ted, as his has been essentially a scientific career. For a number of years past he has held the office of President of the Andersonian University, in Glasgow. Surrounded by the members of his own family and by those of his lamented friend Livingstone—for he has really been *in loco parentis* to the children of the African traveler—Mr. Young, for whose portrait we are indebted to the *Practical Magazine*, now spends the great bulk of his time at his beautiful estate of Kelly, near Greenock, Scotland, or at his no less fine and romantic estate of Durris in Aberdeenshire. But he also mixes to some little extent in public life, contributing liberally to all movements of a patriotic or charitable character, and aiding by every means within his power the progress of scientific knowledge.

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VELOCITY OF NERVOUS IMPULSES.

In his suggestive lecture on the sun, our English visitor, Mr. R. A. Proctor, makes use of several striking illustrations to give an idea of the immense distance between us and our great luminary. One of these supposes an infant with an arm of the inconvenient length of ninety-one millions of miles, who should stretch forth his hand and touch the sun. Naturally the darling would have his finger burnt; but, so slow is the transmission of feeling, he would have to wait until he was a hundred and thirty-five years old before he could be conscious of the fact. In this estimate Mr. Proctor evidently adopts the rate of nerve motion obtained some twenty years ago by the observations of Dr. Hirsch—that is, about one hundred and eleven feet a second. The later and more elaborate researches of Dr. Schleske show a rapidity of conduction by the sensory nerves of about ninety-seven feet a second, which would require our sunburnt infant to wait some years longer before discovering his indiscretion. If he trusted his sight in the matter, he might become aware of the danger of his distant member in the short space of eight minutes, so much more rapid is the speed of light than the movement of feeling along the nerves. The passage of volition along the motor nerves appears to be still slower; so that upwards of a century and a half, perhaps, might elapse before the mental order to withdraw the finger could be carried out.

However slow the rate of nervous movement may be, as compared with the velocity of light or the still fleeter motion of electricity, it is nevertheless so rapid that until quite recently it was thought to be immeasurable, within the limited range in which our observation of it is possible. The most widely separated points in the course of any nerve allow but a few feet of difference at best for timing the periods of sensation or volition; and the nervous impulse travels so quickly that such small distances would seem to be wholly annihilated. To our consciousness a prick on the great toe is discovered as promptly as one on the cheek; and it is only by the intervention of the most delicate and ingenious of mechanical contrivances that the difference in time is made apparent.

The first step toward making the solution of this interesting problem possible was taken in the antiphysiological art of gunnery. In the development of that art, it became necessary to measure the speed of projectiles, both in the gun and during the several stages of their flight. For this purpose Pouillet's chronoscope was devised, by means of which an electric current was made to indicate the duration of the most rapidly transient processes. Thus the passage of a bullet along the barrel of a gun was found to occupy the hundred and fiftieth part of a second. It quickly occurred to Helmholtz that here, possibly, was a means of measuring the speed of nervous action. His application of the method was too complex for description in this place; it was, however, so trustworthy as to leave no doubt of the practical accuracy of its results. His object was to measure the intervals of time, if there were any, between the excitation of a nerve at two different points and the corresponding contractions of the muscle. The difference between such intervals would, of course, be the time required for the passage of the nervous impulse over the space between the two points of excitation. Experimenting with the leg of a frog, two sets of observations were obtained, differing from each other by a small but constant quantity. For the more distant point of excitation, a measurable fraction of a second longer was uniformly required to make the muscle contract. The difference of distance being exactly measured, the rate of propagation of the nervous impulse was easily calculated. Instead of rivaling the velocity of electricity, as had hitherto been supposed, the rapidity of conduction in the motor nerves of the frog was found to be no more than eighty five feet a second. All this was as early as 1851. To test the accuracy of the result thus obtained, Professor Helmholtz devised another and more simple apparatus, which he called a myographicon. In this the contracting muscle was made to directly register the beginning and successive stages of the contraction by means of a style working against a rotating cylinder. This confirmed the general correctness of the estimate obtained with Pouillet's apparatus, the rate demonstrated being a little over 89 feet a second.

