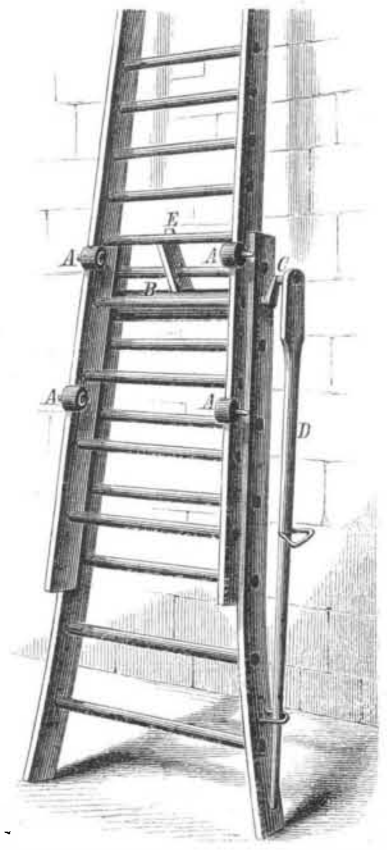
MILLENER'S EXTENSION LADDER.

The object sought in the construction of this improvement is to provide a ladder that can be used by firemen instead of two ladders hooked together. It is, so to speak, one ladder, although made in two sections, and is, it is claimed, fully as strong as an ordinary ladder of equal length. As it is capable of being made both light and strong, it will, it is claimed, be equally adapted to farmers' and mechanics' use.



In the engraving, A represents friction rollers attached to the stationary part by bolt irons. B is a roller placed between the two pairs of friction rollers, the movable part of the ladder working up or down between them, and the sides of this part being in contact with all the rollers and sliding between the side pieces of the stationary part.

The roller, B, is turned by the crank, C, either directly or by means of a rod or pitman, D, by the use of which the roller may be turned and the ladder extended by a person standing upon the ground, so that a person standing on the movable part may be raised or lowered, to the hight required, by those below. A self-acting brace, E, holds the parts extended in any required position.

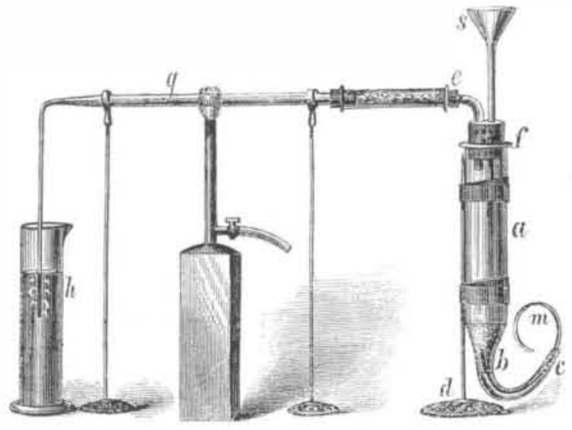
The friction roller bolts are fastened through the sides of the stationary part by screw nuts, so that there may be more or less friction on the movable part.

The invention was patented, through the Scientific American Patent Agency, Feb. 20, 1872, by Mr. Louis N. Millener, of Adams Basin, N. Y.

MAGNESIUM IN MARSH'S TEST FOR ARSENIC.

BY JOHN C. DRAPER, PROFESSOR OF CHEMISTRY, UNIVERSITY MEDICAL COLLEGE, NEW YORK.

The difficulty experienced in obtaining zinc free from arsenic, for Marsh's test, has led to the suggestion of the use of magnesium for this purpose. The latter metal is rarely to be found in any other form than that of strips or ribbons, which expose so large a surface to the action of the acidulated water as to render the evolution of hydrogen too rapid for the proper conduction of the operation. To meet this difficulty, I have contrived an apparatus in which the evolution of the gas is completely under control, and which also shows that the strip or bandlike form of the metal is well adapted to the purposes of this test.



