Scientific American.

ESTABLISHED 1845.

MUNN & CO., Editors and Proprietors.

PUBLISHED WEEKLY AT
NO. 37 PARK ROW, NEW YORK.

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VOL. XXXVII., No. 3. [NEW SERIES.] *Thirty-second Year.*

NEW YORK, SATURDAY, JULY 21, 1877.

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No. 81,
For the Week ending July 21, 1877.

Terms.—SCIENTIFIC AMERICAN SUPPLEMENT, one year, postpaid, *five dollars*. One copy of SCIENTIFIC AMERICAN and one copy of SCIENTIFIC AMERICAN SUPPLEMENT, one year, postpaid, *seven dollars*. CLUBS.—One extra copy of the SUPPLEMENT will be supplied ratis for every club of five SUPPLEMENT subscribers at $5.00 each.

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AN INEXACT SCIENCE.

It seems to us that civil engineering—or at least the civil engineering practice of the past—has, if we are to credit the *dicta* of modern eminent theorists, fairly earned the above title. Here, for instance, are two important works before us, respectively dealing with two great branches of the profession, hydraulic engineering and engineering constructions. The first is entitled "The New Formula for Mean Velocity of Discharge of Rivers and Canals," by W. R. Kutter, translated by Mr. Louis D'A. Jackson, a well known authority. In the preface we are told that the whole of the old velocity formulæ of Eytelwein, Stevenson, Dubriat, Prony, and others, which have been used as "the bases of calculations of discharge for tables, which are still unfortunately believed in by the unreflecting," have no claim to general application, that, in short, said formulæ are altogether erroneous; and in some cases, tabulated velocities based thereon are "but the wildest guesses at the actual velocities." Herr Kutter's formula, founded on the experiments of D'Arcy and Bazin, Humphreys and Abbot, and others, is now absolutely correct; and the other formulæ, with the aid of which our engineering forefathers apparently managed to complete some substantial work, are proscribed.

Now also comes Dr. Weyrauch; and in the preface to his admirable work on "Strength and Calculations of Dimensions of Iron and Steel Constructions," we are told that "the methods hitherto employed in calculating the dimensions of iron steel constructions have been entirely wrong; and that the security of structures, in which their results have been applied, though with great expenditure of material, is much less than supposed." And thereupon Dr. Weyrauch gives us an excellent book, demonstrating new and absolutely correct formulæ, and also proceeds to proscribe the other formulæ which have been relied upon in the building of a great many structures in years past, structures which, unaccountable as it may appear, still manifest no inclination to fall down. Dr. Weyrauch's and Herr Kutter's formulæ are, we are given to understand, fully indorsed by the engineering profession, and, we have no doubt, with excellent reason; but we should like to know what is going to be done about all the engineering work which, in the absence of any other, must have been based on these now scouted and repudiated rules. Are we to leave standing bridges and buildings, the dimensions of every member of which has been calculated wrongly, and which are only apparently secure? Or are we to remain in passive indifference to our waterworks when the chances of their breaking down and drowning us are only fortified by rules based on the "wildest guesses?"

Seriously, and while we shall not presume to say that the two eminent engineers above quoted have proceeded a whit too far in their condemnation of the old rules, we may at least question the fact whether engineers in actual practice confine themselves so closely to theoretical deductions as these and most textbook writers would have us suppose. Indeed, we think it will be found that the average civil engineer—and we may as well include the mechanical engineer with his professional relative—will prefer his own judgment and the teachings of his own experience in matters of construction, especially if both qualities have been often successfully tested, to almost any one's theoretical *dicta*. Not that we mean to say that our engineers constantly prefer thumb rules to scientific accuracy, or dash at conclusions by guesswork. On the contrary, we think, in point of the care displayed in and the exactness of their construction, American civil engineering structures will compare favorably with any in the world, while our mechanical appliances are already renowned for perfection of design. But we believe that, in most instances, if the constructors or designers were asked whose or which formulæ they followed, the large majority would assert that their experience had been taken as the principal guide upon which to found their own theory. A noted civil engineer—one who has constructed perhaps more railway bridges than any other man in the country—recently said that he had never used the calculus in his work in his life. Yet almost any textbook on strains and stresses teems with formulæ based on that abstruse branch of mathematics. The calculus is invaluable to the mathematician; but here at least is one engineer who takes the responsibility of figuring his strains and selecting his sizes of material without its aid.