Various improvements of the myographicon were soon suggested by Du Bois Raymond and others, whose observations, while differing slightly in result, were not conflicting with previous results, due allowance being made for temperature and other disturbing conditions. The maximum rate obtained by the last named observer was 30 meters a second, or 98½ feet. This was the estimate on which he based his widely quoted illustration of the harpooned whale. If one of these sea monsters, a hundred feet long, were struck in the tail, he said it would take a full second before the sense of pain could reach the victim's brain; and, omitting the time necessary for perception and volition, another second must pass before an order could be telegraphed to the tail to retaliate by upsetting the harpooner's boat.

In all the experiments on motor nerves thus far, the leg of a frog had been used. In 1867, Baxt and Helmholtz applied the test to man, using an improvement of the myographicon suggested by Du Bois Raymond. The result gave the rate of conduction for the motor nerves of man, corresponding to that already obtained by Hirsch for the sensory nerves. A very careful series of experiments by the same observers, in the summer of 1869, showed a mean rapidity for the motor nerves in man very much greater, or about 254 feet a second. But this by no means invalidated the result already obtained, since, as Helmholtz had shown, the rate varies greatly with temperature, being not more than one tenth as great at 32° as at 60° or 70°.

More recently it has been established by Dr. Munck that the velocity of nervous impulses is different in different nerves, and in different parts of the same nerves, the rapidity increasing as the termination of the nerve is approached, and by Marey's observation, that fatigue of the muscles has the effect of seriously reducing the rate of nervous conduction; while Wittich has found that the rate is in some degree dependent on the mode of excitation, being greater when electricity is used than when the stimulus is mechanical. The same observer also reports a considerable difference between the rates of motor and sensory nerves, the latter excelling by at least a third.

The measurement of the rate at which the nervous impulse travels brainward necessarily involves a process very different from any employed in the study of the motor nerves. The problem was first attacked by the Swiss astronomer Dr. Hirsch. Soon after Helmholtz took up the other branch of the investigation, and his solution of it was as ingenious as it was successful. It involved the measurement, with the delicate chronometric instruments employed by astronomers, of the difference in time between the appreciation of impressions made at a distance from the brain, say on the great toe, and others nearer, as on the cheek. Roughly described, the plan adopted was substantially this: The observer sat with his finger on a signal key, with which he announced the perception of an electric shock as soon as possible after feeling it, thus closing an electric circuit which had been broken by the shock. The minute interval between the breaking and closing of the circuit measured the time taken by the transmission of the shock to the brain, the time required for the perception of the sensation, time for willing the movement of the signal key, time for the transmission of this volition to the proper muscles, time for the contraction of the muscles, and finally the time lost in the physical process of signaling. Obviously all these parts, except the first, must be substantially the same in all experiments by the same person, using the same finger for making the signal. Any difference in the whole time must therefore be owing to the greater or smaller distance of the particular point of impression from the brain. This difference being measured with tolerable exactness, it is possible to calculate pretty closely the rate at which the nervous impulse is transmitted. The estimate first made by Dr. Hirsch was, as already noted, 111 feet a second. More recent determinations give averages ranging from 97 feet, by Dr. Schleske, to 136 feet, Wittich's estimate for a nervous impulse excited by electricity. With a mechanical stimulus, he found an average velocity of 124 feet. These figures, of course, are to be taken relatively. The rate varies in different individuals, and, doubtless, in the same individual, with varying conditions of health, temperature, and so on, the general average being about that of a high wind, a race horse, or a locomotive. Light excels it about ten million times, and electricity more than fifteen million times.

But, it may be asked, what is the use of all these investigations? Of what account is a delay of the hundredth part of a second, more or less, in the perception of a sensation or the transmission of a volition, so long as we are not conscious of it? In astronomy, it has proved to be of material account; and it is more than probable that the knowledge of the normal rate of nervous impulses thus obtained may some day be of the greatest help in the diagnosis of nervous diseases.

With the nicest appliances for observing and timing phenomena, there still remain discrepancies between the reports of different observers, however skillful. Time is required for the act of perception, for willing the pre-determined signal, and yet more for executing the volition, all of which directly affect the accuracy of the observation; and since these intervals differ with different observers, the exact moment of an occurrence cannot be fixed without knowing and allowing for them.

THE AUTOPSY OF PROFESSOR AGASSIZ.