The instrument in question consists of a stout tube, *a*, about one inch in diameter, open at both ends and six inches long, drawn down at *b c* to a caliber which will permit the free passage of an ordinary magnesium ribbon, *m*. The tube is attached by rubber bands to a paper file, *d*, with a stout iron foot or base which serves the purpose of a support admirably. At *f*, the supply tube, *s*, for the introduction of acid and other liquids, and the escape tube, *e*, pass air tight through a cork. The evolved gas is dried in a chloride of calcium tube at *e*, whence it passes through the hard glass tube, *g*, in which it may be subjected to the action of heat and finally escape through a dilute solution of nitrate of silver at *h*

When the instrument is to be used, it is dried and a column of pure mercury poured into the bend, *b c*. The cork carrying the tubes, *e* and *s*, is put in position and the reduction tube, *g*, properly supported. Pure dilute sulphuric acid (one of acid to six of water) is then introduced through the supply tube, *s*, and a strip of magnesium, *m*, being passed through the mercury into the acid, decomposition instantly takes place and hydrogen is evolved. The rate at which this goes on is indicated by the passage of the bubbles through the solution of nitrate of silver at *h*, and is completely controlled by the rate at which the magnesium is passed through the mercury.

The apparatus having been filled with hydrogen, a Bunsen flame is applied to the hard glass tube at *g*, and a measured length of the magnesium band slowly passed into the acid. The purity of the materials is thus tested as in the case of the ordinary Marsh apparatus, with the great advantage that the length of the strip consumed is known; and the quantity used in the test for purity of materials may be proportioned to that employed in the final examination. Freedom of the materials from arsenic and antimony being thus established, by the failure to produce any metallic stain in the reduction tube *g*, the solution supposed to contain arsenic is introduced through the supply tube, *s*, and the magnesium leisurely passed into the mixture. A few moments are required to expel the pure hydrogen from the apparatus, but the newly evolved gas finally reaching the heated portion of the reduction tube, metallic arsenic is deposited in its characteristic form and manner, and any portions of the arsenide of hydrogen that are not acted on by the heat pass into the solution of nitrate of silver at *h* and produce a dark brown precipitate.

The contact of the magnesium and the mercury with the acid causes the formation of an alloy or amalgam of the two metals, which, since it does not interfere with the detection of very minute traces of arsenic, is not of any moment and may therefore be ignored.

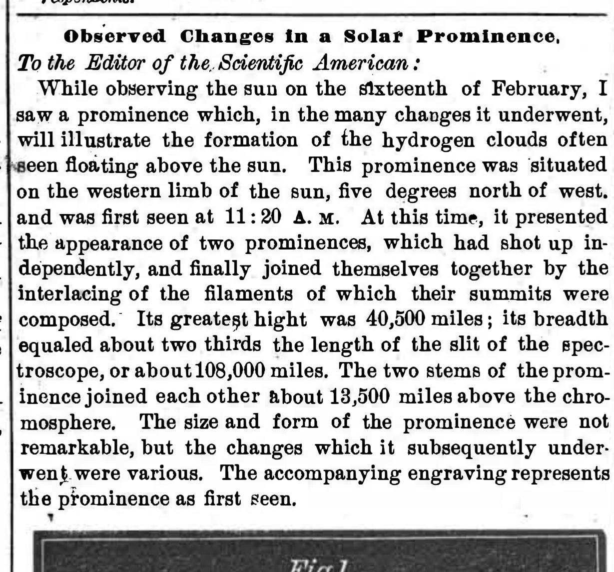
Correspondence.

The Editors are not responsible for the opinions expressed by their Correspondents.

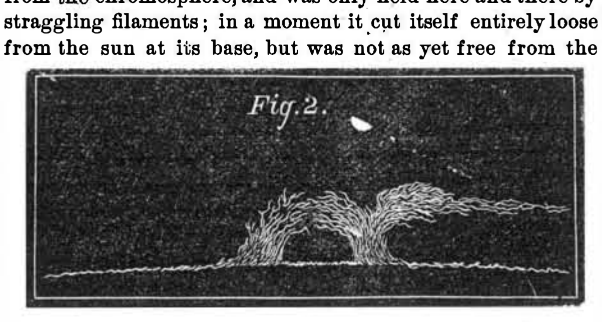
Observed Changes in a Solar Prominence.

