, as he never mentions it. Let me quote for you some of Rendu's expressions, which, however, fail to give an adequate idea of his insight and precision of thought: 'Between the Mer de Glace and a river there is a resemblance so complete that it is impossible to find in the glacier a circumstance which does not exist in the river. In currents of water the motion is not uniform, either throughout their width or throughout their depth. The friction of the bottom and of the sides, with the action of local hindrances, causes the motion to vary, and only toward the middle of the surface do we obtain the full motion.'

"This reads like a prediction of what has since been established by measurement. Looking at the glacier of Mont Dolent, which resembles a sheaf in form, wide at both ends and narrow in the middle, and reflecting that the upper wide part had become narrow, and the narrow middle part again wide, Rendu observes: 'There is a multitude of facts which seem to necessitate the belief that glacier-ice enjoys a kind of ductility, which enables it to mould itself to its locality, to thin out, to swell, and to contract, as if it were a soft paste.'

"To fully test his conclusions, Rendu required the accurate measurement of glacier motion. Had he added to his other endowments the practical skill of a land-surveyor, he would now be regarded as the prince of glacialists. As it was, he was obliged to be content with imperfect measurements. In one of his excursions he examined the guides regarding the successive positions of a vast rock which he found upon the ice close to the side of the glacier. The mean of five years gave him a motion for this block of forty feet a year.

"Another block, the transport of which he subsequently measured more accurately, gave him a velocity of 400 feet a year. Note his explanation of this discrepancy: 'The enormous difference of these two observations arises from the fact that one block stood near the centre of the glacier, which moves most rapidly, while the other stood near the side, where the ice is held back by friction.' So clear and definite were Rendu's ideas of the plastic motion of glaciers, that, had the question of curvature occurred to him, I entertain no doubt that he would have enunciated beforehand the shifting of the point of maximum motion from side to side across the axis of the glacier (§ 25).

"It is right that you should know that scientific men do not always agree in their estimates of the comparative value of facts and ideas; and it is especially right that you should know that your present tutor attaches a very high value to ideas when they spring from the profound and persistent pondering of superior minds, and are not, as is too often the case, thrown out without the warrant of either deep thought or natural capacity. It is because I believe Rendu's labors fulfil this condition that I ascribe to them so high a value. But, when you become older and better informed, you may differ from me; and I write these words lest you should too readily accept my opinion of Rendu. Judge me, if you care to do so, when your knowledge is matured. I certainly shall not fear your verdict.

"But, much as I prize the prompting idea, and thoroughly as I believe that often in it the force of genius mainly lies, it would, in my opinion, be an error of omission of the gravest kind, and which, if habitual, would insure the ultimate decay of natural knowledge, to neglect verifying our ideas, and giving them outward reality and substance when the means of doing so are at hand. In science, thought, as far as possible, ought to be wedded to fact. This was attempted by Rendu, and in great part accomplished by Agassiz and Forbes.

"Here, indeed, the merits of the distinguished glacialist last named rise conspicuously to view. From the able and earnest advocacy of Prof. Forbes, the public knowledge of this doctrine of glacial plasticity is almost wholly derived. He gave the doctrine a more distinctive form; he first applied the term *viscous* to glacier-ice, and sought to found upon precise measurements a 'viscous theory' of glacier-motion.

"I am here obliged to state facts in their historic sequence. Prof. Forbes, when he began his investigations, was acquainted with the labors of Rendu. In his earliest works upon the Alps he refers to those labors in terms of flattering recognition. But, though, as a matter of fact, Rendu's ideas were there to prompt him, it would be too much to say that he needed their inspiration. Had Rendu not preceded him, he might none the less have grasped the idea of viscosity, executing his measurements, and applying his knowledge to maintain it. Be that as it may, the appearance of Prof. Forbes on the Unteraar Glacier in 1841, and on the Mer de Glace in 1842, and his labors then and subsequently, have given him a name not to be forgotten in the scientific history of glaciers."

Here, again, I have to declare that, in writing thus, I had no notion of "raking up" an old controversy. My object was to render my account historically continuous, and there is not a single word to intimate that I took exception to Principal Forbes's treatment of Rendu. Nay, while placing the bishop in the position he merited, I went out of my way to point out that, in all probability, Principal Forbes required no such antecedent. So desirous was I that no unkind or disparaging word should escape me regarding Principal Forbes, that, had a reasonable objection to the phraseology here used been communicated to me by his friends, I should have altered the whole edition of the work sooner than allow the objectionable matter to appear in it. . . . .

My final reference to Principal Forbes was in § 67 of the "Forms of Water," where the veined structure of glacier-ice is dealt with. Its description by Guyot, who first observed it, is so brief and appropriate that I quoted his account of it. But this was certainly not with a view of damaging the originality of Principal Forbes. In paragraph 474 of my book the observation of the structure upon the glacier of the Aar is thus spoken of: "The blue veins were observed independently three years after M. Guyot had first described them. I say independently, because M. Guyot's description, though written in 1838, remained unprinted, and was unknown in 1841 to the observers on the Aar. These were M. Agassiz and Prof. Forbes. To the question of structure, Prof. Forbes subsequently devoted much attention, and it was mainly his observations and reasonings that gave it the important position now assigned to it in glacier phenomena."

This is the account of Guyot's observation given by Principal Forbes himself. But it may be objected that I am not correct in classing him and Agassiz thus together, and that to Principal Forbes alone belongs the credit of observing the veined structure upon the Aar Glacier. This may be true, but would an impartial writer be justified in ignoring the indignant protests of M. Agassiz and his companions? With regard to the development of the subject, I felt perfectly sure of the merits of Principal Forbes, and did not hesitate to give him the benefit of my conviction.

Such, then, are the grounds of Principal Shairp's complaint quoted at the outset—such the "charges" that I have made "against Principal Forbes," and which the "interests of truth" and "justice to the dead" could not "suffer to pass unchallenged." There is, I submit, no color of reason in such a complaint, and it would never, I am persuaded, have been made had not Principal Shairp and his colleagues found themselves in possession of a document which, though published a dozen years ago by Principal Forbes, was never answered by me, and which, in the belief that I am unable to answer it, is now reproduced for my confutation.

The document here referred to appeared soon after the publication of the "Glaciers of the Alps" in 1860. It is entitled "Reply to Professor Tyndall's Remarks in his Work on the 'Glaciers of the Alps, relating to Rendu's 'Théorie des Glaciers.'" It was obviously written under feelings of great irritation, and, longing for peace, the only public notice I took of it at the time was to say that "I have abstained from answering my distinguished censor, not from inability to do so, but because I thought, and think, that within the limits of the case it is better to submit to misconception than to make science the arena of personal controversy." My critics, however, do not seem to understand that, for the sake of higher occupations, statements may be allowed to pass unchallenged which, were their refutation worth the necessary time, might be blown in shreds to the winds. Of this precise character, I apprehend, are the accusations contained in the republished essay of Principal Forbes, which his friends, professing to know what he would have done were he alive, now challenge me to meet. I accept the challenge, and throw upon them the responsibility of my answer. . . .[1]

Having thus disposed of the two really serious allegations in the reply, I am unwilling to follow it through its minor details, or to spend time in refuting the various intimations of littleness on my part contained in it. The whole reply betrays a state of mental exacerbation which I willingly left to the softening influence of time, and to which, unless forced to it, I shall not recur.

The biographer who has revived this subject speaks of "the numerous controversies into which he" (Principal Forbes) "was dragged." I hardly think the passive verb the appropriate one here. The following momentary glimpse of Principal Forbes's character points to a truer theory of his controversies than that which would refer them to a "drag" external to himself:

"The hasty glance," says this biographer, "which I have been able to bestow upon his less scientific letters has shown me that Forbes attached great importance to mere honorary distinctions, as well as the opinion of others regarding the value of his discoveries. It has opened up a view of a, to me, totally unexpected feature of his character." This is honest, but that the revelation should be "unexpected" is to me surprising. The "love of approbation" here glanced at was in Principal Forbes so strong that he could not bear the least criticism

[1] We omit this portion of the discussion, for lack of space.—EDITOR.

of his work without resenting it as personal. I well remember the late excellent William Hopkins describing to me his astonishment when, at the meeting of the British Association at York, a purely scientific remark of his on Forbes's glacier theory was turned, with sudden acerbity, into a personal matter. It is of a discussion arising out of this remark that Principal Forbes writes thus: "We had a postponed discussion on glaciers on Saturday morning, when Hopkins and I did battle, and I am sorry to say I felt it exceedingly; it discomposed my nerves and made me very uncomfortable indeed, until I was soothed by the minster-service yesterday." [1]

But no amount of "minster-service" could cope with so strong a natural bias, and many a bitter drop fell from the pen of Principal Forbes into the lives of those whom he opposed subsequent to this service at York. On hearing of the paper presented by Mr. Huxley and myself to the Royal Society, he at once jumped to the conclusion that the glaciers were to be made a "regular party question." "All I can do," he says, "is to sit still till the indictment is made out; and I cordially wish my enemy to write a book and print it speedily, as any thing is better than innuendo and suspense." [2] What he meant by "indictment" I do not know; and, with regard to "innuendo," neither of the writers of the paper would be likely to resort to it in preference to plain speaking. The words of a witty philosopher at the time here referred to are significant: "Tyndall," he said, "is beginning with ice, but he will end in hot water." He knew the circumstances, and was able to predict the course of events with the certainty of physical prevision.

The quality referred to by his biographer, and the tendency arising from it to look at things in a personal light, caused his intellect to run rapidly into hypotheses of moral action which had no counterpart in real life. I read with simple amazement his explanation to his friend Mr. Wills of the postponement of the publication of the "Glaciers of the Alps." Some of his supporters in the Council of the Royal Society had proposed him for the Copley Medal, but without success. Had the rules of good taste been observed, he would have known nothing of these discussions; and, knowing them, he ought to have ignored them. But he writes to his friend: "I believe the effect of the struggle, though unsuccessful in its immediate object, will be to render Tyndall and Huxley and their friends more cautious in their further proceedings. For instance, Tyndall's book, again withdrawn from Murray's 'immediate' list, will probably be infinitely more carefully worded relative to Rendu than he first intended." [3]

I should be exceedingly sorry to apply to Principal Forbes the noun-substantive which Byron, in "Childe Harold," applied to Rousseau, but the adjective "self-torturing" is, I fear, only too applicable. His quick imagination suggested chimerical causes for events, but

[1] Life, p. 165. [2] Ibid., p. 369. [3] Ibid., p. 387.

never any thing more chimerical than that here assigned for the postponement of my book and its probable improvement. The "struggle" in the council had no influence upon me, for this good reason, if for no other, that I knew absolutely nothing of the character of the struggle. In *Nature*, for May 22, 1873, Prof. Huxley has effectually disposed of this hypothesis;[1] and those who care to look at the opening sentences of a paper of mine in Mr. Francis Galton's "Vacation Tourists for 1860," will find there indicated another reason for the delay. I may add, that the only part I ever took in relation to Principal Forbes and a medal was to go on one occasion to the Royal Society with the express intention of recommending that he should have one.

The features of character partly revealed by his biographer also explain that tendency on the part of Principal Forbes to bring his own labors into relief, to the manifest danger of toning down the labors of others. This is illustrated by the foot-note appended to page 419. It is also illustrated by his references to Rendu, which, frequent and flattering as they are, left no abiding impression upon the reader's mind. By some qualifying phrase the quotation in each case is deprived of weight; while practical extinction for eighteen years was, as already intimated, the fate of the "generous" and "hospitable" Agassiz.

Toward the close of the "Life" his biographer, while admitting that "to say that Forbes thoroughly explained the behavior of glaciers would be an exaggeration," claims for him that he must "ever stand forward in the history of the question as one of its most effective and scientific promoters." This meed of praise I should be the last to deny him, for I believe it to be perfectly just. To secure it, however, no bitterness of controversy, no depreciation of the services of others, was necessary. One point here needs a moment's clearing up. The word "theory," as regards glaciers, slides incessantly, and without warning, from one into the other of two different senses. It means sometimes the purely physical theory of their formation, structure, and motion, with which the name of Principal Forbes is so largely identified. But it has a wider sense where it embraces the geological action of glaciers on the surface of the globe. For a long time "glacier theory" had reference mainly to the geological phenomena; it was in this sense that the words were employed by Principal Forbes in his article in the *Edinburgh Review*, published in 1842. It is in this sense that they are now habitually applied by M. Agassiz, and in relation to the theory thus defined it is no more than natural for his supporters to assign to M. Agassiz the highest place. I mention this to abolish the mystification which threatens to surround a question which this simple statement will render clear.

