lution is proceeded with as before. The soap compound made as above described may be used as soap or for any purpose for which it is suitable, or the compound may be treated with carbonic acid gas and the fatty acid thus recovered from it, while the alkali is converted into a carbonate.

FAVE'S DIVING TIDE GAUGE.

It is now nearly three years ago that Mr. L. Fave presented to the French Society of Physics the interesting apparatus that we propose to describe. The reason that we have not done so sooner is that we have been waiting for it to assume its definite form as a result of the experiments to which it was to be submitted. It has, in fact, been sensibly improved during these three years without the principles upon which it is based having been in any way modified.

The tide gauges permanently installed in a few ports of the coast are excellent, faithful and easily maneuvered instruments, but it is not always possible to make use of them. They necessitate, in fact, a structure whose lower part shall be always washed by the rising tide, and do not lend themselves to a flying installation, upon a strand or even upon a cliff. The scales that may be fixed at a short distance from the coast are liable to be destroyed by the tide or by the malevolence of the shore dwellers, even in civilized countries, and, moreover, as an observation of them, at sufficiently approximate intervals, is very necessary, it would be advantageous to substitute the use of a registering apparatus therefor. Finally, none of the tide gauges known permits of observations in an offing, at somewhat considerable depths. It will be seen, nevertheless, how great an interest attaches to such observations, since the coast has a very great influence upon the tide.

It is to such multiple *desiderata* that the Fave instrument responds. This gauge does not, like those with which we are familiar, register the height of the water, but the pressure, or, more exactly, the variations of such pressure, through a very ingenious mechanism. Upon the whole, it is nothing more than a sea bottom barometer.

In its definitive form (Fig. 1) it consists of two Bourdon tubes fastened to blocks that remain fixed in a same series of observations, but the position of which can be regulated through adjusting screws. Their movable extremity carries flexible strips, L, each of which is provided with a very fine point, P. The pressure of the water, transmitted by the tube, C, opens the spirals and separates the points, one of which moves forward and the other backward. It is the motion of these points that is inscribed. To this effect, a disk of glass, V, covered with varnish and carried by the pillar plate of a clockwork movement, revolves in front of the points, which leave a trace of their passage in the varnish. The very judicious use of two points instead of one point renders the result independent of the centering of the glass disk. It suffices, in fact, to measure the variations in the spacing of the two lines in order to completely eliminate the imperfections of the apparatus. The two small handles that carry the letters, A and B, serve to press the points against the disk or to separate them from it by means of inclined planes that engage under the flexible pieces, L and L'. The entire apparatus is contained in a box, and the interior of the spirals alone communicates with the surrounding medium.

The inscription of the very feeble motion of the points presents peculiar difficulties. The lines must be extremely fine if it is desired to obtain precise results. Silvering gives satisfaction in this respect, but it rapidly blackens, and the lines soon become invisible. Lamp black is difficult to use, and aniline colors are too hygroscopic and the least condensation of moisture clogs up the lines. Nitrate of rosaniline, which is but slightly soluble in water, lends itself to tracings whose fineness is limited only by that of the points. A saturated solution of it is made in concentrated alcohol, say 2.2 grammes to 100 cubic centimeters of alcohol and 10 cubic centimeters of ether. The solution is spread over the disk, which is turned up vertically and exposed to heat until dry. The thickness is varied by modifying the proportion of ether.

Such as we have just described it, the apparatus is perfectly adapted for depths of about ten meters, but when it is desired to submerge it to greater depths, there are two difficulties encountered; if the spirals are thin, they become too much distorted, and if they are too thick, they no longer visibly inscribe the variations of level.

Mr. Fave has solved the problem by causing only a

part to act, and, so to speak, only the variations of the pressure of the apparatus. The artifice employed by him is identical in principle with that upon which is based the Richard statoscope, but it is applied in a very different manner.