To the Editor of the Scientific American:

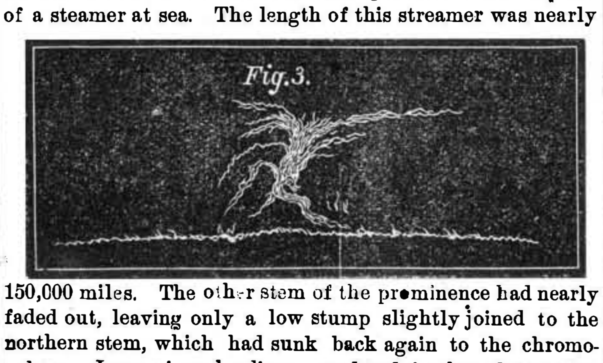
While observing the sun on the sixteenth of February, I saw a prominence which, in the many changes it underwent, will illustrate the formation of the hydrogen clouds often seen floating above the sun. This prominence was situated on the western limb of the sun, five degrees north of west, and was first seen at 11:20 A. M. At this time, it presented the appearance of two prominences, which had shot up independently, and finally joined themselves together by the interlacing of the filaments of which their summits were composed. Its greatest hight was 40,500 miles; its breadth equaled about two thirds the length of the slit of the spectroscope, or about 108,000 miles. The two stems of the prominence joined each other about 13,500 miles above the chromosphere. The size and form of the prominence were not remarkable, but the changes which it subsequently underwent were various. The accompanying engraving represents the prominence as first seen.



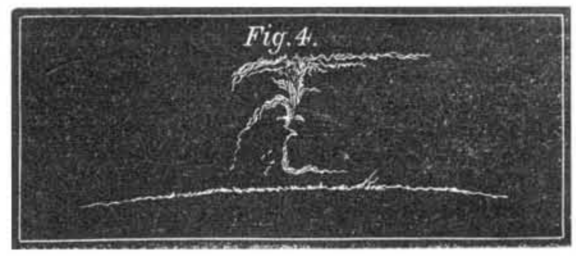
At 11:40, signs of separation began to appear where, a few moments before, all seemed a compact cloud mass; and at 11:50 the two stems were only joined by thin thread-like branches. The northern stem had begun to separate itself from the chromosphere, and was only held here and there by straggling filaments; in a moment it cut itself entirely loose from the sun at its base, but was not as yet free from the



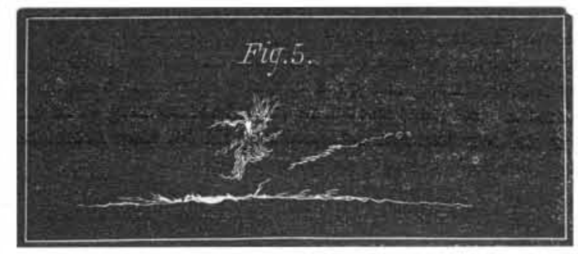
other stem. At 2:20 P. M., when again seen, great changes had taken place; the top of the northern stem had been blown towards the pole, and strikingly resembled the long streamers of smoke often seen issuing from the smokestack of a steamer at sea. The length of this streamer was nearly



150,000 miles. The other stem of the prominence had nearly faded out, leaving only a low stump slightly joined to the northern stem, which had sunk back again to the chromosphere. Increasing cloudiness rendered further observation impossible. The morning of the next day being clear, I again turned my attention to the spot, not expecting to find any traces of the prominence; but in this I was happily disappointed, for the northern stem still remained, torn, shattered, and bound to the chromosphere by one thin thread. Faint traces were left of its former attachments in the form of light thin shreds. The hight had not visibly increased; the breadth had, however, somewhat lessened. It was now 9:20 A. M., and the changes which occurred in its form were too rapid to sketch. Here and there a thread of cloud was seen to form, and as quickly disappear.



At 10:30 A. M., it announced its determination of leaving the chromosphere for good and all by a gradual twisting off of the only thread which held it captive to the sun. This it accomplished about 12 M.; I watched it for some time, until its increasing faintness rendered it a difficult object to make out.



