them with using cheap or inferior material for the sake of gain.

If the question which has been given me means to give some method of testing before using, I confess my inability to answer. For varnish to be pronounced "durable" must be composed of the materials to make it so, and to ascertain this, chemistry must be called in to test it. Comparatively few painters understand chemistry sufficiently to analyze, and if they did, and found the material all that is necessary, the manipulation may have been defective, so as to injure its wearing qualities, and therefore I cannot suggest any way of pronouncing varnish durable before using it.

As to the common custom of hanging out boards prepared and varnished to the exposure of the sun and weather for months does not seem to me to be the correct way of testing durability. It is true we may by this mode get some idea of wearing properties, but the most thorough and correct way is to put the varnish to the same exposure, the tear and wear, that it would have in the regular service on the road on which it is to run. Cars while running are exposed to circumstances which boards on the wall are not subjected to. The cars under my charge run through two different countries and three different States, and therefore subjected to such a variety of climate and soil that the testing by stationary boards would completely fail to give the correct result. For example: I have placed two sample boards, prepared and varnished, and exposed them to all kinds of weather and to the constant and steady rays of the sun for an equal length of time, and both gave favorable results; and I have also put the same varnishes on a car and found very different results. One of the varnishes having some properties adapted to resist the friction caused by cinders, sand, and dust, and consequently not so liable to cut the surface, and therefore much more durable.

The system which I adopted long ago, and to which I still adhere (not on account of "old fogyism," but for want of better), is as follows: I have two varnishes which I want to put into competition to test their relative merits. With varnish No. 1, I do the south half of the east end of the car and the east half of the south side of the car, the north half of the west end, and also the west end of the north side; this is also done with the same varnish. On the other half of the car varnish No. 2 is put.

Thus you will see it is so placed that, should the car be turned at any time, both varnishes on each side will have the same exposure and circumstances to contend with. This I regard as the best method to test the durability of varnish. And again let me say that it would be wrong for me to argue that because the varnish which I use gives me the best results, therefore I would regard it the best for all to use. This would be wrong, inasmuch as we have a diversity of climates between Maine and California, and between the extreme northern and southern States. The varnish which has failed to give me satisfaction may be most suitable for other parts of the Union.

As to the second part of my subject, "What length of time may a car safely remain in service before being taken in for revarnishing?" this must be regulated by the nature of the run and general treatment of the car while in service. Through cars are frequently continuously on the road, and little or no opportunity can be had to attend to them while in service. Such cars should be called in earlier than those which make shorter runs, and where ample time is allowed at both ends of the journey to be kept in order. And again, cars which are run nearest the engine cannot make so large a running record as those I have exposed. Some roads, for a variety of reasons which might be given, can run cars for 14 months with less wear than others can run 12 months. So that I hold that the master painter on every road should keep a complete and correct record of his cars, and have an opportunity to examine these at intervals and report their condition, in order to have them called in before they are too far gone for revarnishing. If this system was more frequently adopted, the rolling stock of our roads would be more attractive, and the companies would be the gainers.

I cannot lay down a standard rule as to the exact time a car should remain in service before being called in for revarnishing, but I find as a general rule with the cars on the Michigan Central Railroad that they should not exceed 12 months' service, and new cars, or those painted from the foundation, should not be allowed to run over 10 months the first year. By thus allowing a shorter period the first year the car will look better and wear longer by this mode of treatment. Cars treated in this way can be kept running for six and seven years without repainting.

THE FIXATION OF MAGNETIC PHANTOMS.

WHEN we place a thin sheet of cardboard or glass upon a magnet and scatter iron filings over it, we observe the iron to take certain positions and trace certain lines which Faraday has styled lines of magnetic force, or, more simply, lines of force. The figure, as a whole, which is thus formed constitutes a magnetic phantom. The forms of the latter vary with that of the magnet, the relative positions of the magnet and plate, etc.

The whole space submitted to the influence of the magnet constitutes a *magnetic field*, which is characterized by the presence of these lines of force, and the study of which is of the most important character as regards electro-magnetic action and that of induction. In order to study these phantoms it is convenient to fix them so that they can be preserved, projected, or photographed. Fig. 1 shows how they may be fixed. To effect this, we cover the plate with a layer of mucilage of gum arabic, allow the latter to harden, and then place the plate over the magnet. Next, iron filings are scattered over the surface by means of a small sieve, and, when the curves are well developed,* the surface is moistened by the aid of an ordinary vaporizer. The layer of gum arabic thus becomes softened and holds the iron filings so that the particles cannot change position. When the gum has hardened again, the magnet is removed, and the phantom is fixed.