For experiments at depths of more than ten meters, the apparatus is inclosed in a metallic cylinder (Fig. 2, No. 2) provided with an aperture at the lower part. It is put in communication with a rubber bag filled with air, through the intermedium of a tube passing under a clamp external to the apparatus (Fig. 2, No. 1). This clamp consists simply of a piece of wood pressed upward by a spring. A coun erpoise keeps the clamp open. The water, gradually entering the cylinder, compresses the bag, and the air, forced into the box of the apparatus, balances the pressure in the interior of the spirals at every instant. When the vicinity of the bottom is reached, the weight deposits, the rubber is compressed and the pressure in the box remains con-

FIG. 2.—FAVE'S TIDE GAUGE.

1, clamp; 2, apparatus inclosed in a metallic cylinder.

stant from this moment. The apparatus is thus in the same conditions of sensitiveness as if it had above it a column of water equal to the length of the chain that supports the weight. The rubber forms at the point where the clamp is situated a curve into which a drop of glycerine is introduced in order to assure a perfect closing.

The apparatus is so constructed that the disk makes one revolution in two days, but it is useful to inscribe the tides at the same place for eight days at least. The use of the two movable points permits of superposing several periods upon the same disk, for the lines always intersect each other at an angle sufficiently wide to allow them to be followed without ambiguity.

We shall finish by citing one of the results obtained with the apparatus. At 200 kilometers, at least, from Brest, and at a depth of 150 meters, it was found that the tide had an amplitude about half less than at the coast. The result cannot be given in precise figures, since, in this experiment, the operation of the apparatus was perturbed by a leakage. But the measurements are going to be taken again, and will doubtless enlighten us as to the delicate question of the action of the coasts upon the tide.—*La Nature*.

CONTAGION AND PHTHISIS PULMONALIS.

By Sir BENJAMIN WARD RICHARDSON, M.D., F.R.S.

As if to discount good influences of a true hygienic character in reducing the death rate from phthisis pulmonalis, some enthusiasts, in writing on the contagion speculation, have endeavored to show that the lessened mortality from the disease, observable during the past few years, is due to the influence of the doctrine of contagion. Dr. Lawrence Flick, of Phila-