This prominence was seen through the C hydrogen line in the telespectroscope used by Professor Young in the Dartmouth College Observatory. The prism train consists of five whole prisms and two half prisms, the light being sent twice through the train by a prism of total reflection at the end of the train, thus making the dispersive power equal to that of 12 prisms. The cloud prominences are often seen floating above the chromosphere, but generally have their origin in the chromosphere, and are the result of the ejection of matter therefrom. Father Secchi states that he has observed the formation of these clouds in the coronal atmosphere. I have many times observed these clouds, but have, without exception, been unable to discover any increase in their size; but, on the contrary, I have met with a gradual fading out and an ultimate disappearance of the cloud mass. This fact, as has already been suggested, may point to one of the sources from which the coronal atmosphere may draw its supplies of the matter whose spectrum of bright lines was first seen in 1869 and 1870, and which the observations made during the last solar eclipse so fully confirm. JOHN H. LEACH.

Dartmouth College.

The Abolition of Models.

To the Editor of the Scientific American:

Your correspondent "B" objects to the proposal of dispensing with the models on account of their supposed superiority for investigating as to the novelty of inventions. I fully acknowledge their usefulness, but contend that the drawings are much better for this purpose in most cases, especially as the models are very far from being complete, and thousands of them are so broken up that it is impossible to tell what particular patents they belong to; and in many instances it would puzzle an expert to state what class of machinery these fragmentary models are intended to represent. Of these broken and separate pieces of models, there are cartloads stowed away up in the room over the portico, which not one person in twenty frequenting the Patent Office for the purpose of examination knows anything about, to say nothing of those fragments which lie in their appropriate cases, as mentioned in my last letter.

Besides this trouble of broken models, there is another reason, that makes the drawings more reliable, which arises from the fact that the models frequently show only the bare outline of the frame or casing of the machine—the details of construction and the smaller parts, in which may consist the essence of the invention as patented, being entirely omitted and only shown in the drawing and specification. In many cases the drawing shows several modifications of the idea embodied in the model, some of them so radically different that no one would suspect that they had any relation to it, except that they belonged to the same class of machinery. I remember an instance of an excavator patent, having only a very simple model, of which the drawings show seventy-five figures, embracing twenty-six different machines for various purposes. How much could our friend "B." tell about the novelty of an invention from an inspection of that model?

That drawings are the readiest means of making an examination is shown by the practice of the examiners, who always use them in making searches and very rarely look at the models. Speaking for myself as an inventor who has had considerable experience, I know that an examination of the drawings, in nine cases out of ten, can be made in less than half the time necessary for viewing the models. That this is so is rather amusingly shown by the experience of one of our ex-commissioners, who, when in office, issued a very stringent order that no one should be allowed access to the portfolios of drawings without a special permit, which was only to be granted for infringement searches, etc. After this gentleman resigned his position, and had resumed his practice of attorney, he found "that it was a poor rule that would not work both ways," as it prevented him from examining the drawings now that he was only an outsider. This caused him so much trouble in his researches that, through his influence, an order was issued allowing ex-commissioners free access to the portfolios at all times, but retaining the rule in force against all others.

The incident brings up the question: Why should ex-commissioners have privileges denied to other people? Is there anything inherent in the office of commissioner that should make its temporary possessor a privileged character for life? If there is anything of this kind, the world should know it, and every ex-commissioner should wear a leather medal, a "feather in his hat," or some equally conspicuous insignia, for, otherwise, the common people—such as Ericsson, Morse, and other poor devils of like character—will never discover this "divinity that doth hedge" an ex-commissioner.

This question brings up another: Why should not inventors and their attorneys have the same privileges with drawings that they have with the models? I think that it is fully shown above that an examination of the latter is not sufficient to determine the question of novelty. Such a search may lead an inventor to suppose that the coast is clear, and cause him to spend hundreds or thousands of dollars in experimenting, only to find, when he applies for his patent, that his money, time and talents have been spent in vain, all of which might have been saved had he access to the portfolios of drawings.

Washington, March, 1872. INVENTOR.

Sulphite of Soda Not a Cure for Small Pox.