We thus have a tangible representation of the magnetic field produced by the magnet in the plane of the glass plate or sheet of paper. The number of these lines, or their density, is at every point proportional to

the intensity of the field, and the curves that are traced show their direction. To finish the definition of the field, it remains to determine the direction of these lines of force. Such direction is, by definition, and conventionally, that in which the north pole of a small magnetic needle, free to move in the field, would travel. It results from this definition that the lines of force issue from the north pole of a magnet and re-enter the south pole, since the north pole of a magnet repels the north pole of a needle, and *vice versa*.

These considerations relative to the direction and intensity of the magnetic field are of the highest importance for the physical theory of magneto-electric machines.

The following is another method of fixing phantoms, as employed by Prof. Baille, of the Industrial School

METHOD OF FIXING MAGNETIC PHANTOMS.

of Physics and Chemistry of the City of Paris. He begins by forming the phantom, in the usual way, upon paper prepared with ferrocyanide, and exposes it to daylight for a sufficient length of time. The filings form a screen which is so much the more perfect in proportion as it is denser, and, after fixation, there is obtained a negative phantom, that is to say, one in which the parts where the field is densest have remained white.

The same processes of fixation apply equally well to galvanic phantoms, that is to say, to the galvanic fields produced by the passage of a current in a conductor, and which consists of analogous lines of force. The processes may be employed very efficaciously and with certainty of success.—*La Nature*.

A CHIPPENDALE SIDEBOARD.

OUR illustration this week is of a unique and handsome piece of Chippendale work. The outline is elegant, and the scrollings delicate. The pedestals are peculiar in their form, the panels being carved in draperies, etc. In the frieze are two drawers, with grotesque heads forming the handles. The back is fitted with shaped glass and surmounted by an eagle. The whole forms a very characteristic piece of work of the period, having been made about 1760-1770. As our readers are aware, Thomas Chippendale published his book of designs in 1764, with the object of promoting good French design in this field of art. This piece of furniture was sold at auction lately for 85 guineas.—*Building News*.

A CHIPPENDALE SIDEBOARD.

* The curves are obtained by striking the plate lightly with a glass rod.

LIQUEFACTION OF THE ELEMENTARY GASES.

By JULES JAMIN, of the Institute of France.

THE earlier experiments of MM. Cailletet and Raoul Pictet in the liquefaction of gases, and the apparatus by means of which they performed the process, were described in the *Popular Science Monthly*, March and May, 1878. The experiments have since been continued and improved upon by MM. Cailletet and Pictet, and others, with more complete results than had been attained at the time the first reports were published, and with the elucidation of some novel properties of gases, and the disclosure of relations, previously not well understood, between the gaseous and the liquid condition. The experiments of Faraday, in the compression of gases by the combined agency of pres-

sure and extreme cold, left six gases which still refused to enter into the liquid state. They were the two elements of the atmosphere (oxygen and nitrogen), nitric oxide, marsh-gas, carbonic oxide, and hydrogen. Many new experiments were tried before the principle that governs the change from the gaseous to the liquid, or from the liquid to the gaseous, form was discovered. Aime sank manometers filled with air into the sea till the pressure upon them was equal to that of four hundred atmospheres; Berthelot, by the expansion of mercury in a thermometer tube, succeeded in exerting a pressure of seven hundred and eighty atmospheres upon oxygen. Both series of experiments were without result. M. Cailletet, having fruitlessly subjected air and hydrogen to a pressure of one thousand atmospheres, came to the conclusion that it was impossible to liquefy those gases at the ordinary temperature by pressure alone. Previously it had been thought that the obstacle to condensing gases by pressure alone lay in the difficulty of obtaining sufficient pressure, or in that of finding a vessel suitable for manipulation that would be capable of resisting it. M. Cailletet's thought led to the discovery of another fundamental property of gases.