Dr. Morrill Wyman, of Cambridge, Mass., has published a report on the autopsy recently made upon the body of Professor Agassiz, from which it may be deduced that the disease to which the great naturalist succumbed was one of long standing. The arteries at the base of the brain showed evidence of extensive chronic disease of their lining membrane, and also several important changes which were fatal. In the left ventricle at the lower third, a firm, organized clot, of the size of a peach stone, attached to the wall at the anterior portion near the septum, was found, and around this clot a more recent one had formed, its center softened and granular. From this, probably some small portions had been carried by the blood to the arteries at the base of the brain, doing their part in obstructing them and causing the fatal alterations above noted. The lungs showed evidence of old inflammation. The entire weight of the brain was 53·4 ounces avoirdupois, and its greatest weight, between the ages of 35 and 40 years, was estimated at 56 5 ounces.

Without entering into the technical details of the investigation, the result shows that the trouble began with inflammation of the lining membrane of the lungs, and that the morbid processes, carried by the blood from heart to brain, there disorganized and checked the circulation. The malady was too deeply situated to have admitted of surgical aid, nor could any effort of human skill have averted death from its effects. The autopsy was made in the interests of science and in deference to the expressed wishes of Professor Agassiz, long since placed on record.

MICROSCOPIC CRYSTALS IN PLANTS.

Besides the familiar bundles of needle-shaped crystals, called raphides, dispersed throughout the cellular structure of certain plants, there are in the seed covers and leaves of several orders of plants, and in the pods of the bean family, multitudes of prismatic crystals of extreme minuteness, which have hitherto escaped detection. In the horned poppy, these crystals are as small as the 8,000th of an inch in diameter. In the gooseberry and elm, they are $\frac{1}{3000}$ of an inch; in the black currant, about half as large; in the black bryony, they are about $\frac{1}{1500}$ of an inch in diameter, thickly set at regular distances throughout the seed covers. In the gooseberry, they are so distinctly and regularly placed in the outer skin—each crystal in a separate cell—that they present the appearance of crystaline tissues. In plants of the bean family, the size is variable, the average being about $\frac{1}{3000}$ of an inch. In the garden pea, they are much larger These crystals appear to consist chiefly of oxalate of lime, sometimes carbonate. Raphides are mainly phosphate of lime.

Plants most relished by animals are found to be especially rich in these microscopic crystals. In a piece of the midrib of a clover leaflet, $\frac{1}{70}$ of an inch in length, Mr. Gulliver, who has added more than any other to our knowledge of these minute but important products of vegetable action, has counted 10 chains of crystals with 25 in a chain, making 250 in all, or no less than 17,500 to the inch. In like manner 21,000 crystals were reckoned for one inch of the sutral margin of a single valve of a pea pod. The pod had four such margins, each three inches in length; so that in a single pod there must have been as many as 250,000 crystals. In view of the marvelous number of these crystals, as well as their regularity and constancy, Mr. Gulliver believes it no longer possible for physiologists to maintain that such structures are accidental freaks of nature, of no relation to or value in the life and use of the species.

THE FIRELESS LOCOMOTIVE.

Mr. Richard H. Buel, a well known consulting engineer in this city, has recently published in the *Railroad Gazette* an account of a trial trip with one of the engines of the Fireless Locomotive Company. This article is interesting as being the first in which the theory of the action has been fully set forth. We have, on several occasions, made mention in our columns of the fireless locomotive, and have pointed out the advantages it possesses in many cases, such as greater comparative safety, less need of skilled attendants, and the absence of smoke and other products of combustion. Mr. Buel, in the article referred to, demonstrates that the locomotive can be operated successfully, if properly designed and managed; and he points out such improvements as seem to be desirable. We give a brief summary of the principal statements, omitting all mathematical work:

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The locomotive with which the experiment was made consists of a platform set upon a four wheeled truck, carrying a cylindrical reservoir 37 inches in diameter and 9 feet long, with a steam dome 1 foot in diameter and 2 feet high. The steam space of the reservoir is connected with a pair of vertical engines, each having a diameter of 5 inches and a stroke of 7 inches; a 2 inch pipe, perforated with small holes, runs the whole length of the reservoir, near the bottom. In charging the reservoir this pipe is connected with the steam space of a stationary boiler, and steam is admitted until the pressure in the boiler and the reservoir are the same. The locomotive is then ready to run, and will continue to move until the water in the reservoir has cooled down so much as to be incapable of furnishing steam of a working pressure.