Another instance: We have before us a letter from a very eminent experimenter upon the strength of metals, etc. He informs us that, by his recent investigations, involving an immense number of experiments, the report of which before many months will be made public, the deductions of Rankine and other authorities as to strength of chain are wholly wrong: that the stud does not increase the strength of links, and the strongest links are not made from the strongest bars, besides other somewhat startling deductions. Theoretically at least, then, all our chain cables have been made under erroneous rules; practically, however, they have served their purpose. It cannot be conceived that new theories in chain construction, or in any other branch of science involving constructive work, can cause materials to do more than is expressed in the latter phrase.

The columns of this journal show perhaps most clearly how much our improved forms of machinery are due to constructive skill based on practice. Take any mechanical invention, adapted by its originator after long study to achieve a certain purpose, and observe its form as first published on these pages. Search for that device five years afterwards, and it is hardly recognizable. Experience has called for more metal here, less there. Actual tests have shown what modifications of the machine are necessary to render it most efficient in its duty; and these modifications have suggested others, and so on. It can be safely stated that, in hundreds of our best mechanical appliances, their capabilities were not known until they were tested. Furthermore, the rule, in nine cases out of ten here (we know it is not so abroad), is that a mechanic wanting a new special machine for a special purpose will design the apparatus which his experience tells him is best adapted to his ends, and test it to determine its duty, search for failings, improve it, and so work up to the desired point; and this he will do while his brother of the Old World is puzzling over his drawing board to discover how, by Greek letter formulæ and the differential calculus, he can produce a device that will give the wished-for results on the first trial. We have a profound respect for theory, for it should be the essence of a vast amount of practice. But if we had a bridge to build, we would prefer the man who had already built half a dozen successfully to the theorist and mathematician who had constructed none except on paper in his study, and those according to formulæ which may be repudiated a year hence.

THE GREAT PROBLEM.

That the earth was at one time incapable of sustaining life, and that at some time in the course of events life began to be, no one doubts for a moment. It is also pretty generally admitted among scientific men that the beginning of life was in all probability a natural event; and that the earlier forms of life did not embrace the more complex types now existing, but were of simpler structure, perhaps not unlike the lowly organisms now studied under the microscope.

Here the question arises: Was the beginning of life a phenomenon single and unique, and are the bacteria of today the unaltered descendants of the earliest forms of life? Or may life have began, and may it still begin, at any time by the concurrence of suitable conditions? This is by all odds the most important question now before the scientific world; and curiously the most strenuous opponents of the theory that life may begin now as well as ever, are found among those who, like Professor Tyndall, believe life to have been derived originally from purely material combinations. That matter should have lost any of its intrinsic "power and potency" in the course of ages seems altogether unlikely; so we must infer that the active opposition of the leading exponents of evolution to the theory of the recent evolution of life *de novo*, arises from pure loyalty to truth experimentally determined. Spontaneous generation is the logical outcome of evolution; but they will not admit the fact until it has been demonstrated beyond the possibility of a doubt.

At first thought this might seem to be a question of speculative interest merely: but it is far more than that. Some of these minute and apparently primary forms of life are among the most potent factors of human health and disease, and of the health and disease of the animals and plants most intimately connected with our sustenance and general wellbeing. Even the air we breathe seems at times to be contaminated by their presence; our blood is poisoned by them, and the struggle for existence rises or degenerates into a struggle against them. It is no wonder then that the question of their origin is one of the highest practical as well as popular interest, or that the foremost men in biological science have essayed its solution.

Years of critical investigation have stripped the problem of many confusing and irrelevant conditions until it stands nakedly thus: Can we take matter which contained no life, perfectly isolate it from possible impregnation, and subject it to conditions under which it will bring forth objects that live and multiply? If so, what kind of matter must be used, and what are the conditions favorable to such origination of life?

Thanks to the labors of many of the acutest minds in experimental science—among them Pasteur and Pouchet, in France; Huitzinga, Cohn, Klebs, Bilbroth, in Holland, Austria and Prussia; Mantegozza, Cantoni and Oehl, in Italy; Bastian, Lister, Sanderson, Tyndall, Dallinger and Roberts, in England; Wyman and others in our own country, with any number of less eminent investigators—the primary conditions of the problem have been satisfactorily mastered. It is admitted by all that by subjecting matter to a sufficiently high temperature it can be entirely freed from life and life germs. It is admitted that the all-pervading germs of life cannot pass through a sound plate of glass; consequently any substance to be tested can be kept perfectly isolated by hermetically sealing the vessel containing it. Other successful methods have been employed; but this is the most exact ing, and is beyond question or suspicion when used with reasonable care. It is admitted also that the temperature at which putrefaction ordinarily takes place most actively is a proper temperature at which to keep the fluids under examination; and fluids must be used since they are the natural habitat of bacteria.