delphia, has written in this strain in the *Medical News* for May 14, 1892. He calculates that the actual decrease of deaths from phthisis in Philadelphia, from 1881 to 1891, is 0.734 per thousand of living people, which, with the present population, shows a saving of 784 lives a year. The result he assumes to be due to the circumstance that members of the medical profession in Philadelphia, having become converted to the hypothesis that phthisis pulmonalis is spread by contagion from particles of dried sputum derived from patients affected with the disease, have taken care that such contagious matter shall be at once destroyed, and so have prevented the spread of the disease by communication. It is astounding how speculations of the kind named are supported on coincidences. There is a physician living in this country who, with equal argument, believes that the reduced mortality from the disease in question is due to a medicinal cure for the disease which he himself has introduced. To prove the beliefs of such advocates, it would have to be demonstrated how many practitioners have prescribed the means of prevention or cure specified, and how many times their suggestions have been practically acted upon. Impossible demonstrations! As to the first—the conversion of the profession to the hypothesis of contagion and the influence of that conversion in modifying the death rate—it is the simplest truth to say that if the profession in Philadelphia is at all like the profession here, there is not a shadow of evidence in support of the proposition. There are a few probably who, accepting the hypothesis out and out, are more particular respecting cleanliness and removal of expectoration than they were; while the many, looking at the hypothesis doubtfully, are as little influenced by it now as they ever were. Why should this not be so? The experience gathered in Naples on this subject, when the most rigid rules for destruction of all morbid material failed to render service, is not likely to be forgotten; while the majority of observers have never been able to see the evidence that would establish the hypothesis. Let me take in illustration my own experience. In my student life I was under an able teacher of the practice of medicine, the late Dr. Andrew Anderson, of Glasgow. In his lectures on phthisis, he entered carefully into the contagion theory, at that date—1846—rather popular. He held a wise reserve on his own part; but he told us the question was a vexed one, and he urged us all to watch most carefully for ourselves in our future lives. I was one who determined to watch intently, without bias—how could I have any occasion for bias?—and persistently. I have sustained my watchfulness in the same spirit from then until this day, *i. e.*, over a period of nearly fifty years. Every opportunity has been afforded me for watching. I entered early in life into the experience of a large general practice; I left that for physician's practice in London. I became physician to an infirmary for diseases of the chest, situated in a densely populated part of London. I held my post for about fourteen years, rising to the senior physicianship. During a part of this same period I acted as one of the physicians to the Metropolitan Dispensary, the Blenheim Street Dispensary, and the Margaret Street Dispensary, another institution for the treatment of consumption. This was a wide field for practice and for observation. Moreover, at these institutions, where I gave systematic instruction, I was fortunate in having able assistants, who kept careful notes of the cases under treatment. In the course of the period named, no fewer than three thousand patients suffering from phthisis came before me; and during all the time the question of evidence on the point of contagion was never for a moment lost sight of. It is the fact that in all this field of practice not a single particle of evidence ever came before me that sustained the contagion theory, or gave, on reliable data, the least color to it. No professional brother, no assistant, no nurse, no person connected in any way with the phthisical sick, ever afforded indication of having received the disease by communication; and it is worthy of remark that no physician of my acquaintance, in the days I speak of, ever expressed a suspicion of communication by contagion. In the fifties, we had in London a society for the special study of chest diseases. In it were men of acute observation, and of skill in diagnosis. We were all engaged in hospital practice, and we all worked earnestly. The late Drs. Baly, Snow, Leared, Sibson, Francis Webb, Risdon Bennett, Edward Smith, were members of this society, as were Sir Richard Quain, Dr. Hawksley, Dr. Cockle, and one or two more whose names escape me. We met in turn at each other's homes; we drafted patients to our meetings for careful study; we discussed every point with the utmost freedom of discussion; but I feel sure at that time not one of us entertained seriously the hypothesis of contagion. Was it that we were all blind to evidence which must have been passing every day before our eyes, evidence which should have occurred to us, apart from any hypothesis, if it existed? Was this revived hypothesis wanted to open our eyes? Or was it that when the hypothesis was once restarted those who accepted it did not really see what they had failed to see previously, but were led to adapt their discernment to the requirement and to receive what was necessary to be received in order that the hypothesis might stand on a logical foundation?

I resigned my work as physician to the special institution for diseases of the chest toward the latter part of 1867, but I have remained up to the present time profiting from another course of experience in a practice of twenty-five years. In the latter part of that time much controversy has been revived on this subject of the communicability of phthisis by contagion, and although I have taken little public part in the controversy, I have been as active in observation as ever I was, endeavoring to learn, without the least prejudice, the exact facts. For many years I have sat at a board of insurance directors, feeling all the responsibility that attaches to that official duty. In every case of phthisis pulmonalis that has come under my notice in practice, and in every case of the same disease of which I have had cognizance otherwise, I have been vigilant to arrive at the circumstances under which the first symptoms of the disease were manifested. Not once has there been evidence of origin of phthisis from contagion on which I could rely. It is true I may more than once have heard the con-

FIG. 1.—FAVE'S TIDE GAUGE.

1 and 2, details; 3, the apparatus as a whole.