I trust I may be permitted to end here. Strong reasons may cause

[1] The words "drift of my statement," employed in Prof. Huxley's letter, ought to be *draft* of my statement.

me to revert to this question, but they must be very strong. I would only warn my readers against the assumption that, if I do not reply to further attack, I am unable to reply to it. The present rejoinder furnishes sufficient proof of the doubtfulness of such a conclusion. There is one darkly-expressed passage in the "Life of Principal Forbes" which may cover something requiring notice. We are informed that he preserved and carefully docketed all letters written to him, and that he retained copies of all his own. It is with regard to this correspondence that his biographer writes thus: "Many extracts, and even entire letters, may be selected which are free from controversy, yet in general these would give but an imperfect notion of the import of the whole. Others again cannot be published at present, because the writers supply him with details of that mysterious wire-pulling which seems to be inseparable from every transaction involving honors (scientific, in common with all others, it is humiliating to confess). The value of this unique series is, however, so great, and its preservation so complete, that it is to be hoped it may be safely deposited (under seal) in the care of some scientific society or institution, to be opened only when all the actors have passed from the scene."

These undignified allusions to "wire-pulling" are perfectly dark to me; but if the letter addressed to Mr. Wills may be taken as a specimen of the entire "series," here referred to, then I agree with the biographer in pronouncing it "unique." Would it not, however, be a manlier course, and a fairer one to those who, writing without *arrière-pensée*, retain no copies of what they write, to let them know, while they are here to take care of themselves, how their reputations are affected by these letters of Principal Forbes? For my own personal part I am prepared to challenge the production of this correspondence *now.—Contemporary Review.*

THE MOON.[1]

OUR satellite holds a somewhat anomalous position in the literature of astronomy. The most beautiful object in the heavens, the orb which telescopists study under the most favorable conditions, and the planet—for a planet she is—which has afforded the most important information respecting the economy of the universe, she nevertheless has not received that attention from descriptive writers which she really merits. The cause is, perhaps, not far to seek. The beauty of the moon can scarcely be described in words, and cannot be pict-

[1] "The Moon: her Motions, Aspect, Scenery, and Physical Condition." By Richard A. Proctor, B. A., Cambridge (England), Honorary Secretary of the Royal Astronomical Society of London; author of the "Sun," "Saturn," "Other Worlds," etc. New York: D. Appleton & Co. Price, $4.50.

ured by the most skilful artist; the information conveyed by the telescope is too definite to permit of speculation as with the other planets, yet not definite enough to solve the questions about which the students of astronomical works take most interest; and the information which astronomers have obtained from the moon's motions can only be appreciated when those motions are thoroughly analyzed, and it has not been found easy to simplify this analysis, that the general reader might fairly be expected to take interest in the matter.

The work before us is intended to remove this long-recognized want in the literature of astronomy. The time has come when this is practicable. The splendid photographs of Rutherford, of New York, and De La Rue, in England, supply the means of exhibiting truthfully the real nature of our satellite's surface. Mr. Proctor has been fortunate in obtaining from Mr. Rutherford permission to use three of his most effective photographs of the moon to illustrate the present work. Recent researches, again, into the processes which are going on within the solar system (so long mistakenly supposed to be unchanging in condition), suggest considerations respecting the past condition of the moon, at once bringing her within the range of speculation and theory. Telescopic observations, also more scrutinizing than those made of yore, and applied more persistently, begin to indicate the possibility at least of recognizing the signs of change, and perhaps of showing that our moon is not the dead and arid waste which astronomers have hitherto supposed her to be. The heat measurements of Lord Rosse also throw important light on the question of her present condition. And then, as respects those points which constitute the main scientific interest of our satellite, her motions under the varying influences to which she is subjected, Mr. Proctor has devoted here his full energies and the results of a long experience, to the endeavor to make clear, even to those who are not mathematicians, the considerations which, weighed and analyzed in the wonderful brain of Newton, supplied the means of demonstrating the theory of the universe.

On this important department of his subject, Mr. Proctor makes the following remarks in his preface: "In Chapter II. I have given a very full account of the peculiarities of the moon's motions; and, notwithstanding the acknowledged difficulty of the subject, I think my account is sufficiently clear and simple to be understood by any one, even though not acquainted with the elements of mathematics, who will be at the pains to read it attentively through. I have sought to make the subject clear to a far wider range of readers than the class for which Sir G. Airy's treatise on 'Gravitation' was written, while yet not omitting any essential points in the argument. In order to combine independence of treatment with exactness and completeness, I first wrote the chapter without consulting any other work. Then I went through it afresh, carefully comparing each section with the corresponding part of Sir G. Airy's 'Gravitation,' and Sir J. Herschel's chapters on the lunar motions in his 'Outlines of Astronomy.' I was thus able to correct any errors in my own work, while in turn I detected a few (mentioned in the notes) in the works referred to. I have adopted a much more complete and exact system of illustration in dealing with the moon's motions than either of my predecessors in the explanation of this subject. I attach great importance to this feature of my explanation, experience having satisfied me not only that such matters should be very freely illustrated, but that the illustrations should aim at correctness of detail, and (wherever practicable) of scale also. Some features, as the advance of the perigee and the retreat of the nodes, have, I believe, never before been illustrated at all."

In Chapter III. Mr. Proctor gives, among other matters, a full explanation of the effects due to the strange balancing motion called the lunar librations. He says: "I have been surprised to find how imperfectly this interesting and important subject has been dealt with hitherto. In fact, I have sought in vain for any discussion of the subject with which to compare my own results. I have, however, in various ways sufficiently tested these results."

But probably, to the greater number of readers, the main interest of the book will be found in the chapters relating to the condition of the moon's surface—the mountains, craters, hills, valleys, which diversify its strange varieties of brightness, color, and tone, and the changes of appearance which are noted as the illumination varies, and as the lunar librations change the position of different regions. It is, by-the-way, to be noted that the moon, which we regard as of silvery whiteness, is in reality more nearly black than white, a fact which will recall to many of our readers a remark of Prof. Tyndall's in the first lecture of the course recently delivered here.

"The moon appears to us," he said, "as if

'Clothed in white samite, mystic, beautiful,'[1]

but, were she covered with the blackest velvet, she would still hang in the heavens as a white orb, shining upon the world substantially as she does now."

Mr. Proctor discusses also the phenomena presented to lunarians, if such there be. The extreme rarity of the lunar atmosphere renders the idea of existence on the moon rather strange to our conceptions, but, as Sir J. Herschel has said in a similar case, "we should do wrong to judge of the fitness or unfitness of" the condition of lunarians "from what we see around us, when perhaps the very combinations which convey to our minds only images of horror may be, in reality, theatres of the most striking and glorious displays of beneficent contrivance." Speaking of the appearances presented by lunar landscapes, two of which we borrow from his work, Mr. Proctor remarks

[1] We quote Tyndall. Tennyson wrote:

"Clothed in white samite, mystic, wonderful."

that "we know far too little respecting the real details of lunar scenery to form any satisfactory opinion on the subject. If a landscape-painter were invited to draw a picture presenting his conceptions of the scenery of a region which he had only viewed from a distance of a hundred miles, he would be under no greater difficulties than the astronomer who undertakes to draw a lunar landscape, as it would actually appear to any one placed on the surface of the moon. We know certain facts—we know that there are striking forms of irregularity, that the shadows must be much darker as well during the lunar day as during an earth-lit lunar light, than on our own earth in sunlight or moonlight, and we know that, whatever features of our own landscapes are certainly due to the action of water in river, rain, or flood, to the action of wind and weather, or to the growth of forms of vegetation with which we are familiar, ought assuredly not to be shown in any lunar landscape. But a multitude of details absolutely necessary for the due presentation of lunar scenery are absolutely unknown to us. Nor is it so easy as many imagine to draw a landscape which shall be correct even as respects the circumstances known to us. For instance, though I have seen many pictures called lunar landscapes, I have never seen one in which there have not been features manifestly due to weathering and to the action of running water. The shadows, again, are never shown as they would be actually seen if regions of the indicated configuration were illuminated by a sun, but not by a sky of light. Again, aërial perspective is never totally abandoned, as it ought to be in any delineation of lunar scenery. I do not profess to have done better myself in the accompanying lunar landscapes. I have, in fact, cared rather to indicate the celestial than the lunarian features shown in these drawings. Still, I have selected a class of lunar objects which may be regarded as, on the whole, more characteristic than the mountain-scenery usually exhibited. And, by picturing the greater part of the landscape as at a considerable distance, I have been freer to reproduce what the telescope actually reveals. In looking at one of these views, the observer must suppose himself stationed at the summit of some very lofty peak, and that the view shows only a very small portion of what would really be seen under such circumstances in any particular direction. The portion of the sky shown in either picture extends only a few degrees from the horizon, as is manifest from the dimensions of the earth's disk; and thus it is shown that only a few degrees of the horizon are included in the landscape."

Our author then pictures the aspect of the lunar heavens by night and by day. We have space but for a few passages from this description: "To an observer stationed upon a summit of the lunar Apennines on the evening of November 1, 1872, a scene was presented unlike any known to the inhabitants of earth. It was near the middle of the long lunar night. On a sky of inky blackness stars innumerable were spread, among which the orbs forming our constellations could be recognized by their superior lustre, but yet were almost lost amid myriads of stars unseen by the inhabitants of earth. Nearly overhead shone the Pleiades, closely girt round by hundreds of lesser lights. From them toward Aldebaran and the clustering Hyades, and onward to the belted Orion, streams and convolutions of stars, interwoven as in fantastic garlands, marked the presence of that mysterious branch-like extension of the Milky-Way which the observer on earth can, with unaided vision, trace no farther than the winged foot of Perseus. High overhead, and toward the north, the Milky-Way shone resplendent, like a vast inclined arch, full 'thick inlaid with patines of bright gold.' Instead of that faint, cloud-like zone known to terrestrial astronomers, the galaxy presented itself as an infinitely complicated star-region—

> 'With isles of light and silvery streams,
> And gloomy griefs of mystic shade.'

"On all sides, this mighty star-belt spread its outlying bands of stars, far away on the one hand toward Lyra and Boötes, where on earth we see no traces of milky lustre, and on the other toward the Twins and the clustering glories of Cancer—the 'dark constellation' of the ancients, but full of telescopic splendors. Most marvellous, too, appeared the great dark gap which lies between the Milky-Way and Taurus; here, in the very heart of the richest region of the heavens—with Orion and the Hyades and Pleiades blazing on one side, and on the other the splendid stream laving the feet of the Twins—there lay a deep, black gulf which seemed like an opening through our star-system into starless depths beyond.

Yet, though the sky was thus aglow with starlight, though stars far fainter than the least we see on the clearest and darkest night were shining in countless myriads, an orb was above the horizon whose light would pale the lustre of our brightest stars. This orb occupied a space on the heavens more than twelve times larger than is occupied by the full moon as we see her. Its light, unlike the moon's, was tinted with beautiful and well-marked colors. . . .

"The globe which thus adorned the lunar sky, and illuminated the lunar lands with a light far exceeding that of the full moon, was our earth. The scene was not unlike that shown to Satan when Uriel—

> 'One of the seven
> Who in God's presence, nearest to the throne,
> Stand ready at command "—

pointing earthward from his station amid the splendor of the sun, said to the arch-fiend:

> 'Look downward on that globe whose hither side
> With light from hence, though but reflected, shines:
> That place is earth, the seat of man; that light
> His day, which else, as th' other hemisphere,
> Night would invade.'

"In all other respects the scene presented to the spectator on the moon was similar; but, as seen from the lunar Apennines, the glorious orb of earth shone high in the heavens; and the sun, source of the light then bathing her oceans and continents, lay far down below the level of the lunar horizon. . . .