The question, What is a killing temperature? has been very hard to settle; that is, a temperature high enough to surely destroy life, yet not so high as to endanger the chemical composition of the solutions to be tested. A comparatively low temperature suffices to kill bacteria, and so far as positively known, bacteria multiply only by fission. They may, however, multiply also by means of invisible germs; and since many germs are known to withstand a higher heat than the developed forms, a higher temperature than suffices to kill bacteria must be insisted on in all experiments in this field. Precisely how much higher, the opponents of spontaneous generation leave an open question, and increase the demand with every failure of a supposed killing heat to prevent the development of life. A series of experiments made by Bastian some years ago, and never in any way invalidated by conflicting experiments, would seem, however, to have placed this question beyond further dispute, though his opponents are very apt to quietly ignore them. His method was briefly this: to take a test-fluid which never engendered life except when purposely inoculated with multiplying bacteria, and never failed to produce life when so inoculated; then after mixing the test-fluid with a bacteria-bearing liquid, heat the mixture after perfect isolation, and await the result. If life appeared, the heating was assumed to be insufficient to kill all the life introduced; if no life appeared, after repeated trials, the temperature was considered fatal. The heat which produced sterility in different fluids varied between 130° and 158° Fah., and the latter was found to be the maximum temperature which a growing and multiplying swarm of bacteria,—in all stages of growth and presumably including the hypothetical germs, if such there be,—could survive. When, therefore, this experimenter found other fluids (capable of spontaneous putrefaction in the open air) to swarm with bacteria after having been subjected to a temperature of 212° Fah. for hours, and thoroughly isolated from any possible contact with germ-bearing air, he claimed to be justified in the conclusion that there had been in such fluids a real origination of life not derived from antecedent life.

A great variety of animal and vegetable infusions, and some purely inorganic solutions have been used in such experiments, with varying results. While it has been found to be impossible to predict the behavior of any particular fluid from its composition, it appears from many observations that neutral or faintly alkaline fluids are more likely to putrify, after boiling and sealing, than acid infusions. Excess of alkali, however, prevents the development of life as well as an excess of acid; and some infusions are found to be almost always sterile unless exactly neutralized; yet positive results have been obtained in many instances, Bastian claims, with both acid and alkaline fluids. The fluids which he found to be most commonly successful were infusions of turnip and hay; the former fortified with a little cheese dust being almost certain to develop life.

It was with infusions of this character that the sceptical Dr. Burden-Sanderson tested Dr. Bastian's conclusions by a series of experimental tests made under his opponent's personal supervision; and failing to detect any flaw in the process, he frankly and publicly admitted that he had been mistaken in his previons doubts. The same experiments were thereupon repeated, and their results verified by Professor Huitzinga, of the University of Groningen, who afterwards made a series of fresh trials, using a mixture of grape sugar and soluble salts instead of the turnip infusion, and substituting soluble peptone for the cheese dust. In every instance, he declares, bacteria appeared when the ingredients were used in certain specified proportions. By altering the proportions of the ingredients he was able to keep his solutions sterile, although in other respects they were treated exactly as before; and he relied on this differential process to prove that any pre-existing germs in the liquid were destroyed and perfect isolation obtained, for the altered fluids were found capable of developing bacteria if once inoculated. Thus having two fluids, each capable of supporting bacteria, both were subjected to the same process of boiling and sealing. After repeated trials, in which one solution invariably swarmed with bacteria and the other as invariably remained clear, it was inferred that all germs had been destroyed by the heat and subsequently kept out by perfect sealing; and that the life developed in the one was due wholly to its proper chemical composition.

The only reply that the opponents of spontaneous generation can make to tests like these is that, in all cases where life appears, there has been either an insufficient exclusion of germs, or else that, owing to the composition of the solution, the contained germs were somehow protected and enabled to withstand a temperature which was fatal to those in the other solutions. Thus there always remains a possibility of doubt, and the result is disputed. A pretty illustration of the almost hopeless struggle which the spontaneous generationists have to wage with persistent doubt is seen in the last controversy between Bastian and Pasteur.

For a number of years Pasteur has held, in spite of Bastian's experiments in regard to the death-point of bacteria, that bacteria germs are not killed in acid fluids below the boiling-point of water, while in neutral or faintly alkaline fluids they are able to survive a somewhat higher temperature. In any case, however, he admits that a temperature of 110° C. is fatal; consequently no life can appear in fluids so heated and kept perfectly isolated. A short time ago, Bastian devised the following test of the alleged protective influence of alkalinity. He placed in a retort a measured quantity of urine of ascertained acidity; also in a small tube a quantity of liquor potassæ, somewhat less than enough to neutralize the urine—his experiments proving that a larger quantity was likely to overdo the matter. He then sealed the potash tube in a blow pipe flame, drawing out the sealed end so that it would break easily, and boiled the inclosed liquor potassæ to kill any possible germs it might contain. Then he placed the tube in the retort with the urine, which was boiled and hermetically sealed in the usual way.