To the Editor of the Scientific American:

An article entitled "A Remedy for Small Pox, etc.," in your issue of February 24, was cut out and sent to me by a friend, together with your favorable notice of the same. Not long ago much agitation was excited by the vaunted cure of cancer by means of a drug called cundurango. The false hopes of cure by the many sufferers from this dreadful malady were dashed to the ground. Again, not a few persons were led, not by the advice of a physician, to an improper use of a new remedy for wakefulness by the name of chloral hydrate.

Not only is there harm created by improper use of remedies by those who know not the nature of disease, but there is also a great obstacle thrown in the way of those who are seeking to establish the science of medicine upon a sure and firm foundation. There is great source of regret that you were led to publish an article upon the subject named in the paper alluded to, trusting more to the candor of the writer than to his powers of observation of Nature; and this has induced me to write these hasty lines, for fear that another agitation of a hopeless remedy has been thrown upon the public. The value of the contribution by your correspondent depends upon the following considerations:

1st. Can an eruption upon the skin, discovered the tenth day or eleventh day after exposure to contagion, be known as small pox? Can the constitutional symptoms referred to be only explained by supposing the child to have small pox?

2d. As physicians have often mistaken, in its early stages, one kind of eruptive fever for another, is it not possible that a non-professional as well as a physician might equally be mistaken as to the character of the eruption noted in the published case?

3d. Might not the suspicions that contagion might spread from the loaned muff suggest to an anxious parent that the child had an exaggeration of symptoms which would naturally be explained by him to be caused by small pox? When the writer of this present article was studying medicine, he was taken by his instructor to see a man who had a slight rash, accompanied with pains in the back, vomiting, etc., which might possibly turn out to be small pox, to which the man had been exposed. The student was cautioned never to state that a given case was small pox, until the peculiar vesicle of that disease was developed, on about the sixth day from the appearance of the rash. After waiting a week and without any remedies except a harmless *placebo*, the rash had disappeared and the man was well.

4th. Does one swallow make a summer? Or does one apparently successful *cure* by a given drug prove that *post hoc, ergo propter hoc?*

The writer of this present article has been occupied, for the past eight years, in studying the action of drugs by experiments upon animals and by clinical observation at the bedside. His faith in the enthusiastic testimony of the success of a given remedy in the hands of well informed and educated physicians has been often shaken; and he believes that one of the greatest obstacles, to the establishing of the science of medicine upon an exact and firm foundation, is this accepting a theory and calling it a fact. If it is difficult for an honest physician of large experience to believe that his patient recovers because he gave him warm water to drink instead of cold, how much more difficult is it for a man of slight medical education and little practical experience to determine the success of a remedy in a given case?

Now observe how the enthusiastic candor of your correspondent has led him to make mistakes: He says "the United States Dispensatory gives in an ample detail the discussion of the valuable properties of this drug (*sodæ sulphice*), by the French College of Surgeons. in the thirteenth editorial article of the edition of 1871." There is no such edition of the work your correspondent refers to; and in the edition of this work for 1870, almost all the articles are "editorial" articles by Drs. Wood and Bache of Philadelphia. The name United States Dispensatory was given this book by these eminent writers; but the official organ of American physicians and apothecaries is the United States Pharmacopœia, a new edition of which is now being prepared.

On pages 826 and 827 of the thirteenth edition, to which I have referred, occur the words your correspondent has quoted in his article, but unfortunately for his candor, he has omitted sentences and words which modify the apparent statements of Dr. Wood, who would feel insulted if he were told that he had written as your correspondent would dictate.

For example, at the commencement of the third paragraph, the words "locally applied" are omitted, and in the last line the word "appear" is omitted. Allow me to furnish the correct text.

"Also, *locally applied, useful* in controlling suppurative ulcers, etc. . . . . . and in any disease in which purulent infection of the blood may be produced by the same cause, (parasitic or zymotic influences). They *appear* almost *to* act as specifics in such cases." "At *a* certain stage of cancer they operate *usefully* in the same way." There is no mention of a discussion of *sodæ sulphice* by the French College of Physicians and Surgeons, nor is there an association by this name in Paris. In 1865, there was a great excitement among physicians with regard to the use of sulphite of soda, but careful trial of it in a large variety of cases limited its action to a very small extent.