"Infinitely more wonderful, however, and transcending in sublimity all that the heavens display to the contemplation of the inhabitants of earth, was the scene presented when the sun himself had risen. I shall venture here to borrow some passages from an essay entitled 'A Voyage to the Sun,' in which a friend of mine has described the aspect of the sun as seen from a station outside that atmosphere of ours which veils the chief glories of the luminary of day: 'The sun's orb was more brilliantly white than when seen through the air, but close scrutiny revealed a diminution of brilliancy toward the edge of the disk, which, when fully recognized, presented him at once as the globe he really is. On this globe could be distinguished the spots and the bright streaks called faculæ. This globe was surrounded with the most amazingly complex halo of glory. Close around the bright whiteness of the disk, and shining far more beautiful by contrast with that whiteness than as seen against the black disk of the moon in total eclipses, stood the colored region called the chromatosphere, not *red*, as it appears during eclipses, but gleaming with a mixed lustre of pink and green, through which, from time to time, passed the most startlingly brilliant coruscations of orange and golden yellow light. Above this delicate circle of color towered tall prominences and multitudes of smaller ones. These, like the chromatosphere, were not red, but beautifully variegated. . . .'

"Much more might be said on this inviting subject, only that the requirements of space forbid, obliging me to remember that the moon and not the sun is the subject of this treatise. The reader, therefore, must picture to himself the advance of the sun with his splendid and complicated surroundings toward the earth, suspended almost unchangingly in the heavens, but assuming gradually the crescent form as the sun drew slowly near. He must imagine also how, in the mean time, the star-sphere was slowly moving westward, the constellations of the ecliptic in orderly succession passing behind the earth at a rate slightly exceeding that of the sun's approach, so that he, like the earth, only more slowly, was moving eastward, so far as the star-sphere was concerned, even while the moon's slow diurnal rotation was carrying him westward toward the earth."

In the last chapter the physical condition of the moon's surface is treated, and the processes by which she probably reached her present condition are discussed at considerable length.

EDITOR'S TABLE.

AMERICAN SCIENTIFIC ASSOCIATION—PRESIDENT SMITH'S ADDRESS.

THE twenty-second meeting of the American Association for the Advancement of Science, which commenced at Portland, Me., August 20th, was fairly attended by the members, and presented very good results in the way of scientific work. In estimating its contributions, we must not overlook the fact that, while the numbers of those in this country who are at liberty to pursue original investigations untrammelled, is not large, on the other hand we have two national associations, through which the moderate amount of original research that takes place is published to the world. While the American Association was the only organization of national scope for the publication of new scientific results, its papers were creditable both in number and quality, and it compared favorably with its prototype, the British Association for the Advancement of Science. But, when, a few years ago, a considerable number of its ablest members joined in the organization of the National Academy of Sciences, having substantially the same object in view as the American Association, but exclusive in its membership, and under government patronage, the necessary effect was greatly to weaken the older organization. The National Academy meets twice a year, and draws closely upon the original work of its associates. If, therefore, the numbers in attendance upon the Association and the grade of scientific contributions might seem to indicate a decline in American science, the circumstances here referred to will sufficiently qualify the conclusion.

The address of the retiring president, J. Lawrence Smith, while containing many excellent suggestions, was not conformed to the better type of such productions. It is the custom of the eminent scientific men who are honored with the office but once in their lives to devote the occasion, either to a general review of recent scientific work, or to some special subject with which they are most familiar, and upon which they can speak with the force of authority. Dr. Smith has been favorably known in the world of science as a chemist who has made valuable contributions in its inorganic department. The great activity in chemical inquiries at the present time, and the important transition through which chemical theory is now passing, would certainly have afforded the president a most pertinent and instructive theme, but he preferred to employ the occasion in considering certain aspects of science that are now prominent in public attention, and upon which the scientific world is in much disagreement. The leading feature of the address was an attack on the Darwinians, and this portion of it we publish; and, as the question is thus reopened officially, it becomes a proper subject of comment.

The predecessor of President Smith, Dr. Asa Gray, of Harvard College, had followed the better usage of presiding officers in his address at Dubuque last year, and discussed some of the larger problems of botany in the light of the derivation theory. The most eminent of American botanists, an old and untiring student of the subject, a man of philosophic grasp, and with a candor and sincerity of conviction that commanded the highest respect, after long and thorough study of the question, Prof. Gray did not hesitate to give the weight of his authority to that view of the origin and diversities of living forms of which Mr. Darwin is now the leading representative. And although in the field of biology large numbers of its most eminent students, who are of all men most competent to decide upon it, have accepted that doctrine as representing the truth of Nature more perfectly than any other, and as of immense value in their researches into the laws of life, yet Dr. Smith, as our readers will see, denounces it as a groundless hypothesis due to a riotous imagination, and, in the language of Agassiz, a "mere mire of assertions." His declarations have called forth the applause of the press—always so candid, and intelligent, and independent, on such matters—who seize the occasion to preach new sermons on the "vagaries of science," and declare that they "take sides with the angels against the monkeys," and are "with the Creator against Darwin."

The course of the president was not commended even by his own party. Dr. Newberry, an eminent student of biology and geology, is reported as having spoken in the following decided way: "Prof. Newberry, after a handsome allusion to the retiring president, Prof. J. Lawrence Smith, protested against the opposition to the development theory as expounded in that gentleman's address. Prof. Newberry said he was not himself a Darwinian, but he recognized the value of the evolution theory in science. You cannot measure its value as you can the work of an astronomer, measured by definite ratios of space and time; but he considered the hypothesis one of the most important contributions ever made to a knowledge of Nature. Most men and women are partisans, and some are willing to suppose that the hypothesis is sufficient to account for all the phenomena of the animal kingdom, while, on the other hand, there are those who see in it nothing but failure and deficiency. Let us assume a judicial position, and allow the tests of time and truth to settle the questions involved. Go, however, in whatever direction the facts may lead, and throw prejudice to the winds. Recollect that all truth is consistent with itself."

Dr. Smith can hardly be said to have argued the question of Darwinism. He gave us his own opinion of it, and quoted, to sustain it, two distinguished authorities in natural history. But he gave the influence of his name and position to the charge that it transcends the legitimate limits of inductive inquiry, and is only a wild and absurd speculation. While the technical and difficult questions of natural history by which the truth or falsity of the doctrine must be determined are beyond the reach of unscientific readers, and belong to the biologists to decide, the question here raised as to whether the investigation, as conducted, is legitimately scientific or not, is one of which all intelligent persons ought to be capable of forming a judgment. We have repeatedly considered this point in the pages of The Popular Science Monthly, and have endeavored to show that the present attitude of the doctrine of evolution is precisely the attitude which all the great established theories and laws of science had to take at their first promulgation. It is familiar to all who know any thing of the progress of science, that astronomy and geology, in their early stages, passed through precisely the same ordeal that biology is passing through now; their leading doctrines were reprobated as false science, and the wild dreams of distempered imaginations. Let us now take another case, in the department of pure physics, and see how scientific history repeats itself:

The undulatory theory of light is now a firmly established principle in physics. Dr. Smith says that "the failure to explain one single well-observed fact is sufficient to cast doubt upon, or subvert, any pure hypothesis," and, he adds, in reference to the undulatory theory, that, "up to the present time, it serves in all cases." In order that this theory, now so perfect, should be adopted, it had, of course, to be first propounded. The conception of an ethereal medium to explain the phenomena of light was suggested by Huyghens and Euler, but they did not experimentally demonstrate it, and their authority was overborne by that of Newton, who maintained the emission or corpuscular theory. The true founder of the undulatory hypothesis of light was Dr. Thomas Young, Professor of Natural Philosophy in the Royal Institution of Great Britain, and whom Prof. Tyndall regards as the greatest physicist who has appeared since Newton. Dr. Young is thus estimated by the German Helmholtz: "His was one of the most profound minds that the world has ever seen; but he had the misfortune to be in advance of his age. He excited the wonder of his contemporaries, who, however, were unable to follow him to the heights at which his daring intellect was accustomed to soar. His most important ideas lay, therefore, buried and forgotten in the folios of the Royal Society, until a new generation gradually and painfully made the same discoveries, and proved the exactness of his assertions, and the truth of his demonstrations."

Now, in this case, there was no monkey in the question, and no capital of public prejudice that could be made available in the discussion, to repress obnoxious opinions. The hypothesis was certainly innocent enough, and its truth or falsehood was a matter of simple determination by experiment. Dr. Young made the experiments which established it—the Royal Society recognized the value of the experiments, and, in 1801, assigned to their author the distinguished honor of delivering the Bakerian lecture, in which his experiments were described, and their conclusions demonstrated. Yet, with the Royal Society to back him, and with his views capable of proof before all men, Dr. Young was crushed, and that by outside influences appealing to the public, on the ground that his hypothesis was spurious science—mere wild absurdity of the imagination.

We ask attention to the similarity of the present ground of attack upon Darwin, and the ground of attack upon Dr. Young three-quarters of a century ago. Dr. Smith prefaces his strictures upon Darwinism with the following declaration: "It is a very common attempt nowadays for scientists to transcend the limits of their legitimate studies, and, in doing this, they run into speculations apparently the most unphilosophical, wild, and absurd; quitting the true basis of inductive philosophy, and building up the most curious theories on little else than assertion."

Henry Brougham, afterward Lord-Chancellor of England, writing in the second number of the *Edinburgh Review* concerning Young's Bakerian lecture, said: "We have of late observed in the physical world a most unaccountable predilection for vague hypotheses daily gaining ground; and we are mortified to see that the Royal Society, forgetful of those improvements in science to which it owes its origin, and neglecting the precepts of its most illustrious members, is now, by the publication of such papers, giving the countenance of its highest authority to dangerous relaxations in the principles of physical logic. We wish to raise our feeble voice against innovations that can have no other effect than to check the progress of science, and renew all those wild phantoms of the imagination which Bacon and Newton put to flight from her temple. . . . Has the Royal Society degraded its publications into bulletins of new and fashionable theories for the ladies of the Royal Institution? *Proh pudor!* [1] Let the professor continue to amuse his audience with an endless variety of

[1] For shame!

such harmless trifles, but, in the name of science, let them not find admittance into that venerable repository which contains the works of Newton and Boyle. . . . The making of an hypothesis is not the discovery of a truth. It is a mere sporting with the subject; it is a sham-fight which may amuse in the moment of idleness and relaxation, but will neither gain victories over prejudice and error, nor extend the empire of science. A mere theory is in truth destitute of merit of every kind, except that of a warm and misguided imagination." Dr. Young's theory "teaches no truth, reconciles no contradictions, arranges no anomalous facts, suggests no new experiments, and leads to no new inquiries. It has not even the pitiful merit of affording an agreeable play to the fancy. It is infinitely more useless, and less ingenious, than the Indian theory of the elephant and tortoise. It may be ranked in the same class with that stupid invention of metaphysical theology. . . . We cannot conclude our review of these articles without entreating for a moment the attention of that illustrious body which has admitted of late years so many paltry and unsubstantial papers into its transactions. . . . We implore the council, if they will deign to cast their eyes upon our humble page, to prevent a degradation of the institution which has so long held the first rank among scientific bodies."

For the second time Dr. Young was selected by the Royal Society to give the Bakerian lecture, and he again chose for its subject "Experiments and Calculations relative to Physical Optics," and again the *Edinburgh Review* came down upon him as follows: "The paper which stands first is another Bakerian lecture, containing more fancies, more blunders, more unfounded hypotheses, more gratuitous fictions, all upon the same field on which Newton trode, and all from the fertile yet fruitless brain of the same eternal Dr. Young." The reviewer thus winds up the controversy: "We now dismiss, for the present, the feeble lucubrations of this author, in which we have searched without success for some traces of learning, acuteness, and ingenuity, that might compensate his evident deficiency in the powers of solid thinking, calm and patient investigation, and successful development of the laws of Nature, by steady and modest observation of her operations. We came to the examination with no other prejudice than the very allowable prepossession against vague hypothesis, by which all true lovers of science have for above a century and a half been swayed. We pursued it, both on the present and on a former occasion, without any feelings except those of regret at the abuse of that time and opportunity which no greater share of talents than Dr. Young's are sufficient to render fruitful by mere diligence and moderation. From us, however, he cannot claim any portion of respect, until he shall alter his mode of proceeding, or change the subject of his lucubrations; and we feel ourselves more particularly called upon to express our disapprobation, because, as distinction has been unwarily bestowed on his labors by the most illustrious of scientific bodies, it is the more necessary that a free protest should be recorded before the more humble tribunals of literature."