The experiments of Despretz and Regnault had shown that the scope of Mariotte's law (that the volume of gases increases or diminishes inversely as the pressure upon them) was limited, and that its limits were different with different substances. Andrews confirmed the observations of these investigators, and extended them. Compressing carbonic acid at 13° C. (55° Fahr.), he found that the rate of diminution in volume increased

more rapidly than Mariotte's law demanded, and at a progressive rate. At fifty atmospheres the gas all at once assumed the liquid form, became very dense, and fell to the bottom of the vessel, where it remained separated from its vapor by a clearly defined surface, like that which distinguishes water in the air. Experimenting in the same way with the gas at a higher temperature (21° C. or 70° Fahr.), he found that the same result was produced, but more slowly; and it seemed to be heralded in advance by a more rapid diminution in volume previous to the beginning of the change, which continued after the process had been accomplished; as if an anticipatory preparation for the liquid state were going on previous to the completion of the change. Performing the experiment again at 32° C. (90° Fahr.), the anticipatory preparation and the after-continuation of the contraction were more marked, and, instead of a separate and distinct liquid, wavy and mobile striæ were perceived on the sides of the vessel as the only signs of a change of state which had not yet been effected. At temperatures above 32° C. (90° Fahr.), there were neither striæ nor liquefaction, but there seemed to be a suggestion of them, for, under a particular degree of pressure, the density of the gas was augmented, and its volume diminished at an increasing rate. The temperature of 32° C. (90° Fahr.) is, then, a limit, marking a division between the temperatures which permit and those which prevent liquefaction; it is the critical point, at which is defined the separation, for carbonic acid, between two very distinct states of matter. Below this point, the particular matter may assume the aspect of a liquid; above it, the gas cannot change its appearance, but enters into the opposite constitution from that of a liquid.

Generally, a liquid has considerably greater density than its vapor. But, if a vessel containing both is heated, the liquid experiences a dilatation which is gradually augmented till it equals and even exceeds that of the gas; whence, of course, an equal volume of the liquid will weigh less and less. On the other hand, a constantly larger quantity of vapor is formed, which accumulates above the liquid and becomes heavier and heavier. Now if the density of the vapor increases, and that of the liquid diminishes, they will reach a point, under a suitable temperature, when they will be the same. There will then be no reason for the liquid to sink or the vapor to rise, or for the existence of any line of separation between them, and they will be mixed and confounded. They will no longer be distinguishable by their heat of constitution. It is true that, in passing into the state of a vapor, a liquid absorbs a great deal of latent heat, but that is employed in scattering the molecules and keeping them at a distance; and there will be none of it if the distance does not increase. We are then, at this stage of our experiments, in the presence of a critical point, at which we do not know whether the matter is liquid or gaseous; for, in either condition, it has the same density, the same heat of constitution, and the same properties. It is a new state, the gaso-liquid state. An experiment of Cagniard-Latour re-enforced this explanation of the phenomena. Heating ether in closed vessels to high temperatures, he brought it to a point where the liquid could be made wholly to disappear, or to be suddenly reformed on the slightest elevation or the slightest depression of temperature accordingly as it was raised just above or cooled to just below the critical point. The discovery of these properties suggested an explanation of the failure of previous attempts to liquefy air. Air at ordinary low temperatures is in the gaso-liquid condition, and its liquefaction is not possible except when a difference exists between the density of the vapor and that of the liquid greater than it is possible to produce under any conditions that can exist then. It was necessary to reduce the temperature to below the critical point; and it was by adopting this course that MM. Cailletet and Raoul Pictet achieved their success. The rapid escape of the compressed gas itself from a condition of great condensation at an extremely low temperature was employed as the agent for producing a greater degree of cold than it had been possible before to obtain. M. Cailletet used oxygen escaping at -29° C. from a pressure of three hundred atmospheres; M. Raoul Pictet, the same gas escaping at -140° from a pressure of three hundred and twenty atmospheres; and both obtained oxygen and nitrogen, and M. Pictet hydrogen, in what they thought was a liquid, and possibly even in a solid form.

Still, it could not be asserted that hydrogen and the elements of the air had been completely liquefied. These gases had not yet been seen collected in the static condition at the bottom of a tube and separated from their vapors by the clearly defined concave surface which is called a *meniscus*. The experiments had, however, proved that liquefaction is possible at a temperature of below -120° C. (-184° Fahr.). To make the process practicable, it was only necessary to find sufficiently powerful refrigerants; and these were looked for among gases that had proved more refractory than carbonic acid and protoxide of nitrogen. M. Cailletet selected ethylene, a hydrocarbon of the same composition as illuminating gas, which, when liquefied by the aid of carbonic acid and a pressure of thirty-six atmospheres, boils at -103° C. (-153° Fahr.). M. Wroblewski, of Cracow, who had witnessed some of M. Cailletet's experiments, and obtained his apparatus, and M. Olzewski, in association with him, also experimented with ethylene, and had the pleasure of recording their first complete success early in April, 1883. Causing liquid ethylene to boil in an air-pump vacuum at -103° C., they were able to produce a temperature of -150° C. (-238° Fahr.), the lowest that had ever been observed. Oxygen, having been previously compressed in a glass tube, became a permanent liquid, with a clearly defined meniscus. It presented itself, like the other liquefied gases, under the form of a transparent and colorless substance, resembling water, but a little less dense. Its critical point was marked at -113° C. (-171° Fahr.), below which the liquid could be formed, but never above it; while it boiled rapidly at -186° C. (-303° Fahr.). A few days afterward, the Polish professors obtained the liquefaction of nitrogen, a more refractory gas, under a pressure of thirty-six atmospheres, at -146° C. (-231° Fahr.). Long, difficult, and expensive operations were required to produce this result, for the extreme degree of cold it demanded had to be produced by boiling large quantities of ethylene in a vacuum. M. Cailletet devised a cheaper process, by employing another hydrocarbon that rises from the mud of marshes, and is called *for-*