In making the trial trip, the pressure in the reservoir at starting was 142 pounds per square inch; and at the end of 49 minutes, during 35·5 of which the engines were in motion, it had fallen to 22, giving a mean pressure during the run of 81·5 pounds per square inch. At the start, the reservoir was half full of water. Indicator diagrams were taken during the run, and such *data* were noted as were possible. From these, it appears that the whole distance run was about 4·5 miles; the average horse power developed by the engines was 3·61, and the number of pounds of water evaporated in the reservoir, calculated from the indicator diagrams, 147. The writer then shows that if the engines had been designed in accordance with the best modern practice, the distance run by the locomotive, with this same evaporation, would have been from 2 to 2½ times as great. A calculation is then given, showing that, with a reservoir of the same size and engines about one and a half times as powerful as in the actual case, starting with a pressure in the reservoir of 275 pounds per square inch and ending with a pressure of 20, the locomotive might be expected to continue in motion for nearly six hours before the reservoir required recharging.

THE ORIGIN OF THE DIAMOND.

If we can trust a paragraph just now going the rounds of the press, the "diamond in the sky" of the nursery verses must be taken not as a happy comparison but as a genuine prophecy of scientific discovery.

It—that is, the paragraph—gravely alleges, on the strength of some supposed philosopher's opinion, that diamonds are in all probability a cosmic product—chips of original creation, so to speak—which the earth has picked up in the course of her travels through space; in short, that they are of meteoric origin. To the popular mind there must be something plausible in the suggestion, else it would not have been so favorably received by so many intelligent editors, ever on the alert for bits of valuable scientific information wherewith to regale their intelligent readers. Indeed, what could be more plausible to those whose knowledge of the diamond is embraced by the one word, carbon, and whose acquaintance with it is limited to some little familiarity with the appearance of the cut gem? How pure, how hard, how brilliant! What fitter product could there be of the heavenly spaces? But facts are earthly and very stubborn, prone ever to take the shine out of splendid theories. It is true that the diamond is a puzzle even to chemists; that the mode of its formation is a mystery; that even its place in the order of Nature is a matter of doubt. Like amber, it is found among minerals. Amber is known to be a vegetable product; and the diamond is thought by some to show strong evidence of a similar origin.

Its antecedents are mysterious, it must be admitted, but not wholly dark. Enough is known to make it certain that the notion of its cosmic origin is not to be seriously entertained, unless one is prepared to accept at the same time the farfetched, germ-bearing meteor which Sir William Thomson suggested as the importer of life to our previously lifeless planet. In no other way, barring the earthly production of the gem, can we account for the presence of plant germs in the bodies of diamond crystals. Where in extra terrestrial spaces could the diamond, now at Berlin, have picked up its enclosed organic forms, so closely resembling *protococcus pluvialis?* Or that other diamond its chain of green corpuscules, like *polinoglæa macrocca?*

As surely as flies in amber prove the presence of animal life during some stage in the formation of that singular substance, the vegetable organisms found in diamonds are proof that these gems were formed amid surroundings not inconsistent with the presence of vegetation, perhaps in water: a supposition that finds support not only in the fact of their occasional inclusion of organic matter, but still more in the presence of *dendrites*, such as form on minerals of aquatic origin, in a diamond belonging to Professor Goppert. Crystals of gold, iron and other minerals have also been found inside of diamonds; still other diamonds are superficially impressed by sand and crystals, which leads some to believe them to have been originally soft; but it is quite as probable that these foreign substances may have interfered in some way with the perfect development of the diamond crystals, forcing them to grow around or partly around the obstructions.

Thus, even in its crystaline condition, the diamond is not always such a simple body as is popularly supposed. The writer of the paragraph in question speaks of it as "pure carbon crystalized," fit product of pure matter in pure space. So it is, sometimes, but it is also stained with impurities at times, and tinged with color, a thing of grades and degrees. And lower down in the scale are the imperfectly crystaline forms, known as boart and carbonado, harder than the true gem, but cruder and possibly more useful. It would be as correct to judge the common mineral quartz solely from its appearance in what is known as Brazilian pebble, as the diamond solely from the flashing brilliant. One exhibits no greater range of grades and shades and qualities than the other.

Though supremely beautiful in its best estate, the diamond appears to be but an earthly product, after all, subject like everything else, even theories, to earthly imperfections There may be a diamond factory up in the sky somewhere but the evidence of it is not strong. Arizona, even, promises a better field for exploration.