The reader will perceive that this strain is not unfamiliar. Young was denounced as Darwin is now denounced, professedly in the interest of science; but the pretext was as false then as it is now. In the former case the *animus* of the assault was mere personal spite: Brougham's inordinate vanity having been wounded by some very moderate criticisms of Dr. Young upon his mathematical works. But a man who did not understand the subject, appealing to a tribunal which knew nothing about it, against wild speculations degrading to science, was able to depreciate and suppress for a quarter of a century one of the most solid and perfect theories of natural phenomena that modern research has produced. And, strange as it may seem, the work was effectually done; for, although Young made a masterly reply, but a single copy was sold, and, as Tyndall remarks, "for twenty years this man of genius was quenched—hidden from the appreciative intellect of his countrymen—deemed, in fact, a dreamer through the vigorous sarcasm of a writer who had then possession of the public ear."

Happily, the time is past when the investigators of Nature can be thus crushed out; but still the old tactics are imitated, and not without evil effect for the time. The men of science, to whom the question belongs, are not left to pursue it in peace. The press and the pulpit, with such scientific help as it is not difficult to get, stir up such a clamor of popular opprobrium that biological students who hold to evolution as the fact and law of Nature, and guide their researches by its light, do not choose to have it publicly known that they are adherents of the doctrine. We are behind England in fair and tolerant treatment of the Darwinian question, but may expect the same improvement in this respect that Huxley tells us has taken place with the English. In a recent article he remarks: "The gradual lapse of time has now separated us by more than a decade from the date of the publication of the 'Origin of Species;' and whatever may be thought or said about Mr. Darwin's doctrines, or the manner in which he has propounded them, this much is certain, that, in a dozen years, the 'Origin of Species' has worked as complete a revolution in biological science as the 'Principia' did in astronomy—and it has done so, because, in the words of Helmholtz, it contains 'an essentially new creative thought.' And, as time has slipped by, a happy change has come over Mr. Darwin's critics. The mixture of ignorance and insolence which, at first, characterized a large proportion of the attacks with which he was assailed, is no longer the sad distinction of anti-Darwinian criticism. Instead of abusive nonsense, which merely discredited its writers, we read essays, which are, at worst, more or less intelligent and appreciative; while, sometimes, like that which appeared in the *North British Review* for 1867, they have a real and permanent value."

THE EDUCATIONAL CONVENTION AT ELMIRA.

The national educational association recently held at Elmira, N. Y., was of unusual interest, and evinced a marked progress in the public method of dealing with educational subjects. We have for some years refrained from attendance upon teachers' conventions, having been wearied with the narrow technical range and pedantic pettiness of the discussions. But the recent meeting showed that educators are beginning to outgrow their old professional limitations, and to consider the various questions that come before them in the light of broad principles, and in the spirit of radical and rational improvement. Many men of ability, presidents of leading colleges, eminent professors, principals of high-schools, and State and city superintendents, were present, contributing valuable papers, and giving strength and character to the debates which followed them.

President McCosh delivered an able address on the higher education, and maintained that the national Government should not give the balance of its lands to the agricultural colleges, nor yet to other collegiate institutions, but should appropriate them for the benefit of high-schools and academies throughout the country. Dr. McCosh thus stated his main position:

"I don't propose that any portion of this $90,000,000 should be given to colleges. We cannot aid all, and to select a few would be injurious. In regard to elementary education, the Northern, the Middle, and the Western States, are able and willing to do their duty. I venture to propose that in these the unappropriated lands be devoted to the encouragement of secondary schools. Let each State obtain its share, and the money handed over to it under certain rigid rules and restrictions to prevent the abuse of the public money. In particular, to secure that upper schools be endowed only where needed, I suggest that money be allocated only when a district, or, it may be, a combination of two or more districts, has raised a certain portion, say one-half, of the necessary funds. By this means the money may be made to stimulate the erection of high-schools all over America. These schools would aid colleges far more powerfully than a direct grant to them, as, in fact, the grand difficulty which colleges have to contend against arises from there being so few schools fitted to prepare young men for them with their rising standard of excellence. But I plead for these schools, not merely as a means of feeding colleges, but as competent to give a high education in varied branches, literary and scientific, to a far greater number who do not go on to any thing higher. These schools, like the elementary schools, should be open to all children, of the poor as well as the rich. They should be set up, like the German gymnasium, in convenient localities, so that all the population may have access to them. They should embrace every useful branch suited to young men and women under sixteen and eighteen years of age—English composition, English language, history, classics, modern language, and elementary science. The best scholars in our primary schools would be drafted up to these higher schools, and thus the young talent of the country would be turned to good account, while the teachers in the common schools would be encouraged by seeing their best pupils advance."

The discussion that followed this speech brought out difficulties which the doctor had not considered, and, in fact, opened the way to the most vital problem of American education. The colleges of the country represent the old scholastic culture which took its shape at a period when popular education was not thought of, and culture was confined to the professional classes. These institutions are not holding their own at the present time. Their students are falling off, for the reason that there is a decline in the academies by which the colleges are fed; that is, as Dr. McCosh says, "the grand difficulty which colleges have to contend against arises from there being so few schools fitted to prepare young men for them."

But the cause of the decline of the academies is the rivalry of the newly-instituted high-schools, and these are the outgrowth and now an essential part of the common-school system. The modern idea of universal education has become organized in such a way as to antagonize the old college system. The common schools are not constructed upon the scholastic pattern; they aim to give to all a useful practical education, that shall be available in the common work of life. It was found that they did not go far enough in this direction for the wants of many, and so high-schools were organized in which the pupils of the common schools might graduate into the working world with a better preparation than the lower schools can furnish. It was stated in the discussion that but one in fifteen hundred of the population passes through college, while it is left for the common and high schools to educate the rest of the people. As the old academies disappear, therefore, the colleges seek to get control of the high-schools, to be used as feeders for themselves; and this, of course, necessitates a high-school curriculum fitted to prepare young men for college. This is the point at which the two systems are unconformable, and is to be the point of conflict in the future. What shall be the course of study in the high-schools? Shall it be a sequel to the common schools, or a prelude to the colleges, for these are different things? Already in some of them we have two distinct systems of education. A principal of one of these institutions in the West said to the writer: "We are working under the disadvantages of a double curriculum. We have a scheme of studies, scientific and practical, drawn with reference to the larger number of our pupils who come from the common schools, and who close their studies with us. We take them through an English course, with mathematics, book-keeping, political economy, physics, chemistry, botany, and physiology. And we have also a classical course for a small number of students who are preparing for college. But the exactions of Latin and Greek are so great upon these that they get hardly a smattering of the subjects pursued by the other students." The tactics of Dr. McCosh were admirable. To keep the proceeds of the public lands from going to the agricultural colleges and scientific institutions, he is willing to resign all claim upon them for the benefit of the classical colleges; at the same time, if the money is expended for the extension of high-schools, as the doctor says, "these schools would aid colleges far more powerfully than a direct grant to them." Yet, as long as the two systems of education remain so diverse that the regular high-school graduation is not accepted as preparation for college, there will be conflict for the control of these establishments. Only as the college curriculum becomes more broad, modern, and scientific, and the classical studies are restricted to the special classes who have need of them, can American education become harmonized in its elements and unified in its system.

The report of President Eliot, of Harvard, on a national university, was a strong document. We publish the last portion of it, which deals with the main question, and ask attention to the high grounds on which he bases his demand for the non-interference of government with the system of higher education. His paper started a warm debate on the broad and important question of the proper relations of government to the work of instruction, and, of course, his views met with vigorous opposition. It was maintained that there is no break in the logic by which government action is prescribed; and that, admitting the propriety of state action in primary education, there is no halting-place until the government takes charge of the entire school machinery of the country. And such is the overshadowing influence of politics, and so profound the superstition regarding government omnipotence, that this view found its urgent advocates, who seem blind to the consequences that are certain to follow when the people shirk the responsibilities of attending directly to the education of the young, and shoulder it off upon a mass of politicians holding the offices of government. The friends of state education certainly pressed their case to its extreme conclusions. Government contributes money to support common schools, and appoints officers to regulate them; therefore let it appropriate $20,000,000 to establish a national university at Washington, with $1,000,000 a year to be divided among the congressional appointees, who will hold the professorships. Dr. McCosh suggested that recent congressional experiences were hardly calculated to inspire confidence in the action of that body, and asked what guarantee we should have against a university ring and systematic educational jobbing; and it was objected by others that the class of men who congregate in the capital, and the whole spirit of the place, would make it more unfit than any other in the country for such an institution. Prof. Richards, of Washington, came to the rescue of the reputation of his town, and asked, emphatically, "Where do its knaves and rascals come from? *We* do not make them; you send them to us from all parts of the nation." But the argument was not helped by the retort, for it is quite immaterial whether Washington breeds its scoundrels or imports them. If our republican system is one that sifts out its most venal and unscrupulous intriguers and sharpers, and gathers them into one place, it is questionable whether that place had better not be avoided as the seat of a great model university—especially if said intriguers and sharpers are to have the management of it.

ELECTIVE STUDIES AT HARVARD.

In an instructive article upon this subject, the *Nation* says: "There was a vague but very general impression, a few years ago, that, if the elective system were introduced into the older American colleges, the practical sciences, as they are called, especially physics, chemistry, and natural history, would crowd out the study of the ancient languages. There was also a feeling that the obvious utility of the modern languages, and particularly of French and of German, would help to throw the "dead languages" into the background. A great many enthusiasts fancied that the good time a-coming was at hand, when books would be thrown aside, and all intellectual activity would be narrowed down to the study of physical Nature; and so much noise has been made about the natural sciences that a great many people undoubtedly think this is the principal if not the only subject taught where an elective system prevails."

To submit this matter to a test, and "ascertain what it is that the mass of students feel the need of most and flock to most when the choice is left entirely to themselves," the *Nation* overhauls the university catalogue of Harvard for 1872–'73, and presents the statistics which bear upon the subject. The "elections" of subjects of study or choices of the students are shown in a succession of tables, the last of which divides the college studies into "disciplinary" and "practical," and exhibits the results as follows:

DISCIPLINARY STUDIES.	
Ancient languages	100
History	37
Mathematics	21
Philosophy	15
Political science	12
	185

PRACTICAL STUDIES.	
Modern languages	80
Physics and chemistry	37
Natural history	28
	145

"By this arrangement the disciplinary studies preponderate over the practical in the ratio of 185:145 or 100:78."

Upon this the *Nation* proceeds to remark: "The figures show conclusively that, in spite of the crusade which has been carried on against the ancient languages, they are still full of vitality, still a power, still a popular study, and, in fact, the greatest interest in the little college world. As our inquiry is purely numerical and statistical, we do not ask why the students make the selections they do. Doubtless, the reasons are not very obvious; still, one fact is plain, that they are not guided wholly by utilitarian views."

Now, if the *Nation* had looked a little into the "why" of this matter, we are sure it would have found the reasons for this state of things obvious enough, and, although it might have somewhat qualified its conclusion, it would have made the statement more valuable. The number of votes cast at an election is usually an expression of public opinion, but, if in any case there happen to have been military interference and dictation, the numerical report of ballots cast, if taken alone, would be misleading. We are told that the working of the option system at Harvard affords an indication of the preferences and tendencies of the students in regard to the studies they incline to pursue; but is not entrance to Harvard a part of its policy, and what about the option *there?* Is there not at the door of the university a big winnowing-machine which delivers the "disciplinary" studies as acceptable wheat, and blows the "utilitarian" studies to the winds as the veriest chaff? All the preparation exacted of students for entrance to college is in the "disciplinary" studies, and mainly in the Latin and Greek languages. Besides being incessantly told in the preparatory schools that the very poles of the intellectual world are two dead languages, and that a classical education is the only real broad liberal education, they are kept for years drilling at Latin and Greek as the only condition upon which they can get to college at all. The standard is here kept as high as it was twenty years ago, and President Eliot stated at the late Elmira convention that, in the estimation of the preparatory teachers in New England, Harvard requires a year more study of Latin and Greek than the other colleges. The student thus enters college warped and biassed by his preparation for it. Of the sciences he knows nothing, and he is prejudiced against them as mere utilitarian studies to be contrasted on all occasions with liberal mental pursuits. When these facts are remembered, it is certainly no matter of surprise that Latin and Greek lead in the collegiate elections of study; it is rather surprising that they lead by so small a number. It is very far from being a fair or open choice when a pupil has to repudiate his past acquisitions, and stem the tide of opinion which has forced them upon him, to take up studies under the grave disadvantage of no early preparation. We think the lesson of the Harvard statistics is not altogether exhilarating to the partisans of the classics. When Harvard will accept a scientific preparation for college as of equal value with the classical, we shall be better prepared to estimate the strength of the tendencies in the two directions.