mene. It is less easily liquefied than ethylene, but for that very reason can be boiled in the air at a lower temperature, or at -160° C. (-256° Fahr.); and at this temperature nitrogen and oxygen can be liquefied in a bath of formene as readily as sulphurous acid in the common freezing mixture.

MM. Cailletet, Wroblewski, and Olzewski have continued their experiments in liquefaction, and acquired increased facility in the handling of liquid ethylene, formene, atmospheric air, oxygen, and nitrogen. M. Olzewski was able to report to the French Academy of Sciences, on the 21st of July, 1884, that by placing liquefied nitrogen in a vacuum he had succeeded in producing a temperature of -213° C. (-351° Fahr.), under which hydrogen was liquefied. Contrary to the suppositions founded on the metallic behavior of this element, that it would present the appearance of a molten metal, like mercury, the liquid had the mobile behavior and the transparency of the hydrocarbons.

EXAMINATION OF FATS.

THE methods employed up to the present in examination of fats, animal and vegetable, are mere reactions lacking general application; scattered throughout the literature, and doubtful with regard to reliability, they are of little or no value to the experimenter—an approximate quantitative examination even of a simple mixture being exceedingly difficult if not impossible, since the qualitative composition of fatty substances is the same, and the separation of the nearer components impracticable. The object of analysis consisted in estimating the accompanying impurities of fat, as, resin, albuminoids, and pigments. The nature of these substances depends on the mode of extraction and preservation of the fat, and are subject in the course of time to alteration. The only reaction based upon the chemical constitution of fat is produced by treatment of oleic or linoleic acid with nitrous acid, which therefore is of some value in the examination of drying oils. Of general application are the methods which correspond to the chemical constitution of fats, and thus determine the relative quantity of the components; advantage can then be derived from qualitative reactions, inasmuch as they further affirm the result of the quantitative test, or dispel any doubt with regard to the correctness of the result. The principal methods which comply with these demands have been carefully studied by Hueble for the purpose of discovering a process of general application; methods founded on the determination of density, freezing, and melting point were compared with those dependent on the solubility of fatty substances in glacial acetic acid or a mixture of alcohol and acetic acid; also the method of Hehner for testing of butter, the determination of glycerine and oleic acid, and at length the process of saponification. Nearly all fats contain members belonging to one of the three series of fatty acids, *e. g.*, acids of the type of acetic acid (stearic and palmitic acids); such as are derivatives of acrylic acid (oleic and erucic acids); and such as are homologues of tetrolic acid (linoleic acid). It is likely that the relative quantity of each of these acids is variable, with regard to the same fat, within definite limits, and changes with the nature of the fatty substance. The groups of fatty acids are distinguished by a characteristic deportment toward halogens; while members of the first series are indifferent to haloids, those of the second and third class combine readily, without suffering substitution, with two respectively four atoms of a haloid. In view of this behavior the first series is termed saturated, the second and third that of unsaturated acids. Addition of halogen to one of the unsaturated acids yields on subsequent examination an invariable quantity of the former, representing two or four atoms, according to one or the other of unsaturated groups; and as the molecular weights of fatty acids are unequal, the percentage quantity of halogen will be found varying with regard to members belonging to the same series. The amount of iodine absorbed by some of the fatty acids is illustrated by the following items:

Hypogallic acid, $C_{18}H_{30}O_2$	combines with 100.00 grammes iodine.
Oleic acid, $C_{18}H_{34}O_2$	" " 90.97 " "
Erucic acid, $C_{22}H_{44}O_2$	" " 75.15 " "
Ricinoleic acid, $C_{18}H_{34}O_3$	" " 85.24 " "
Linoleic acid, $C_{18}H_{32}O_2$	" " 201.59 " "