THE DEATH OF DR. LIVINGSTONE.

Information has recently reached England of the decease of Dr. Livingstone, the celebrated African explorer, during June last. It seems that, in journeying over a partially submerged country, he was obliged to wade some four days through quite deep water. The exposure brought on a severe attack of dysentery, of which he fell the victim.

David Livingstone was born near Glasgow, Scotland, in the year 1815, and at the age of twenty-five became one of the agents of the London Missionary Society in Southern Africa. During the sixteen years of his residence in that country, he traversed the region from the Cape of Good Hope to 10° south latitude, and then followed the Zambesi river to its mouth, thus completing a journey of over 11,000 miles. Returning to England, he organized a small expedition which set out in 1858, and returned in 1863, after further exploring the above mentioned country. In 1868 Dr. Livingstone again went back to Africa, and again entered a region totally unknown to civilization. Until found by the *Herald* reporter Stanley, some two years ago, little was heard from him, and numerous rumors of his death were extensively circulated. After Stanley's departure, he continued his exploration, but no news of him has been received until the present time, when the British officials at Zanzibar transmit the intelligence of his death.

It would be difficult to describe the labors of this most indefatigable of travelers in the space here at our disposal. In his death geographical science loses one of its most persevering students. It may be truly said that for a blank spot on the map of Africa—for a region unknown save through tradition—he has substituted a country rich, fertile and productive, which, before many years, will exercise no small effect upon the commerce of the world. His labors toward the suppression of the slave trade are well known, and have tended largely to limit the spread and decrease the barbarities of that infamous traffic. He resolutely refused to discontinue his work until he should believe it complete; and so, isolating himself from home and his own race for nearly a quarter of a century, he has existed among the savages, enduring privations without number. Though to many his toil may appear fruitless, and the years of patient search, barren in directly useful results, the world is nevertheless the gainer by the example of "one who loved his fellow men," who, single hearted in his devotion, died as he had lived, a martyr to science.

STEAM BOILER EXPLOSIONS.—THE WORK ACCOMPLISHED BY THE UNITED STATES COMMISSION.

We have received many inquiries of late as to what has been accomplished by the Commission appointed to investigate the causes of steam boiler explosions. The preliminary report of this Commission has just been transmitted to Congress. Below we give a summary of the principal points:

The following Commissioners were appointed to conduct the experiments: D. D. Smith, Supervising Inspector-General of Steam Vessels, President; Charles W. Copeland, of New York city; Benjamin Crawford, of Alleghany City, Pa.; Isaac V. Holmes, of Mount Vernon, O.; and Francis B. Stevens, of New Jersey. Mr. Stevens having declined to serve, J. R. Robinson, of Boston, Mass., was appointed in his place. The Commission above named held their first meeting on June 25, 1873, and in September issued circulars to scientific men and engineers, asking for expressions of their views. In these circulars they state the various theories of steam boiler explosions:

1. Gradual increase of steam pressure.
2. Low water and overheating of the plates of the boiler.
3. Deposit of sediment or incrustation on the inner surfaces exposed to the fire.
4. The generation of explosive gases within the boiler.
5. Electrical action.
6. Percussive action of the water, in case of rupture of boiler in the steam chamber.
7. The water being deprived of air.
8. Spheroidal condition of the water.
9. Repulsion of the water from the fire surfaces or plates.

The Commission also issued a circular, asking that safety valves be sent to them for test.

They received numerous replies to their first circular, which they state contained valuable suggestions. More than twenty safety valves were sent to them, both from the United States and abroad.

The Commissioners divided themselves into two committees, the eastern and western, the first to make arrangements for conducting experiments at Sandy Hook, and the second at Pittsburgh. There were five boilers, with the necessary connections, at Sandy Hook, which had been placed there by Mr. Stevens, and these were presented to the Commission by that gentleman. A bomb-proof was erected, the pipes were re-arranged and extended, a blower engine and blower were set up, an old steamboat boiler was connected, and four ordinary range boilers were set up. Gages were procured, and were compared with each other and otherwise tested for several days. On the 7th of November, 1873, the Commissioners commenced their experiments.

A boiler was tested by hydrostatic pressure to 182 pounds per square inch. A pyrometer was arranged so that the temperature of the crown sheet could be ascertained. Steam was raised to 50 pounds per square inch, and the water was blown off below the crown sheet. When the temperature of the latter had reached 750°, and the steam pressure was 54 pounds, one of the flues collapsed.