The question to be decided was this: Acid urine does not putrefy (that is, develop bacteria) after boiling; alkaline urine sometimes does—the difference being attributed by Pasteur and his school to the protective action of the alkali. Now, if acid urine after boiling (by which process its inclosed germs are admitted to be destroyed) be neutralized by a liquor also made sterile by prolonged boiling, it ought not to develop life; if it does, the result cannot be due to any protective action of the alkali, which is not added until after the germs are killed.

After cooling, retorts as above described were shaken so as to break off the fragile end of the inclosed potash tube and allow the contained fluid to mix with and neutralize the boiled urine; then they were placed in an incubating bath kept at a temperature about 122° Fah., together with other retorts similarly treated, except that the potash tubes were not broken. After a day or two, the neutralized urine invariably putrefied, while the acid urine in the control flasks remained permanently barren. Having thus shown that the effect was the same, whether the acidity of the urine was reduced after or before boiling, Bastian insisted that the influence of the potash was not due to its alleged power of protecting the germs so as to enable them to bear a higher temperature, but to some vivifying chemical or molecular action, and claimed a victory for spontaneous generation.

Pasteur repeated the experiment with a like result, but was not satisfied; so he tried again, substituting solid potash for the liquor potassæ, and the result was negative; to which Bastian replied that the potash had been used in excess. Pasteur retorted with a charge of insufficient heating of the potash in Bastian's experiments, and challenged him to repeat the experiment with the single variation that the potash tube should be heated either for twenty minutes at 110° C., or for five minutes at 130° C.

Bastian did so, and more: he kept the potash tubes at the required temperature for twenty *hours*, and obtained life as before. To this Pasteur's only reply was a vehement protest that the alleged result was impossible.

The English opponents of spontaneous generation have more wisely endeavored to prove the impossibility of such a result by critical experimentation. Among these, Dr. Roberts, of Manchester, and Professor Tyndall, both claim to have repeated the experiments over and over again, and always without obtaining life. This is not the first time that Dr. Bastian's assertions have been flatly contradicted; and he knows from experience what it is to have his opponents afterwards admit, in the face of experimental evidence, that he was right and they were wrong. The issue has been so narrowed now that it ought not to be impossible for the opposing parties to agree to undertake together a series of crucial experiments. By mutual suggestion, skill, and watchfulness, the common charge of inexactness in manipulation, or prejudice in the interpretation of results, ought easily to be prevented, and the issue fairly and squarely met. Isolated partisan work, however skillful, will not force a conclusion nearly so quickly or satisfactorily as united effort; and to that test we hope, for the credit of science, the question will be speedily submitted.

THE PHYSICAL CHARACTERISTICS OF AMERICAN CHILDREN.

Some months ago we renewed the data relative to the height and weight of Americans, drawn from the records of the Provost Marshal General's Bureau, made during the late war. The conclusions reached were that, in point of stature, even the lowest mean obtained would entitle the American people to the first rank among nations. The results of measurements of nearly a million and a half of American born white men exhibited a mean stature of 67·646 inches. In the matter of weight it further appeared that Brother Jonathan is as heavy as the heaviest even in his youth: and the apparent slimness of his immaturity, due to his superior height, is fairly made up by the time he reaches his full development.

The above important series of investigations on the physical characteristics of Americans, has recently been supplemented by researches made by Dr. H. P. Bowditch, of Boston, Mass., in which the object of inquiry has been the growth of children. Both sexes are included, so that a direct comparison is instituted between them. About 24,500 observations were made upon the pupils of the schools in Boston and vicinity. These were tabulated according to nationality, etc., and from these tables graphic diagrams were constructed, exhibiting with great clearness the results obtained. Both tables and diagrams, together with Dr. Bowditch's report, are before us, in the recently issued Eighth Annual Report of the State Board of Health of Massachusetts. From the data obtained relative to the comparative rate of growth of the sexes, it appears that the greatest annual increase in height occurs for girls at 12 and for boys at 16 years of age, while the maximum increase in weight is, for boys at the same age, and for girls one year later than the maximum increase in height. In other words, at about the ages of 13 and 14 years girls are (during more than two years) both taller and heavier than boys at the same age, though before and after that period the reverse is the case. Dr. Bowditch points out, that on the principle enunciated by Carpenter and Herbert Spencer, that growth and reproduction are to some extent antagonistic properties, it may reasonably be supposed that at the age at which the organism becomes potentially reproductive, a period of excessive growth will not occur; and the data above noted seem to show that this is the case.