"On the whole, and not to occupy more space with a statement of claims which seem at present (April, 1868) to be excessive, we (Stillé, *Materia Medica and Therapeutics*) are disposed to adopt the conclusions of Semmola, Professor of Clinical Medicine at Naples, when he says: 'Diseases which have been attributed to morbid fermentation, such as typhoid fever, scarlatina, measles, and malarial affections' (of course including small pox) 'are in no wise influenced by the sulphites; and their grave types continue to be fatal, in twithstanding these remedies. Syphilis, malignant pustule, and purulent infection are equally unaffected by their operation.'" (*Bull. de l'Academia de Médécine*, XXIX, 1003).

It is unnecessary to quote from other authors who have tested, clinically, the use of this drug, as its failures in treatmentare many times larger than its success in curing that class of diseases to which small pox belongs.

Crude petroleum, though very useful in the treatment of certain skin diseases, has caused death by its indiscriminate local application to the whole surface of the body.

The popular use of vaunted remedies is an exceedingly harmful matter, and occasions more work for the educated physician. It has often been truly said that the quack is the strongest ally of regular physicians. Though I have trespassed largely upon your space, permit me to point out an other fallacy. The best application for the sting of an insect, or the poisonous bite of a snake, is ammonia spirits; though in snake bites it must be injected into the circulation. If your correspondent had made the local application of a strong solution of sulphite of soda, and omitted the alcohol and ammonia, he could have borne stronger testimony to the efficiency of sulphite of soda in "the sting of a male seventeen year locust."

Allow me in conclusion to state that some competent person should write a treatise upon the popular abuse of drugs, and scatter it far and wide. The effect upon the sanitary condition of our people would be marvellous.

EXPERIATUR.

Shaving with Pumice Stone.

To the Editor of the Scientific American:

I notice, in your issue of March 2, a communication on the subject of shaving, in which the writer suggests, and he probably considers it an original idea, the substitution of pumice stone for the time-honored razor and brush. Now, I do not agree with your correspondent when he claims superiority for what he suggests, as I have tested it, and the result was very unsatisfactory—a sore face.

As for the originality of the idea, I will state that, about the year 1850, an article resembling a file in shape, and which seemed to be made of a preparation of pumice stone, was patented in France, the inventor claiming that rubbing (or filing) the face with his invention was equally as good as shaving with a razor and soap. But the article did not meet with any success.

I have, on three occasions, imagined that I had become an inventor, but each time I discovered, through your valuable paper, that some one else had had the same idea before me. If your correspondent will only put his pumice stone theory in practice on his own face, he will probably be glad to resort to the usual mode of shaving. G. P.

Shaving with Pumice Stone.

To the Editor of the Scientific American:

I beg to enter my protest against the practical joker who rubs his face with pumice stone, and recommends it to those suffering with tender skin, cross grained beards, and dull razors. Now, I have experimented, from the Davenport tricks down to Solliday's paper windmill; and can truthfully assert that I have suffered nothing that can compare with this barbarous method of using pumice stone. I followed the writer's instructions to the letter, with the single exception that I rode no donkey or other hobby, for fear of aggravating the result; and what is it? The hair is off my face and the skin too, and he is laughing in his sleeve as he reads this. Yours in affliction, H. E. M.

A REVISION of the existing version of the Bible is now in in progress by companies of eminent divines, working conjointly, in this country and Europe. In about seven years the work will be completed, and it is expected that we shall then have a version embodying the best results of the most learned and accurate thinkers.

[For the Scientific American.]

THE COLOR OF THE GULF STREAM, AND ITS EXCESS OF SOLID MATTER ACCOUNTED FOR.

BY WILLIAM L. WALKER.