LIFE OF PRINCIPAL FORBES.

The biographer of Sir Walter Scott alludes to a "first love" which ended unfortunately for the great romancer. It is related that, rain happening to fall one Sunday after church-time, Scott offered his umbrella to a young lady, and, the tender having been accepted, he escorted her to her home. The acquaintance was continued, and ripened into a strong attachment on the part of Scott; but he was doomed to disappointment, and Lockhart states that it produced a profound effect upon his character. "Keble, in a beautiful essay on Scott, more than hints a belief that it was this imaginary regret haunting Scott all his life long which became the true well-spring of his inspiration in all his minstrelsy and romance." Be that as it may, the lady, whose name was Williamina Belches, instead of marrying Scott, chose his friend, Sir William Forbes. They had a family, of which the youngest, James David, was born in 1809. When the son was nineteen years old his father died, and, under the immediate influence of the bereavement, he drew up a set of brief resolutions for the regulation of his life, one of which was "to curb pride and over-anxiety in the pursuit of worldly objects, especially fame." Young Forbes became a famous man. He took to science, and mastered it rapidly under the guidance of his intimate friend Sir David Brewster, choosing physics as his department. At the death of Sir John Leslie, Professor of Natural Philosophy in the University of Edinburgh, he offered himself as a candidate for the chair, in opposition to his old friend Brewster and others, and was elected to the position at the age of twenty-four. He was an original investigator in a wide field of physics, contributed to the extension of knowledge in many directions, and was an able writer. His health failing, he resigned his chair in the Edinburgh University, and accepted the principalship of St. Andrew's, and is therefore known as Principal Forbes. He died the last day of 1868, and an elaborate biography, by three of his Scotch friends, has just been published by Macmillan, which is an extremely interesting book.

Among other subjects of his investigation were the glaciers, upon which he published an important volume. He met Agassiz in the Alps, while that gentleman was experimenting upon glacial motions, and they made observations together, but subsequently fell out with each other about the division of the honors of discovery. The complication extended, involving the claims of Bishop Rendu, Prof. Guyot, and others. In his "Glaciers of the Alps," published in 1860, Prof. Tyndall undertook to do justice to the claims of all parties. Prof. Forbes was not satisfied with the awards, and replied to Prof. Tyndall's work, vindicating his own claims to a larger share of the investigation than had been accorded him. To this Prof. Tyndall at the time made no rejoinder; but in his recently-published "Forms of Water" he restated the case in a way that was not satisfactory to Forbes's biographers, who have met it by an appendix to the volume. In the *Contemporary Review* for August, Prof. Tyndall returns to the question in an elaborate paper, entitled "Principal Forbes and his Biographers," of which we publish the first and last portions, that are of most general interest. We have not space for the whole article, which is long, and omitted the extended extracts from Rendu's work in French, and that portion of the argument which will mainly concern the special students of glacial literature. In an introductory note to the article, Prof. Tyndall briefly states the origin and cause of the controversy, and earnestly deprecates its present revival. He says, speaking of the biographers: "I am challenged to meet their criticisms, which, I find, are considered to be conclusive by some able public journals and magazines. Thus the attitude of a controversialist is once more forced upon me. Since the death of Principal Forbes no one has heard me utter a word inconsistent with tenderness for his memory; and it is with an unwillingness amounting to repugnance that I now defend myself across his grave. His biographers profess to know what he would have done were he alive, and hold themselves to be the simple executors of his will. I cannot act entirely upon this assumption, or deal with the dead as I should with the living. Hence, though these pages may appear to some to be sufficiently full, they lack the completeness, and still more the strength, which I should have sought to confer upon them had my present position been forced upon me by Principal Forbes himself instead of by his friends."

It is to be feared that Prof. Forbes did not sufficiently abide by the rule of life which was formed under the solemn circumstances of his father's death.

We commend to the attention of our scientific readers, with philosophical inclinations, the series of articles on "The Primary Concepts of Modern Physical Science," the first of which appears this month, on "The Theory of the Atomic Constitution of Matter." The depth and force of the criticism are only equalled by the clearness of the conceptions, and the precision and felicity of the statement. The interest of the discussion will not be lessened when we say that it is by an Ohio lawyer—formerly a judge of Cincinnati. It has been held as one of the redeeming features of the English bar, that the author of the able and admirable essay on "The Correlation of Forces" belongs to it; and it is certainly to the credit of the legal profession in this country that a member of it has cultivated physical philosophy to such excellent purpose as is evinced by the article we now publish.

LITERARY NOTICES.

The Unity of Natural Phenomena. A Popular Introduction to the Study of the Forces of Nature. From the French of M. Emile Saigey. With an Introduction and Notes by Thomas Freeman Moses, A. M., M. D. Boston: Estes & Lauréat. Price $1.50. 253 pages.

Although this neat and attractive little volume claims to be a popular introduction to the study of the forces of Nature, we think it should rather be regarded as a book for those who have been previously introduced to the subject. It is rather devoted to an exposition of the author's speculative views than to a simplified and elementary statement for those who are beginning to study. The author holds to a universal ether, and maintains besides that matter is constituted from it, and consists of it, and he aims to build up the universe of ethereal atoms and motion. The work is written from the modern point of view of the correlation of forces, and contains much interesting information upon this subject, but the author is less concerned merely to interpret the phenomena of interaction among the forces than to get below them to what he regards as the causes of their unity. "The atom and motion, behold the universe!" is a somewhat Frenchy and fantastic cosmology. To readers of a speculative turn of mind the book will prove interesting.

Sanitary Engineering: a Guide to the Construction of Works of Sewerage and House-Drainage. By Baldwin Latham, C. E. 352 pages. Price $12. New York: E. & F. N. Spon.

This work is in all respects a contrast to that of M. Saigey. Instead of transcendental ether, it treats of descendental sewerage, and, instead of remote imaginative speculations, it is occupied with the most immediate and practical of the interests of daily life. Of the importance of the subject treated, the preservation of life and health by the thorough construction of sanitary works, there can be no question, and the author claims that it is the first book exclusively devoted to subjects relating to sanitary engineering. He has gathered his material from official reports, periodical papers, and various works which touch the subject incidentally, and, adding to them the results of his own practice, has produced a most valuable treatise. As science unravels the complicated conditions of life, it becomes more and more apparent that health can only be maintained by the destruction or thorough removal of those deleterious products which are engendered in dwellings. The necessity of drainage is well understood, and the art has been long practised in all civilized countries; but, like all other arts, its intelligent and efficient practice depends upon scientific principles, and therefore progresses with a growing knowledge of the subject. The questions involved in the proper sewerage of a district are numerous. Its geological character and physical features have to be considered; the meteorological element of rainfall is important; the constitution of the soil and subsoil must be taken into account; the sources and extent of artificial water-supply are of moment; and the area of the district to be sewered, and its present and prospective population, cannot be overlooked. Much information of this kind requires also to be called into requisition in the construction of separate country-residences. The physical circumstances being given, there then arise numerous questions in regard to drainage, construction, household contrivances, the materials employed, and the cost, efficiency, and permanency of works. Mr. Latham's volume treats this whole series of topics in a systematic and exhaustive way. It is profusely illustrated with woodcuts and maps, and contains numerous tables which are indispensable for the guidance of constructors. It is not reprinted, but is supplied by the New-York branch of the London house, who hold it at an exorbitant pice.

Myths and Myth-Makers: Old Tales and Superstitions interpreted by Comparative Mythology. By John Fiske. Price, $2.00. Boston: James R. Osgood & Co., 1873.

Travellers to the United States, and American authors themselves, have often remarked on the affectionate veneration shown by Americans for the oldest things in Europe, and for all the associations connecting their present life with the life of their forefathers in the old country. Not long ago, it may be remembered, the builders of a new meeting-house at Boston (United States), sent for a brick from the prototype still standing at our Boston in England. We now find an officer of Harvard University putting forth labor which is evidently a labor of love, and the literary skill and taste in which the best American writers set an example worth commending to many of ours; and the things he speaks of belong to the Old World; to a world, indeed, so far off that for centuries we had lost its meaning, and have only just learned to spell it out again. His theme takes him back from the New World, not only to England, not only to Europe, but to the ancient home of the Aryan race, a world still full of wonders for the dwellers in it, whose changes of days and seasons, interpreted by the analogy of human will and action, were instinct with manifold life; where the imagination of our fathers shaped the splendid and gracious forms which have gone forth over the earth, as their children went forth, and prevailed in many lands, and have lived on through all the diverse fates of the kindred peoples in India, in Greece, in Iceland, to bear witness in the latter days to the unity of the parent stock. This book, which Mr. Fiske modestly introduces as a "somewhat rambling and unsystematic series of papers," seems to us to give the leading results of comparative mythology in a happier manner and with greater success than has yet been attained in so small a compass. It is the work of a student who follows in the steps of the great leaders with right-minded appreciation, and who, though he does not make any claim to originality, is no ordinary compiler. He is enthusiastic in his pursuit, without being a fanatic; his style has the attractiveness, due to a certain subtle tact or refinement hard to analyze, but quite sensibly felt, which marks the best American essay-writing; and his manner of dealing with his subject is well fitted to reassure those who have been deterred from seeking any acquaintance with comparative mythology, either by the formidable appearance of philological apparatus and Vedic proper names, or by the aggressive boldness of one or two champions of the new learning. It is very natural to feel a rebellious impulse at being told that half the gods and heroes of the classical epics, or even the nursery tales, which have delighted us from our youth up, are sun and sky, light and darkness, summer and winter, in various disguises.

The myth is in its origin neither an allegory—as Bacon and many others have thought—nor a metaphor—as seems now and then to be implied in the language of modern comparative mythologists—but a genuinely-accepted explanation of facts, a "theorem of primitive Aryan science," as Mr. Fiske happily expresses it. This view is brought out in the last essay of the volume, entitled "The Primeval Ghost World," where the genesis of mythology is held not to be explicable by the science of language alone, and is rather ascribed to the complete absence of distinction between animate and inanimate Nature, which is now known to be common to all tribes of men in a primitive condition, and to which Mr. Tylor has given the name of Animism. We are pleased to find Mr. Fiske praising Mr. Tylor's work warmly, and even enthusiastically: here is another of the many proofs that the ties of common language and culture are in the long-run stronger than diplomacy and Indirect Claims. We find mentioned, among other instances of animism, the belief that a man's shadow is a sort of ghost or other self. This belief has, in comparatively-recent times, made its mark even in so civilized a tongue as the Greek. Στοιχειὸ in Romaic is a ghost, or rather a personified object generally, and seems to correspond exactly to the *other self* attributed by primitive man to all creatures, living or not living, indiscriminately. Mr. Geldart, in a note to his book on Modern Greek (Oxford, 1870), which well deserves the attention of students of language and mythology, traces this as well as older allied meanings from the original meaning of *στοιχεῖον* in classical Greek, as the shadow on the sun-dial, acutely observing that the moving shadow would seem to the natural man far more alive and mysterious than the fixed rod.

There are several matters dealt with in special chapters by Mr. Fiske which we must put off with little more than allusion: the book is indeed a small one, but so full of interest that choice among its contents is not easy. An essay on "The Descent of Fire" treats of the divining-rod and other talismans endowed with the faculty of rending open rocks and revealing hidden treasure, which all appear to be symbols, sometimes obvious, sometimes remotely and fancifully derived, of the lightning which breaks the cloud and lets loose the treasures of the rain. There is also a chapter on the mythology of non-Aryan tribes, showing the difference between the vague resemblance of these to Aryan myths and to one another, and the close family likeness which leads to the certain conclusion that the great mass of Aryan mythology came from a common stock.—*Spectator.*

Home and School: A Journal of Popular Education. Morton & Co., Louisville.