elusion that some contagious influence was at work, but on further inquiry the evidence has broken down. There were living near to me two sufferers from the malady who occupied the same house. They were brother and sister, the only children of a professional friend; they had the hereditary history painfully characteristic of phthisis, and in the course of the affection in them from the beginning to the fatal end their symptoms ran so closely as to be like twin phenomena. Could there have been contagion in these cases? Many might have said so who had not taken pains to investigate the facts. To me the facts were familiar. The brother was often assisting me in my work, so that I knew his history; and his attack dated from a severe cold contracted by exposure to an intense chill at an evening party, to which he went lightly clad, and in which he added to the risk from changing portions of his dress for some much lighter ones in order to take part in a charade or play. He sent for me at once when he became ill. It was the old story of pneumonia followed by rapid tubercular phthisis. The sister at this time was many miles in the country, where she had been for several months, during which time the two had not once met each other. She, like her brother, took a severe cold; and when she came home, and passed under my care, she was in the same state of phthisis as he. In neither case was there the remotest evidence of contagion, yet because of their affinity I afterward heard it adduced that contagion was at work. Not infrequently on being called upon to prescribe for phthisical patients the statement has in late times been made to me that the disease had been contracted by contagion, but in not one instance has any proof been supplied that would in a court of law of the first instance bear investigation.

I ask naturally in conclusion, Can I be blind to open facts, or do I see correctly, and has the hypothesis of contagion blinded those who see differently from myself? Willingly I will admit myself blind, and will submit myself to any correction that will give me sight; but the correction must be supplied, and in a form that makes all so clear that he who runs may read. I must be shown one case of phthisis in which the evidence is absolute that the disease was contracted from contagion, and that it could not possibly have been produced by any other determining cause.

Returning to Dr. Flick and his argument—that the mortality from consumption in Philadelphia, or any other city for the matter of that, is reduced by the precautions taken in regard to the disposal of the sputum of afflicted persons—the point is clear enough that what he traces as cause and effect is due to nothing more than coincidence.

No sanitarian for the past forty years, to my knowledge, has doubted the fact that phthisis pulmonalis will disappear just in proportion as broad sanitary views are established and brought into practice. We have all believed, and, as we think, proved, that under new conditions, in which there is provision for a better heredity, in which cleanliness becomes the watchword of sanitation, in which the youth of both sexes can enjoy free exercise and recreation in the open air, and in which the labor of life in confined places is being reduced to a proper standard; we have all believed that with these changes the disease would die out altogether. In my Essay on the Hygienic Treatment of Pulmonary Consumption, published in 1856, these principles were so strenuously enforced that I was looked upon in enforcing them as a visionary, and as wanting in that practical acumen which was necessary for successful effort in the treatment and prevention of disease. I was not alone in that argument, but I was one of a very few. In time our principles prevailed, and the reduction of mortality from consumption in military barracks, owing to reforms which were insisted upon in our teaching, afforded direct evidence, long before this contagious idea was advanced, of the truth and correctness of our observation.

The process of purification has since then gone on by leaps and bounds, and we are beginning to see the good results; a new hypothesis springs up coincidentally which may or may not be right to a certain extent within its own narrow range, but which has no more to do with the great results at this time boasted of than the reduction of typhus in its worst forms has to do with carrying of camphor in the pocket, or placing rue in the wardrobe or on the desk before the judge of assize. It may be quite practical, it is quite practical, to take care that the expectorated secretion of consumptive patients is destroyed. The late Dr. William Farr put his finger on this fact, and every sanitarian felt the importance of his advice. But this is merely a matter of cleanliness, merely a part of the whole system of perfect hygiene; and if we let this one thing stand as if it were the whole thing, we shall merely lead to carelessness in regard to everything else, and shall make matters worse instead of better. Dr. Flick is so good an observer and so earnest a man, there can be no hesitation in being critical on this particular part of his instructions, and it is only because it is painful to see him narrowing himself to one issue that this criticism is ventured upon. I hope he will take a wider view as time rolls on, and accepting the old message of the Mishna, that "Outward cleanliness is inward godliness," or, as John Wesley transformed it, "Cleanliness is next to godliness," will become a preacher of true catholic doctrine in all that relates to sanitation and the physical welfare of the world.—*The Asclepiad*.

AUGUST HIRSCH.