The Gulf stream differs in color from the surface waters through which it flows, its color being blue, while the others are green. It also holds in solution, mechanically, more solid matter than the latter. This difference in color and suspended matter is to be accounted for as follows:

"Color," as observed by Tyndall in his researches on the color of sea water generally, "resides in white light, appearing generally when any of the constituents of the white light are withdrawn. The water attacks the visual rays with different degrees of energy. The red are attacked first and extinguished; as the solar beam plunges deeper into the sea, the orange and yellow disappear; next the green, and next the blue; and if water were dark enough, and contained no suspended matter, it would work the absolute extinction of the solar beam and become as black as ink. But, in all natural water, matter is held in suspension, and a modicum of light is thrown back to the eye before the depth of absolute extinction is reached." The color, therefore, results from the relation of the solids. The stream, by reason of its greater expansion through its higher temperature, relatively contains less solids, thus cutting out the green, and leaving the blue.

It is more difficult to account for this physical difference in the stream, and why, with an excess of solid matter, it does not sink, but continues to float upon the surface.

There is a difference, generally, between the waters at the surface of the sea and those at its greatest depths, the latter being heavier, colder, and containing more suspended matter. Since all admit that an entire circulation of the oceanic waters must occur, the cold, dense waters which exist below must be hot to the surface, and *transformed* by some process into a surface current such as found in the stream. We will now attempt to point out the principle upon which this takes place.

Aside from gravity, it is heat which causes all the surface and submarine movements of the sea. The continued expansion at the equator and chilling at the poles cause the expanded particles to flow through this and similar streams on the opposite side of the globe into the polar basins. A weight added to one end of an equally balanced beam will cause that end to go down; but, at the same time, the other end must go up. In the equipoise of waters as they exist in the sea, if 1,000 cubic miles of water is carried daily through this and similar streams into the polar basins, an additional weight is thus added to the end of the beam; and, as this must go down from a loss of temperature, an equal amount must go up at the other end (the equator), through an increase in temperature. In both instances, it will be observed that the movement takes place from a change in the molecular motions in the water, a loss at the one end, and an equal gain at the other. In the polar basins, as each particle loses its temperature, it loses its buoyancy; and when it reaches its lowest temperature it attains its greatest density, and, as a matter of course, sinks to the lowest depths, where all such particles aggregate into masses and pierce their way by a slow submarine movement to the equator. These cold, dense particles, now resting upon the lowest depths of the sea, must each be expanded by heat before an upward movement can begin, and transform themselves into a current also. Two agencies operate directly in effecting this change.

First, the direct rays of the sun, as they fall upon the surface of the equatorial sea, perform the following mechanical functions: "The vast body of the thermal rays," as observed by Tyndall, "are beyond the red, being invisible. They are absorbed close to the surface of the sea. and are the great agents in evaporation." The other rays follow, and are severally extinguished; and at a very limited depth, not exceeding a few hundred feet at most, are totally absorbed. But these rays have performed their functions in expanding the particles near the surface, giving rise to a lateral movement, and in turn enabling those at the bottom to move upward; but, before any movement of this kind can begin among these particles below, they must receive some expansion by heat directly imparted from below, since the sun's rays cannot reach them. The slightest change in the specific heat, or increase in the temperature of the particles by reason of the excessive pressure and constantly diminishing weight of the column, also, by the action of the sun's rays expanding the surface, will enable them to ascend and reach the surface in a deflected motion caused by the earth's rotation, and be thrown into a compact current, such as we now find it, moving into the polar basin.

In this transformation of the dense cold water from the lowest depths of the sea into a surface current, it will be observed that no change has been wrought in its physical properties. It is the same in all respects except that it has become buoyant from expansion, and now floats upon the surface as all bodies do which are relatively lighter than the liquid in which they are immersed. Bulk for bulk, of the same temperature, the water in the stream is 15 per cent heavier, but, with its increased temperature, the stream is lighter.

It follows that no mechanical application of the sun's rays alone can produce the movement; and that the heat from the interior is related to it, is an induction from established laws.

FOR distinguishing benzole, which is made of coal tar, from benzine, which is made from petroleum, Brandberg recommends us to place a small piece of pitch in a test tube, and pour over it some of the substance to be examined. Benzole will immediately dissolve the pitch to a tarlike mass, while benzine will scarcely be colored.