In a late number of this journal is an excellent article by Prof. Alexander Hogg, of the Alabama Agricultural and Mechanical College, entitled "More Geometry—less Arithmetic," that contains various suggestions worthy the thoughtful attention of teachers. It was a favorite idea of the late Josiah Holbrook, which he enforced upon educators on all occasions, that rudimentary geometry should be introduced into all primary schools; but he insisted with equal earnestness upon his theory of their order, which was embodied in his aphorism, "Drawing before writing, and geometry before arithmetic." The priority of geometrical or arithmetical conception in the unfolding mind is a subtle psychological question, into which it is not necessary for the teacher to go, the practical question being to get a recognition of the larger claims of geometry, and this is the point to which Prof. Hogg wisely directs the discussion. The fact is, mental development has been too much considered in its linear and successive aspects, and the theories that are laid down concerning the true order of studies have been hitherto too much confined to this idea. Starting with inherited aptitudes, mental development begins in the intercourse of the infant mind with the environment, and, while it is true that there is a sequence of mental experience in each increasing complexity, it is equally true that many kinds of mental action are unfolded together. Ideas of form are certainly among the earliest, and therefore should have an early cultivation. To all that Prof. Hogg says about the need of increasing the amount of geometry in education we cordially subscribe, and we think he is equally right in condemning the excess of attention that is given to arithmetic, which is mainly due to its supposed practical character as a preparation for business. But neither is geometry without its important practical uses. The professor says:

"Let us see, then, what a pupil with enough arithmetic and the plane geometry can perform. He can measure heights and distances; determine areas; knows that, having enclosed *one* acre with a certain amount of fencing, to enclose *four* acres he only has to *double* the amount of fencing; that the same is true of his buildings. In circles, in round plats, or in cylindrical vessels, he will see a beautiful, universal law pervading the whole—the increase of the circumference is proportional to the increase of the diameter, while the increase of the circle is as the *square* of the diameter. . . .

"Thousands of boys are stuffed to repletion with 'interest,' 'discount,' and 'partnership,' in which they have experienced much 'loss' but no 'profit;' have mastered as many as *five* arithmetics, and yet, upon being sent into the surveyor's office, machine-shop, and carpenter-shop, could not erect a perpendicular to a straight line, or find the centre of a circle already described, if their lives depended upon it. Many eminent teachers think that young persons are incapable of reasoning, and that the truths of geometry are too abstruse to be comprehended by them. . . .

"Children are taught to read, not for

what is contained in the reading-books, but that they may be able to read through life; so, let enough of the leading branches be taught, if no more, to enable the pupil to pursue whatever he may need most in after-life. Let, then, an amount of geometry commensurate with its importance be taught even in the common schools; let it be taught at the same time with arithmetic; let as much time be given to it, and we shall find thousands who, instead of closing their mathematical books on leaving school, will be led to pursue the higher mathematics in their maturer years."

The Mystery of Matter and Other Essays. By J. Allanson Picton. 12mo, pp. 482. Price $3.50. Macmillan & Co.

The purpose of this work is to reconcile the essential principles of religious faith with the present tendencies of thought in the sphere of positive and physical science. Mr. Picton is not a votary of modern skepticism, although he recognizes the fact of its existence, and its bearing on vital questions. Nor is he a partisan of any of the current systems of philosophy or science, but discusses their various pretensions in the spirit of intelligent and impartial criticism. He has no fear of their progress or influence; he accepts many of their conclusions; he honors the earnestness and ability of their expounders; while he believes that their results are in harmony with the essential ideas of religion. It is possible, he affirms, that all forms of finite existence may be reduced to modes of motion. But this is of no consequence in a religious point of view, for motion itself is only the visible manifestation of the energy of an infinite life. "To me," he says, "the doctrine of an eternal continuity of development has no terrors; for, believing matter to be in its ultimate essence spiritual, I see in every cosmic revolution a 'change from glory to glory, as by the Spirit of the Lord.' I can look down the uncreated, unbeginning past, without the sickness of bewildered faith. I want no silent dark eternity in which no world was; for I am a disciple of One who said, 'My Father worketh hitherto.' My sense of eternal order is no longer jarred by the sudden appearance in the universe of a dead, inane substance, foreign to God and spiritual being. And if, with a true insight, I could stand so high above the world as to take any comprehensive survey of its unceasing evolutions—here a nebula dawning at the silent fiat 'be light,' there the populous globe, where the communion of the many with the One brings the creature back to the Creator—I am sure that the oneness of the vision, so far from degrading, would unspeakably elevate my sense of the dignity and blessedness of created being. I have no temptation, therefore, to join in cursing the discoverer who tracks the chain of divine forces by which finite consciousness has been brought to take its present form; because I know he can never find more than that which was in the beginning, and is, and ever shall be—the 'power of an endless life.'"

With regard to the speculations of Prof. Huxley, the author, so far from bewailing their effects, pronounces them decidedly favorable to the interests of religion. They present a formidable barrier to the encroachments of materialism. In this respect, he thinks that Prof. Huxley has rendered services to the Church, if less signal, not less valuable, than those which he has rendered to science. He has brought the religious world face to face with facts with a vigor and a clearness peculiar to himself. Not only so. In the opinion of the author, he has made suggestions concerning those facts of vast importance to the future of religion. He has defined the only terms on which harmony is possible between spiritual religion and physical science. Equalling Berkeley in transparent distinctness of statement, while he far surpasses him in knowledge of physical phenomena, Mr. Huxley has shown that, whether we start with materialism or idealism, we are brought at length to the same point. He has thus proved himself one of the most powerful opponents that materialism ever had. All that he did in his celebrated discourse on the "Physical Basis of Life" was, to call attention to certain indisputable facts. "And perhaps it was the impossibility of denying these facts which was a main cause of the uneasiness that most of us felt. Thus he told us that all organizations, from the lichen up to the man, are all composed mainly of one sort of matter, which in all cases, even those at the extremity of the scale, is almost identical in composition. And the one other fact on which he insisted was, that every living action, from the vibrations of cilia by the foraminifer to the imagination of Hamlet or the composition of the Messiah, is accompanied by, and in a sense finds an equivalent expression in, a definite waste or disintegration of material tissue. Thus it is no less certain that the muscles of a horse are strained by a heavy load, than it is that the brain of a Shakespeare undergoes molecular agitation, producing definite chemical results, in the sublime effort of imagination."

But, at first blush, such statements produce a shock in the minds of most readers. They are reluctant to be told that the soul never acts by itself apart from some excitement of bodily tissue. It seems monstrous that thought and love, which in one direction find their expression in the majesty of eloquence, should in another direction find their expression in evolving carbonic acid and water. Such a union between soul and body seemed to amount to identity. And yet the soul was conscious that, whatever might be said, it was not one of the chemical elements, nor all of them put together.

The mental anxiety referred to has been aggravated by the hold which has been taken on most inquiring minds, by the doctrine of development. Whether natural selection is or is not sufficient to account for the origin of species, the idea of successive acts of creation out of nothing has been virtually abandoned by all whose observations of Nature have been on such a scale as to entitle their opinions to any weight. What was once the property of a few isolated thinkers has been made completely accessible to minds of common intelligence. But the terrors which have been awakened by the popular reception of novel scientific theories are entirely founded on the assumption that matter and spirit are fundamentally distinct in their nature. It has been the general belief that matter was something heavy, lifeless, inert, something that forms the hidden basis of the ethereal vision of the world. But, argues the author, if that assumption be the mere creature of false analogy, and is wholly incongruous and unthinkable, it does not indeed follow that materialism, in a fair sense of the word, is impossible, still the conclusion cannot be avoided that materialism and spiritualism would then exhibit only different aspects of the same everlasting fact, and physical research might henceforth unfold to us only the energies of Infinite Life self-governed by eternal law.

But, admitting the universal action of molecular mechanics, the author adduces numerous instances which show that the explanation they offer of the phenomena of sensation cannot be realized in consciousness. Nothing is really an explanation which cannot be reproduced in consciousness as such. We demand a cause from which the effect can rationally be educed. The perception of distance, for example, is explained by the action of the muscular sense and the experience of touch. This is an adequate explanation, for it can be realized in consciousness. But the case is far otherwise with the explanation of sensation by molecular mechanics. Physical research lands us in a dead inert substance called matter, which, though without soul or meaning in itself, produces by its vibrations the most beautiful visions and sublime emotions in our consciousness. But the external phenomena, inseparable from our consciousness of sight or sound, cannot be rationally connected with the consciousness that gives them all their interest. No one to whom the Hallelujah Chorus utters the joy of heaven, or for whom a sonata of Beethoven gives a voice to the unutterable, can make it seem real to himself that his mind is invaded by mere waves of vibrating air. At no point in the chain of vibrations, not even the point most deeply buried in the brain, can we conceive that molecular action is converted into any thing besides material movement, or resistance to movement. But this does not exhaust the consciousness. The emotional, imaginative, and moral wealth of human life opens a world of reality immeasurably greater than can be contained in mere mechanical movement.

Assuming, then, the fact of a nature in man, of which the molecular laws are not the substance, but the condition, the author takes up the inquiry as to the essential nature of religion. This he defines to be the endeavor after a practical expression of man's conscious relation to the Infinite. The savage who wonders at the unseen but mighty wind that streams from unknown realms of power has already the germ of the feeling which inspires religion. But the conscious relation to the Infinite includes every stage in this consciousness, just as the name of a plant includes the blade as well as the fruit. If the evolution of religion be a normal phase in the development of mankind, there must be at the root of it that grand and measureless Power which is the inevitable complement of the conception of evolution. All evolution implies a divine Power, but religious evolution has to do with the dim apprehension of that Power in consciousness. Mr. Herbert Spencer, to continue the reasoning of the author, has been much blamed, by many religious thinkers, for making the reconciliation between science and religion to lie in the recognition on both sides that "the Power which the universe manifests to us is utterly inscrutable." Yet the very persons who most strenuously object to this suggestion are in the habit of quoting the words of Scripture which declare the unsearchable mystery of the Divine Nature. Those words are used to rebuke the arrogance of philosophy. But, when philosophy learns the lesson, its humility is condemned as wilful blindness. The true philosophy of ignorance, however, retains as an indestructible element of human consciousness an apprehension of something beyond all fragmentary existence, the Absolute Being, at once the only true substance, and the One that constitutes a universe from the phenomenal world. It is inevitable that attempts should be made to give practical expression to this feeling. And in such efforts we find the first germs of religion.

With the imperfect summary which we have given of the views maintained in this volume, it will be perceived that its position in literature is that of a commentary on new developments of thought, rather than of a complete exposition of any system of philosophy or science. Accepting the consequences of modern physical research, it aims to establish their consistency with the principles of a high religious faith, and thus to remove the vague alarms which their prevalence has called forth in certain portions of the community. The author is evidently a man of an ardent poetical temperament, of a reverent and tender spirit, and an aptitude for illustration rather than for demonstration.—*N. Y. Tribune.*

Chimneys for Furnaces, Fireplaces, and Steam-Boilers. By R. Armstrong, C. E., 12mo, 76 pages. Price, 50 cents.

This is number one of Van Nostrand's science series, and is a technological monograph that will be useful to engineers and builders. The author says: "Furnaces or closed fireplaces, which it is the main design of this essay to treat upon, are essentially different in principle and construction to the ordinary open fireplaces of dwelling-houses, as they are exceedingly different in their general scope and object, and in the vast variety of their applications;" and he then proceeds to expound the general philosophy of special chimneys for furnaces and steam-boilers.

Steam-Boiler Explosions. By Zerah Colburn. 12mo, 98 pages. New York: D. Van Nostrand.

This is number two of the same series, and is a most instructive and readable essay. The editor states that, although published ten years ago, later experiences would add but little if any thing to the knowledge it affords. The various observed scientific questions in regard to the causes of steam-boiler explosions, such as over-heating, electricity, the spheroidal state, decomposed steam, etc., are considered, but Mr. Colburn maintains that, whether these are valid causes of explosion or not, they are collectively as nothing compared with the one great cause—defective boilers. The style in which this essay is written is a model of simplicity and clearness.

Bulletin of the Buffalo Society of Natural Sciences. Vol. I., Nos. 1 and 2. Buffalo, 1873.

The Buffalo Society of Natural Sciences commences this year the publication of their *Bulletin*, which it is proposed to continue, four numbers to be issued annually. The two numbers before us contain seven papers, six of which are devoted to the describing and cataloguing of American moths, and one gives descriptions of new species of fungi. The author of the latter paper is Charles H. Peck; all the others are by Augustus R. Grote. Mr. Grote is well known to entomologists as an authority on the subjects which he discusses, and the Buffalo society is to be congratulated for being the medium through which the laborious and valuable researches of so able a naturalist are published to the world. The papers are strictly scientific and technical, being intended solely for those who pursue methodically the special branches of science to which they refer. They are not *popular* expositions, but rather brief notes on certain departments of natural science, to be understood and valued only by the initiated. The *Bulletin* is handsomely printed on good paper, in octavo form. Subscription price, $2.50 per volume.