AUGUST HIRSCH, the founder of historical-geographical pathology, died in Berlin January 28, after a long illness. He was born October 4, 1817, in Danzig, and studied in Berlin and Leipzig. His career was influenced particularly by Hecker, the historian of medicine, and Schonlein, the clinic professor. He graduated in Berlin in 1843, his thesis being on the subject of croup, and after having taken the state examination, he began practice. This did not prove entirely satisfactory to him, and he thought of entering the Dutch East Indian Colonial service; he was, in fact, in the Hague, ready to enlist, but was prevented from taking the final step by the advice of a German physician who had had disagreeable experiences in this service. This plan, however, affected his whole future, for he had begun the study of medical geography

in order to be prepared for medical service in the Indian Colonies. He first interested himself in malaria, the most common of tropical diseases, and then in the relation of malaria to tuberculosis which was then supposed to exist. Later he occupied himself with diseases of the typhoid type.

In further pursuance of these studies he entered upon a new undertaking, the study of diseases from a geographical and historical point of view. To be sure there had been some investigations of this nature which would serve as stepping stones to a historical pathology, but Hirsch's work was of a different nature; he showed the organic connection of geographical medicine with researches in the history of medicine. His "Handbuch der Historisch-Geographischen Pathologie" (Handbook of Historico-Geographical Pathology) appeared first in 1860-1863, and again, fully revised,

AUGUST HIRSCH.

in 1880-1884, and caused a great sensation, but the author would have continued to occupy the modest position of a practicing physician in Danzig, if he had not been called to the Berlin medical faculty. Here he found his proper field for work. The faculty immediately began to study hygienics, and Hirsch, whose researches in regard to epidemics were well known, was the adviser of the administration, which sent him to West Prussia to study cerebro-spinal meningitis, to West Prussia and Posen to study cholera and to Astrakhan to study the plague. He also took a prominent part in the cholera convention. It is true that later, after the dawn of the bacteriological era, Hirsch no longer stood in the foreground as adviser. But the change in the state of affairs did not prevent his recognizing the importance of the study of bacteriology in connection with hygiene, although he earnestly warned the profession against the error of attributing epidemics exclusively to this cause. Much of his time was occupied in editing the "Jahresberichte über die Fortschritte der Medizin" (Annual Report of the Progress of Medicine), the "Biographical Lexicon," and the "Allgemeine Deutschen Biographie" (Universal German Biography). He also found time to write "Geschichte der Medicinischen Wissenschaften" (The History of Medical Science), which, with the "Historisch-Geographischen Pathologie," will secure Hirsch a lasting fame. For the portrait of August Hirsch we are indebted to our contemporary the *Illustrirte Zeitung*.

LOUIS KOSSUTH.

THE career of Louis Kossuth closed as it began, amid storm and patriotic excitement. While Kossuth

LOUIS KOSSUTH.
Born 1802. Died March 20, 1894.