From the tables exhibiting effect of race on size and rate of growth, it appears that, almost without exception, American children are both taller and heavier than children of the same age and sex whose parents are of other nationalities. One of the most curious facts brought out by Dr. Baxter in the investigations referred to in our initial paragraph, was that natives of foreign countries enlisting in the United States, possessed a greater average height than natives of the same countries enlisting at home. Dr. Baxter explained this circumstance by a difference in the average age of the individuals mentioned, but Dr. Gould, through other statistical investigations, has shown that, even making allowance for this difference of age, the same result holds true. Now, Dr. Bowditch presents the opinion that the superiority of stature is owing to the greater average comfort of the people of this country, as compared to that of inhabitants of European States, and the observations of Quetelet, Villerme, and Cowell, which are referred to, seem further to show that in a given community the children of the wealthier classes are, as a rule, larger than those of the poorer classes.

But it is evident that an important question is, whether similar conclusions to those reached by Drs. Baxter and Gould for adults are applicable to growing children, and in order to eliminate the possible effect which comfort or misery may have on the rate of growth, it is necessary to select for comparison, sets of observations made upon children belonging to corresponding classes in the communities in which they live. To this end, the pupils of certain select schools in Boston were compared with those belonging to non-laboring classes attending English public schools and universities; and the two sets of figures show the marked superiority of the American boy, both in size and weight. Hence the superior size of American children may be taken as due partly to the greater comfort surrounding them, and partly to difference of race and stock.

One of the most interesting portions of the entire investigation is that which deals with the relation of height to weight in growing children of both sexes and of various races. Growing boys are heavier in proportion to their height than growing girls, until the height of 58 inches is reached. Above that point the reverse is the case. The difference between children of American and those of foreign parents is constant in one direction for all ages. Boys of German parentage, who are uniformly heavier in proportion to their height than American boys, form the exception to the rule. The deprivation of the comforts of life, curiously enough, exercises a greater tendency to diminish the stature than the weight of the growing child. And finally Dr. Baxter's conclusion "that the mean weight of the white native of the United States is not disproportionate to his stature," is, as far as boys are concerned, as applicable to growing children as to adults.

Dr. Bowditch appends to his report reference to the formulæ determined by Professor Lanza; and based on the observations of President Runkle, of the Massachusetts Institute of Technology, expressing the relation between the weight and height of growing children from five to eighteen years of age. In the case of boys ranging in height from 42 to 66 inches inclusive, the formula is $y=0\cdot002428\ x^{2\cdot69}$; and in that of girls ranging in height from 42 to 61 inches inclusive, the expression is $y=0\cdot001277\ x^{2\cdot75}$, y representing the weight in pounds, and x the height in inches, in both cases. These formulæ are quite accurate, as the greatest difference between calculated and observed values is, in the case of boys, 0·65 lbs., and in that of girls, 1·41 lbs., with one exception, where it is 3·01 lbs.

Dr. Bowditch's investigations are replete with suggestions for future statistical research, the results of which can scarcely fail to be of the highest value to the community. To those who may undertake the necessary inquiries, the following subjects are commended. Drs. Gould and Baxter having shown that the size of adult Americans is very different in different States of the Union, and even in different parts of the same State, it would be interesting to determine, by observations of children, how early in life this difference becomes apparent; in general, what is the influence of geographical and climatic conditions of growing children? What number of generations is necessary for the complete development of the influence of changed climatic conditions on the rate of growth of a given race? This examination might be conducted on emigrants and their descendants, coming from some limited region of the old world. What effect (if any) does the season of the year have on rate of growth? And what is the comparative effect of city and of country life on the same? What is the relation between diseases and the rate of growth? Dr. Bowditch suggests that it would be especially interesting "to inquire whether in the rapid growth, which is said to follow certain diseases, especially fevers, the height and weight increase in normal ratio: whether this accelerated growth *after* the disease is simply a compensation for a retardation *during* the disease; whether abnormally rapid growth causes a predisposition to disease, and whether any connection can be traced between the rate of growth and the frequency with which certain diseases of growing children (*e. g., chorea*) occur at different ages." Finally, by systematic comparative study of the physique of the growing population in different localities, the effect of local hygienic conditions might be determined.

IN our SUPPLEMENT of this week particulars and engravings are given of a remarkably powerful and fast little steam launch designed by H. S. Maxim. Boiler 26 inches length, 20 inches diameter. Test pressure, 450 lbs. per inch. Length of boat, 21 feet. Speed, 10 miles per hour.