Atmospheric Theory of the Open Polar Sea: with Remarks on the Present State of the Question. By William W. Wheildon. First Paper. Boston, 1872.

This paper was read at the meeting of the American Association for the Advancement of Science, held at Newport, R. I., in 1860, and was published in the volume of proceedings of the Association for that year. The extraordinary interest taken in Arctic affairs during the past two years has led to its reissue in pamphlet form, with brief introductory observations on the present state of the problem. Accepting the view, now quite generally held, that an open sea, or at least a much ameliorated climate, exists in the vicinity of the pole, the author, in this paper, aims to show that such a condition of things "is largely if not entirely due to the currents of the air from the equatorial regions which move in the higher strata of the earth's atmosphere, bearing heat and moisture with them." How well he succeeds in this undertaking, we leave the readers of the argument to judge.

BOOKS RECEIVED.

Washington Catalogue of Stars. By order of Rear-Admiral Sands, U. S. N. Washington, 1873.

First Annual Report of the Minnesota State Board of Health. St. Paul, 1873, pp. 102.

Scientific and Industrial Education. A Lecture. By G. B. Stebbins. Detroit, 1873, pp. 24.

The Railroads of the United States. By Henry V. Poor. New York: H. V. & H. W. Poor, 68 Broadway, pp. 29.

Cosmical and Molecular Harmonics, No. II. By Pliny Earle Chase, M. A. Philadelphia, 1873, pp. 16.

Nickel. By Dr. Lewis Feuchtwanger, pp. 19.

Diminution of Water on the Earth, and its Permament Conversion into Solid Forms. By Mrs. George W. Houk. Dayton, O., 1873, pp. 39.

Sixth Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology. Cambridge, 1873, pp. 27. Mr. Gillman's report of his explorations of the ancient mounds on the St. Clair River is an important contribution to archæology. The museum is in a flourishing state, and growing steadily. The Niccolucci collection of ancient crania and implements was the most important addition made during the past year.

MISCELLANY.

Utilization of Waste Coal.—The *English Mechanic* gives an historical sketch of the various processes suggested for the utilization of the waste of coal-mines. From this account it would appear that so early as the close of the sixteenth century the waste of small coal attracted notice. About the year 1594 one Sir Hugh Platt proposed a mixture of coal-dust and loam, together with such combustible materials as sawdust and tanners' bark: the loam being the cement which was to hold the other ingredients together. But Sir Hugh's suggestions did not receive much attention in those early times, when coal was but little used, wood being the staple fuel of England.

It was only at the beginning of the present century that this question began to receive serious attention. A patent was then granted for a mixture of refuse coal with charcoal, wood, breeze, tan, peat, sawdust, cork-cuttings, and other inflammable ingredients. A capital objection to such a scheme is its expense. The product would necessarily cost about as much per ton as good coal, without being at all as serviceable. The next attempt was the production of "gaseous coke." Here the object was to convert small coal, by the addition of coal-tar, either pure, or mixed with naphtha, into a well-mixed mass. It was then to be put into an oven and coked; afterward it was to be broken into suitable blocks for use. There were several modifications of this process, but as they all more or less involved the previous manufacture of their most essential ingredient, coal-tar, the anticipations of the projectors were not realized.

In 1823 a step was taken in the right direction by the combination of bituminous and anthracite coals, and converting them, by partial carbonization in an oven, into a kind of soft coke. In 1845 Frederick Ransome introduced a plan for cementing together small coal by means of a solution of silica dissolved in caustic soda, the small refuse coal so treated to be then compressed into blocks suitable for use. In 1849 Henry Bessemer proposed simply to heat small coal sufficient to soften it, and thus render it capable of being easily pressed into moulds and formed into solid blocks. The coal, according to this plan, might be softened either by the action of steam or in suitable ovens. Coal alone was used, no extraneous matter of any kind being employed. In 1856 F. Ransome brought forward one of the best plans yet offered. He placed the small coal in suitable moulds, which were then passed into an oven, and there heated just sufficiently to cause the mass to agglomerate.

Though the writer in the *Mechanic* commends highly the Ransome and the Bessemer plans, it is clear that they do not fully solve the problem, for inventors are still busy on both sides of the Atlantic devising other and better methods. Perhaps, however, the successful working of the Cranston "Automatic Reverberatory Furnace," which is adapted for the consumption of powdered coal, will cause such a demand for small coal as will leave these utilizing processes without material to work on.

Quatrefages on Human Crania.—Quatrefages is engaged on a work entitled "Crania of the Human Races," and recently laid before the Paris Academy of Sciences a synopsis of the results which he there proposes to establish. The materials he has at hand for this investigation are abundant—no less than 4,000 skulls; and he acknowledges the valuable assistance rendered to him by the most eminent *savants* both of France and of the rest of Europe. He holds that the fossil races are not extinct, but that, on the contrary, they have yet living representatives. He regards the skull discovered in 1700 at Canstadt, near Stuttgart, as the type of the most ancient human race of which we have any knowledge. This skull is dolichocephalous—that is, having a length greater than its breadth. With the Canstadt skull he classes those of Enghisheim, Brux, Neanderthal, La Denise, Staengenaes, Olmo, and Clichy—the last-named three being the skulls of females. Among the representatives, in historical times, of the dolichocephalous race, M. Quatrefages reckons Kay Lykke, a Danish statesman of the seventeenth century, whose skull is portrayed in the forthcoming work; Saint Mansuy, Bishop of Toul in the fourth century, whose skull is also figured; and Robert Bruce. Whether the cranium is long or short—dolichocephalous or brachycephalous—is a question which has nothing to do with the intellectual status of the man, according to M. Quatrefages.

Heart-Disease and Overwork.—The early break-down of health observed among Cornish miners, and commonly regarded as an affection of the lungs—"miners' phthisis"—is declared, by competent authority, to proceed rather from disturbed action of the heart; and this, according to Dr. Houghton, the distinguished Dublin physiologist, is caused by the great and sudden strain put upon the system by the ascent from the pits, at a time when the body is not sufficiently fortified with food. In his valuable address on the "Relation of Food to Work," Dr. Houghton says: "The labor of the miner is peculiar, and his food appears to me badly suited to meet its requirements. At the close of a hard day's toil the weary miner has to climb, by vertical ladders, through a height of from 600 to

1,200 feet, before he can reach his cottage, where he naturally looks for his food and sleep. This climbing of the ladders is performed hastily, almost as a gymnastic feat, and throws a heavy strain (amounting to from one-eighth to one-quarter of the whole day's work) upon the muscles of the tired miner, during the half-hour or hour that concludes his daily toil. A flesh-fed man (as a red Indian) would run up the ladders like a cat, using the stores of force already in reserve in his blood; but the Cornish miner, who is fed chiefly upon dough and fat, finds himself greatly distressed by the climbing of the ladders—more so, indeed, than by the slower labor of quarrying in the mine. His heart, over-stimulated by the rapid exertion of muscular work, beats more and more quickly in its efforts to oxidate the blood in the lungs, and so supply the force required. Local congestion of the lung itself frequently follows, and lays the foundation for the affection so graphically though sadly described by the miner at forty years of age, who tells you that his other works are very good, but that he is 'beginning to leak in the valves' Were I a Cornish miner, and able to afford the luxury, I should train myself for the 'ladder-feat' by dining on half a pound of rare beefsteak and a glass of ale from one to two hours before commencing the ascent."

Poisonous Volcanic Gases.—During a volcanic eruption on the little island of San Jorge, one of the Azores, in the year 1808, vaporous clouds were seen to roll down the sides of the mountain, and to move along the valley. Wherever they passed, plants and animals wilted and perished instantaneously. From this asphyxiating action, as also from their downward movement on the mountain-side and toward the sea, we may conclude that they consisted chiefly of some dense, deleterious gas, most probably carbonic acid. Their opacity is to be attributed to the presence of watery vapor, and their reddish color to the presence of fine volcanic dust. Finally, their injurious action on plants was doubtless owing to the presence of chlorhydric and sulphurous acid. Similar phenomena have been observed on occasion of other volcanic outbreaks, but nowhere so marked as in the case of San Jorge. In 1866, for instance, the volcano of Santorin emitted smoke charged with acid, which produced on plants effects similar to those observed at San Jorge in 1808.

A writer in the *Revue Scientifique* is of the opinion that the facts above stated give the solution of some of the problems raised by the exhumations at Pompeii. The strange posture of skeletons found in the streets of that town is very difficult to account for, if we insist on finding analogies with phenomena observed in modern eruptions of Vesuvius. A shower of ashes, however heavy, however charged with humidity, could never have thrown down and choked a strong man like the one who met his death while making his escape, in company with his two daughters, along one of the public roads. They must have inhaled a poisonous gas of some kind, which caused them to perish in fearful agony. This gas would not lie in a layer of equal thickness: in some places it might have a greater depth than in others. Hence, while some of the inhabitants would perish, the remainder would escape.

It is very probable that the eruption in the year 79 was accompanied with local emissions of carbonic acid, springing from points remote from the crater. In all volcanic regions, says the author, there are localities where, even when the volcano is inactive, carbonic acid exists in the atmosphere, in quantities sufficient to produce asphyxia: and the neighborhood of Vesuvius is particularly noted for the number of such localities. During an eruption, the amount of the gas given out is usually increased, and wells, ditches, quarries, etc., are filled with carbonic acid. It is sometimes dangerous to enter cavities in the rocks on the coast when a fresh breeze does not keep them free of the poisonous gas. In 1861 Ste.-Claire Deville came near meeting his death by entering one of these cavities for a few moments. The following week he and the author barely escaped being asphyxiated in the bed of a great quarry, which they had previously visited many a time with impunity.

A Relic of Ancient Etrurian Art.—An antiquarian discovery of very considerable interest was recently made at Cervetri, Italy, being a terra-cotta sarcophagus of native Etruscan production. The ancient Etrurians were noted for the honor they bestowed upon their dead, and their custom of paying homage to ancestors by placing their effigies upon their tombs seems to have been peculiar to themselves, and unknown among the Greeks. The recently-discovered sarcophagus is now in the British Museum. It measures internally four feet ten inches in length, and two feet in width. The floor is hollowed out, or rather marked by a raised border, which takes the form of a human figure. It rests upon four claw feet projecting beyond the angles, and terminating above in the head and breasts of a winged siren. The lid of the sarcophagus represents an upholstered couch upon which recline two human figures, male and female. There are inscriptions on the four sides of the couch. The panel at the foot has the figures of two warriors in panoply, and the front panel exhibits the same pair of warriors engaged in mortal combat. Several accessory figures are also to be seen. On the panel at the head of the couch are represented four sitting figures in opposing pairs, plunged in deep sorrow. The monument has no counterpart among those of its kind hitherto discovered, the only one at all resembling it being that of the Campana Collection in the Louvre. The latter is, however, of a much more recent date than the former, nor is it adorned with either reliefs or inscriptions. The Cervetri sarcophagus probably dates from the period of Etruscan ascendency in Italy.

Audible and Inaudible Sounds.—The phenomenon of color-blindness is a familiar fact; but an analogous phenomenon, what might be called pitch-deafness, though not uncommon, is not so generally known. By *vitch-deafness* is meant insensibility to certain sound-vibrations. Prof. Donaldson, of the University of Edinburgh, used to illustrate the different grades of sensibility to sound by a very simple experiment, namely, by sounding a set of small organ-pipes of great acuteness of tone. The gravest note would be sounded first, and this would be heard by the entire class. Soon some one would remark, "There, 'tis silent," whereas all the rest, perhaps, would distinctly hear the shrill piping continued. As the tone rose, one after another of the students would lose sensation of the acute sounds, until finally they became inaudible to all.

There is reason for supposing that persons whose ear is sensitive to very acute sounds are least able to hear very grave notes, and *vice versa.* Probably the hearing capacity of the human ear ranges over no more than 12 octaves. The gravest note audible to the human ear is supposed to represent about 15 vibrations per second, and the sharpest 48,000 per second.