An old steamboat boiler that had been tested with cold water to a pressure of 44 pounds was next experimented with. A fire was made in the furnace, the boiler having an ample supply of water; and when the pressure was 70 pounds per square inch, two of the top sheets of the boiler gave way. The pressure gradually rose to 73 or 74 pounds, when the safety valve suddenly opened and the experiment was brought to a close. A subsequent examination showed that an old crack had existed at some points of the rupture.

On the 13th of November, the water in the pipes was frozen, and the Commissioners decided to suspend operations at Sandy Hook for the season.

Preparations had been completed for the experiments at Pittsburgh, five boilers being placed in position, bomb-proofs erected, and a shop and store room fitted up. The boilers were of the ordinary two flue variety in use on western rivers, two of them being of steel and three of iron.

Experiments were commenced on November 20, with one of the iron boilers. The fire was not sufficient to produce a greater pressure than 195 pounds per square inch. The experiment was repeated, and a pressure of 202 pounds per square inch was attained. On the 21st of November another boiler was tested, and the fire gave out when the pressure had reached 342 pounds per square inch, with no other effect than producing some slight leaks. The first boiler was also tried again, its furnace having been enlarged, but the highest pressure attained was 220 pounds per square inch. On the 22d of November, the same boiler was tried once more, and this time a pressure of 275 pounds per square inch was reached before the fire gave out. Steam was then raised on the second boiler, and both flues were collapsed from end to end. An instant before the collapse, two men entered the bomb-proof, which contained three recording gages. According to their statements the three gages showed, at this time, 400, 450, and 500 pounds per square inch, respectively; but the record given by the gages, when examined after the collapse, was 350 pounds. The Commissioners remark thatone of the results of the experiments has been to develope the fact that the instruments employed were quite unreliable, under the extraordinary pressures and temperatures to which they were subjected. No other results or conclusions are given, it being remarked that they can be more effectively embodied in a final report. The Commissioners report that about $50,000 (half of the appropriation) has already been expended.

The above is a careful synopsis of the report, given nearly in the words of the Commissioners. But we feel that we ought not to let the matter drop without some few comments. Our own position on the subject of boiler explosions has been often clearly defined, and our readers well know that we look for nothing more mysterious than too much steam, or too weak a boiler. At the same time, we are willing to concede that great good may result from experiments of this kind, properly carried out. One part of the work of the Commission we have looked forward to with the greatest interest. We refer to the test of safety valves. The only extended trial of the kind of which we have knowledge was made by the Life Saving Commission, a few years ago; and the result of that trial showed clearly that many of the safety valves in common use were wrongly named. It is difficult for us to see why the Commission, organizing early in the summer, delayed their experiments until the approach of cold weather. It is still more difficult to understand why the cessation of operations has been so complete. Surely, if every change of pressure and temperature of the steam affects the accuracy of a gage (which is quite a novel proposition to engineers) the Commissioners could continue their experiments, and reach a definite conclusion upon this point. The tests of safety valves, also, might well be continued through the winter. There is a strong suspicion in the minds of many that this Commission is not working purely in the interest of Science. It seems somewhat remarkable that the Supervising Inspector-General, who has so many other important duties, should be the chief man in the Commission. The fact that such an enormous sum of money has been expended, with such slight results, is calculated to awaken inquiry; and the refusal of Mr. Stevens, who inaugurated this style of experiments, to serve on the Commission, is a most significant fact. The public are vitally interested in all work of this character, and we but do our duty in calling attention to the shortcomings of the Commissioners, as evidenced by their own report.

Dangers of Gasoline.

At Bennington, Vt., last month, the knitting factory of H. E. Bradford was destroyed by fire. A leakage of the gasoline pipes permitted the flow of this heavy hydrocarbon gas along the floor until it reached the fire at the boiler, when a terrible explosion took place, demolishing the building. Nine women were instantly killed and several others were badly hurt. One of the especial dangers attending the use of gasoline is that, in case of leakage, it moves along the cellar bottom or lower floors of the building, and there is no means of detecting its presence until it reaches fire, when the entire mass explodes with terrible violence. Many sad accidents have occurred from its use. The ordinary street gas is less dangerous, because, being lighter, it commingles with the air, and its presence is soon detected by the olfactories.

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