lay on his deathbed, the national party in Hungary were using his name as a firebrand against the government, and his death produced a popular demonstration which has kept Hungary in a fever ever since. The national mourning has been genuine and sincere, for his long exile of forty-five years had not dulled the nation's gratitude. Nearly half Kossuth's life had been spent away from his native country, for he was almost ninety-two years old at his death. He was of Slavonian descent, and was born in 1802 at Monok, in the county of Zemplin, a rich agricultural district, where his father practiced as a lawyer. Young Kossuth was brought up to his father's profession. He first came into notice through his excellent parliamentary reports when sitting in the Presburg National Diet as representative of the absentee landlord Count Hunyady. These reports were published as a *Parliamentary Gazette*, and soon got Kossuth into trouble with the Austrian government when the young writer extended his reports to the Pesth Assembly. He was arrested, tried for high treason, and condemned to four years' imprisonment. Public feeling, however, was too much for the government, and Kossuth was released. Through the medium of his newspaper, the *Pesth Journal*, the first political organ in Hungary, Kossuth steadily worked for national independence with such success that when the revolutionary breeze of 1848 blew throughout Europe, the time was ripe for action. Kossuth, then leader of the Pesth Lower House, proposed the address to the Emperor Ferdinand, demanding Hungarian independence, and when the insurrection in Vienna had cleared the way he entered the capital in triumph to present the demand in person, and meet with a wildly enthusiastic reception. The Emperor granted a separate government for Hungary, and from that time Kossuth devoted his whole energies to developing the civil and military institutions of his beloved country. His work was hardly complete before the Croats, led by Jellachich, revolted against Hungarian supremacy. By then Kossuth was disgusted with the weak Austrian sovereign, and threw off the last vestige of allegiance when he called upon the Hungarians to rise in defense of their land. Then followed the struggle in which the Hungarian successes were rendered fruitless by the intervention of Russia. Thoroughly disheartened, Kossuth resigned his powers to Gorgey, and when the latter surrendered to Russia, Kossuth fled to Turkey, only to be imprisoned and proscribed. Austria demanded his life, but, through English and American influence, the patriot was at last set free to find asylum in that general refuge of the destitute—England. Here he received ample hospitality and for several years remained in London stirring up his compatriots to maintain their lost cause. He was never to return to his country, for when the long desired constitution was given to Hungary and Emperor Francis Joseph was crowned king, Kossuth refused to share in the general amnesty and to be reconciled to the Hapsburg dynasty. For the last thirty years Kossuth lived in Turin, where he passed away on March 20, after a long illness. His death was the signal for a fresh outburst of political animosity. As the Hungarian government refused to accord state honors to Kossuth's remains, although willing to allow him to be buried at Pesth with all due ceremony, violent rioting broke out. The students led the way in disorder, preventing the theatrical performances and hoisting black flags, while the mob took the opportunity to loot and riot wholesale. Eventually the military quelled the disturbance, and Kossuth's sons telegraphed that they would not permit their father to be buried in Hungary if order were not restored. Then the students enrolled themselves as a "Guard of Order," and every preparation was made to receive Kossuth's body with great honors. The patriot's remains left Turin amid every sign of respect, and were taken to Pesth to rest by the side of his wife and daughter.—*The Graphic*.

DUCK FATTENING IN ENGLAND.

AMONG the smaller rural industries of England that of rearing and fattening ducks is one of much interest, and also one that might be made of greater importance. In the country around Leighton Buzzard, in the counties of Beds and Bucks, this business has been pursued for a number of years, those engaged in it being chiefly cottagers. The accommodation required is not extensive, nor can the initial capital be large, at least so far as money is concerned, though the capital represented by time, trouble and careful attention must be far from insignificant.

The Aylesbury duck is the breed employed, and, as a rule, the ducks which are reared and fattened are not bred on the establishment. The custom is for the fatterer to buy his eggs from people in the neighborhood and hatch them under hens. Some trouble is taken to secure an adequate contingent of sitting hens at the time the eggs are ready, and the people engaged in the business are fully alive to the advantage of getting the fat ducklings early to market. The game season ends in February, and from then to the reopening of the shooting season in August is the time when fat ducklings make their market. Prices are highest in February and March, being then from 14s. to 16s. a couple, and gradually decreasing to 5s. or 6s. in July, on the London market, the average dead weight of the birds being about 6 lb.; dressing to 4½ lb. After hatching out, the young birds remain for a week with the hen, and are well fed on boiled egg and other dainty morsels. With other birds of the same age they are then drafted into a "flock" of 100 to 125 birds. At this age they are exceedingly pretty objects—little animated balls of yellow down, with heliotrope colored bills and a plaintive chirp. As they get older the feathers become white and the bills grow paler. On a single duck "farm" numbers of flocks may be seen of one, two, three or more weeks old, and this classification is the only one adopted, as all the birds are destined to be killed at eight or nine weeks old.

Good rice, either Rangoon or stained Japan, at about 16s. a cwt., is the chief food of the younger birds and is boiled for use. They are also very fond of toast soaked in water. Later on barley meal and toppings, or sharps, are introduced; and when the birds are five weeks old they also get greaves or tallow scrap, which is boiled up in the food, into which stinging nettles