The auditory range of animals is doubtless very different from that of man; they hear sounds which are insensible to us, and *vice versa.* Many persons are insensible to the scream of the bat—it is too acute. But to the bat itself that sound must be in all cases perfectly sensible. If, then, we suppose the bat to have an auditory range of 12 octaves, and its scream or cry to stand midway in that range, the animal would hear tones some six octaves higher than those audible to the human ear—two and a half million vibrations per second.

Scoresby and other arctic voyagers and whale-hunters have observed that whales have some means of communicating with one another at great distances. It is probable that the animals bellow in a tone too grave for the human ear, but quite within the range of the cetacean ear.

The Motions of the Heart.—According to the generally-accepted teachings of physiologists, the heart rests after each pulsation; that is, each complete contraction during which the auricles are emptied into the ventricles, and the ventricles into the vessels, is followed by a moment's repose, when the organ is entirely at rest. Dr. J. Bell Pettigrew, in his recently-published lectures on the "Physiology of the Circulation," takes a different view, affirming that the normal action of the heart is a continuous one, and that as a whole it never ceases to act until it comes to a final stop. He says: "When the heart is beating normally, one or other part of it is always moving. When the veins cease to close, and the auricles to open, the auricles begin to close and the ventricles to open; and so on in endless succession. In order to admit of these changes, the auriculo-ventricular valves, as has been stated, rise and fall like the diaphragm in respiration; the valves protruding, now into the auricular cavities, now into the ventricular ones. There is in reality no pause in the heart's action. The one movement glides into the other as a snake glides into the grass. All that the eye can detect is a quickening of the gliding movements, at stated and very short intervals. A careful examination of the sounds of the heart shows that the sounds, like the movements, glide into each other. There is no actual cessation of sound when the heart is in action. There are periods when the sounds are very faint, and when only a sharp or an educated ear can detect them, and there are other periods when the sounds are so distinct that even a dull person must hear; but the sounds—and this is the point to be attended to—merge into each other by slow or sudden transitions. It would be more accurate, when speaking of the movements and sounds of the heart, to say they are only faintly indicated at one time, and strongly emphasized at another, but that neither ever altogether ceases. If, however, the heart is acting more or less vigorously as a whole, the question which naturally presents itself is, How is the heart rested? There can be little doubt it rests, as it acts, viz., in parts. The centripetal and centrifugal wave-movements pass through the sarcous elements of the different portions of the heart very much as the wind passes through the leaves: its particles are stirred in rapid succession, but never at exactly the same instant; the heart is moving as a whole, but its particles are only moving at regular and stated intervals; the periods of repose, there is every reason to believe, greatly exceeding the periods of activity. The nourishment, life, and movements of the heart are, in this sense, synonymous."

Poisoning by Oxygen.—M. Paul Bert, whose observations upon the physiological effects of high atmospheric pressure we have already noted in the MONTHLY, communicates to the Paris Academy of Sciences the results of his observations on the toxic action of oxygen. Placing sparrows in oxygen under a pressure of 350 (that of the atmosphere being represented as 100), he found the birds seized with violent convulsions. The same result followed when sparrows were confined in common air under a pressure of 17 atmospheres. In oxygen, at 3½ atmospheres' pressure, or in air at 22 atmospheres, the convulsions were extremely violent and quickly fatal. The symptoms in the latter case were these: Convulsions set in after four or five minutes: in moving about, the bird hobbles on its feet, as though walking on hot coals. It then flutters its wings, falls on its back, and spins about, the claws doubled up. Death supervenes after a few such spasms.

The toxic dose of oxygen for a dog was found to require, for convulsions, a pressure of 350 in oxygen; and a pressure of 500 is fatal. The amount of oxygen in the arterial blood of a dog in convulsions was found to be considerably less than twice the normal quantity. Hence the author's startling conclusion, that *oxygen is the most fearful poison known.*

Taking a dog in full convulsion out of the receiver, M. Bert found the paws rigid, the body bent backward in the shape of an arch, the eyes protruding, pupil dilated, jaws clinched. Soon there is relaxation, followed by another crisis, combining the symptoms of strychnine-poisoning and of lockjaw. The convulsionary periods, at first recurring every five or six minutes, become gradually less violent and less frequent.

The author sums up his conclusions as follows: 1. Oxygen behaves like a rapidly-fatal poison, when its amount in the arterial blood is about 35 cubic centimetres per cent. of the liquid; 2. The poisoning is characterized by convulsions which represent, according to the intensity of the symptoms, the various types of tetanus, epilepsy, poisoning by phrenic acid and strychnine, etc.; 3. These symptoms, which are allayed by chloroform, are due to an exaggeration of the excito-motor power of the spinal cord; 4. They are accompanied by a considerable and constant diminution of the internal temperature of the animal.

Infant Mortality.—During the year 1868, 23,198 children under one year of age, died by convulsions in England, the number of births being 786,858—one in 34. In the same year the births in Scotland were 115,514, and only 312 infants under one year—one in 370—fell victims to convulsions. This striking difference in the mortality statistics of the two countries is accounted for in a report of the Scottish Registrar-General by the difference between the English and the Scottish modes of rearing infants. "The English," he writes, "are in the habit of stuffing their babies with spoon-meat almost from birth, while the Scotch, excepting in cases where the mother is delicate, or the child is out nursing, wisely give nothing but the mother's milk till the child begins to cut its teeth." The statistics of infantile deaths from diarrhœa may also be adduced as an argument in favor of the Scottish system. In England more than twice as many infants die of this disorder than in Scotland.

On comparing these statistics with those of the last United States census, it will be seen that the chances of life for infants in their first year are far more favorable in this country than in England, though not so favorable as in Scotland. In the year ending May 31, 1870, there were born in the United States 1,100,475 children. Of these there died, during the same year 4,863 by convulsions, and 1,534 by diarrhœa, or one in 236 from the former cause, and one in 724 from the latter. In England the deaths from diarrhœa amounted to 138 in 100,000 infants, and in Scotland to 66 in the same number. It will be seen, on computation, that the proportion of deaths from this cause are by a very small fraction less in the United States than in Scotland. But now are we to attribute these very creditable results to our more rational system of rearing children, or to the better social condition of the population here?

Snakes swallowing their Young.—The question, "Do snakes swallow their young?" that is, give them shelter in the maternal stomach when danger threatens, was discussed in a paper presented to the American Association by G. Brown Goode. The author some time since asked, through the public press, for testimony bearing on this subject, and he now comes forward with what appears to be perfectly satisfactory evidence in favor of the affirmative side. He has the testimony of fifty-six witnesses who saw the young enter the parent's mouth. Of these fifty-six, nineteen testify that they heard the parent snake warning her young of danger by a loud whistle. Two of the witnesses waited to see the young emerge again from their refuge, after the danger was past; and one of them went again and again to the snake's haunt, observing the same act on several successive days. Four saw the young rush out when the parent was struck; eighteen saw the young shaken out by dogs, or escaping from the mouth of their dead parent. These testimonies are confirmed by the observations of scientific men, such as Prof. Smith, of Yale College, Dr. Palmer, of the Smithsonian Institution, and others.

NOTES.

The year 1759, which witnessed the completion of the Eddystone Lighthouse, closed with tremendous storms, and the courage of the light-keepers was tested to the utmost. A biography of John Smeaton, the builder of the Eddystone, states that for twelve days the sea ran over them so much that they could not open the door of the lantern, or any other door. "The house did shake," said one of the keepers, "as if we had been up a great tree. The old men were frightened out of their lives, wishing they had never seen the place. The fear seized them in the back, but rubbing them with oil of turpentine gave them relief!"

Sir Charles Lyell, in his "Geology," speaking of Madagascar, says that, with two or three small islands in its immediate vicinity, it forms a zoological sub-province, in which all the species except one, and nearly all the genera, are peculiar. He singles out for special remark the lemurs of Madagascar, comprising seven genera, only one of which has any representatives on the nearest main-land of Africa. Hitherto no fossil remains of these Madagascar species have been known to exist, but M. Delfortrie, of the French Academy of Sciences, announces that he has found, in the phosphorite of the department of Lot, an almost complete skull of an individual belonging to this lemurine family.

Of the 35,170,294 passengers carried over the railroads of Pennsylvania last year, only thirty-three were killed, less than one in a million. But the English lines make a far more favorable showing, the number killed in the year 1871 being only twelve—or one in 31,000,000.

In the "History of the Fishes of the British Islands," Giraldus Cambrensis, a writer of the twelfth century, is quoted for the observation that in the Lyn y Cwn, or Pool of Dogs, in Wales, the trout, the perch, and the eel, were deficient of the left eye. A recent work on "Trout and Salmon Fishing in Wales," strangely enough, confirms in part this observation, asserting that one-eyed trout are still caught in the same waters.

Professor Smee recently, at the Berlin Chemical Society, proposed a method for detecting organic matters contained in the air, and for effecting at the same time a kind of distillation by cold. A glass funnel, closed at its narrow end, is held suspended in the air and filled with ice. The moisture of the air is condensed, in contact with the exterior surface; it trickles to the bottom of the apparatus, and falls into a small basin placed for its reception. The liquid obtained in a given time is weighed. It generally contains ammonia, which is determined by known methods. Distillation by cold may be employed for separating volatile substances which might be injured by heat. Thus, if flowers are placed under a large bell-glass along with the refrigerating funnel, a liquid is obtained in the basin saturated with the odorous principles of the flowers.

At various points on the river Thames, between Woolwich and Erith, there are visible at low water the remains of a submerged forest, over which the river now flows. This fact, taken in connection with other local phenomena, has led geologists to conclude that the present outlet of the Thames to the North Sea is of quite recent origin, the waters having formerly passed southward into the Weald by channels which still remain. Excavations in the marshes expose to view a deep stratum of twigs, leaves, seed-vessels, and stools of trees, chiefly of the yew, alder, and oak kinds.

A traveller in Zanzibar describes the red and black ants as one of the greatest scourges with which Eastern Africa is afflicted. These insects, he says, move along the roads in masses so dense that beasts of burden refuse to step among them. If the traveller should fail to see them coming, in time to make his escape, he soon finds them swarming about his person. Sometimes, too, they ascend the trees and drop upon the wayfarer. The natives call them *madinodo*, that is, boiling water, to signify the scalding sensation produced by their bite. These ants are of great size, and burrow so deep into the flesh that it is not easy to pick them out. In certain forests they are said to exist in such numbers as to be able to destroy rats and lizards.

An eccentric and methodical man is Dr. Rudolf, Danish governor of Upernavik, Greenland. Dr. Rudolf is a scientist of some distinction, and has contributed his share to the scientific literature of his own country, yet it is his choice to live in a region where darkness prevails four months in the year, and where he can have no communication with civilized life beyond the annual visit from the government storeship, and the casual arrival of whalers. By the storeship the governor receives annually a file of Danish newspapers; but instead of glancing through them hastily, he takes a fresh journal every morning, reading the *Dagblad* of Jan. 1, 1872, on Jan. 1, 1873. He thus follows, day for day, the changes in the mind of Denmark: is glad in the order in which Copenhagen is glad, and *vice versa*, but always precisely twelve months after the event.

If the white of an egg be immersed for some 12 hours in cold water, it undergoes a chemico-molecular change, becoming solid and insoluble. The hitherto transparent albumen assumes an opaque and snow-white appearance, far surpassing that of the ordinary egg. Dr. John Goodman, writing in the *Chemical News*, recommends this material for diet in cases where a patient's blood lacks fibrine. The substance being light and easily digested, it is not rejected even by a feeble stomach; and as it creates a feeling of want rather than of repletion, it promotes, rather than decreases, the appetite for food. After the fibrine has been produced in the manner described above, it must be submitted to the action of a boiling heat, and is then ready for use.

One of the great dangers attending the use of the various sedatives employed in the nursery is that they tend to produce the *opium-habit*. These quack medicines owe their soothing and quieting effects to the action of opium, and the infant is by them given a morbid appetite for narcotic stimulants. The offering for sale of such nostrums should be prohibited, as tending to the physical and moral deterioration of the race. In India mothers give to their infants sugar-pills containing opium, and the result is a languid, sensual race of hopeless debauchees. In the United States the poisonous dose is administered under another name; but the consequences will probably be the same.

During last autumn, says the *Journal of the Society of Arts*, there were no less than seventeen companies engaged in extracting gold from the auriferous sand of Finland. The alluvial deposits at Toalo are said to be extremely rich in gold, the total production last season being estimated at about $50,000. One of the companies returned a dividend of 70 per cent. The largest nugget weighed 40 grammes.