Of the halogens employed in the examination, iodine is preferable to either chlorine or bromine; it acts but slowly at ordinary, but energetically at elevated temperatures. The reagents are solution of mercury iodo-chloride prepared by dissolving of 25 grms. iodine, 500 c. c. alcohol of 95 per cent., and of 30 grms. mercury chloride in an equal measure of the same solvent; both liquids are filtered and united; a standard solution of sodium hyposulphite produced by digestion of 24 grms. of the dry salt with 1 liter water and titration with iodine solution; solution of potassium iodide of 1:10; chloroform, and finally a solution of starch. The above solution of mercury iodo-chloride acts on both free unsaturated acids and glycerides, producing addition products. For testing a sample of 0.2 to 0.4 grm. of a liquid, and from 0.8 to 1.0 grm. of a solid fat being used, which is dissolved in 10 c. c. chloroform and treated with 20 c. c. mercury iodo-chloride solution run into it from a burette, if the liquid appear opalescent a further measure of chloroform is introduced, while the amount of mercury iodo-chloride must be such as to produce a brownish coloration of the chloroform for two subsequent hours. The excess of iodine is determined, on addition of from 10 to 15 c. c. potassium iodide solution and 150 c. c. distilled water, by means of caustic soda. From a burette divided into 0.1 c. c. a solution of caustic soda is poured with continual gyration of the flask into the tinged liquid, and the percentage of combined iodine ascertained by difference; for this purpose 20 c. c. of mercury iodo-chloride are tested, on introduction of a solution of potassium iodide and starch, previously to its use as reagent. Adulteration of solid or semi-liquid fats, especially lard, butter, and tallow, with vegetable oils are readily detected by this method, since the latter yield on examination a high percentage of iodine. Animal fats, absorb comparatively less halogen than vegetable fats, and the power to combine with iodine increases with the transition from the solid to the liquid state, and attains its maximum with vegetable oils—the method being adapted to the examination of fat mixtures containing glycerides and free saturated fatty acids, provided that substances which under similar conditions combine with iodine are absent. These conditions are fulfilled

with regard to the examination of animal fats and soap. Etheral oils are also acted upon by iodine; the reaction proceeds similar to that observed in ordinary fat mixtures. Alcoholic mercury iodo-chloride can probably be used with success in synthetical chemistry, as it allows determination of the free affinities of the molecule and conversion of unsaturated compounds into saturated chlorine-iodo addition products.—*Rundschau*.

NOTES ON NITRIFICATION.*

By R. WARINGTON.

IN the following brief notes I propose to consider in the first place the present position of the theory of nitrification, and next to give a short account of the results of some recent experiments conducted in the Rothamsted Laboratory.

The Theory of Nitrification.—The production of nitrates in soils, and in waters contaminated with sewage, are facts thoroughly familiar to chemists. It is also well known that ammonia, and various nitrogenous organic matters, are the materials from which the nitric acid is produced. Till the commencement of 1877 it was generally supposed that this formation of nitrates from ammonia or nitrogenous organic matter was the result of simple oxidation by the atmosphere. In the case of soil it was imagined that the action of the atmosphere was intensified by the condensation of oxygen in the pores of the soil; in the case of waters no such assumption was possible. This theory was most unsatisfactory, as neither solutions of pure ammonia, nor of any of its salts, could be nitrified in the laboratory by simple exposure to air. The assumed condensation of oxygen in the pores of the soil also proved to be a fiction as soon as it was put by Schloesing to the test of experiment.

Early in 1877, two French chemists, Messrs. Schloesing and Müntz, published preliminary experiments showing that nitrification in sewage and in soils is the result of the action of an organized ferment, which occurs abundantly in soils and in most impure waters. This entirely new view of the process of nitrification has been amply confirmed both by the later experiments of Schloesing and Müntz, and by the investigations of other chemists, among which are those by myself conducted in the Rothamsted Laboratory.

The evidence for the ferment theory of nitrification is now very complete. Nitrification in soils and waters is found to be strictly limited to the range of temperature within which the vital activity of living ferments is confined. Thus nitrification proceeds with extreme slowness near the freezing-point, and increases in activity with a rise in temperature till 37° is reached; the action then diminishes, and ceases altogether at 55°. Nitrification is also dependent on the presence of plant-food suitable for organisms of low character. Recent experiments at Rothamsted show that in the absence of phosphates no nitrification will occur. Further proof of the ferment theory is afforded by the fact that antiseptics are fatal to nitrification. In the presence of a small quantity of chloroform, carbon bisulphide, salicylic acid, and apparently also phenol, nitrification entirely ceases. The action of heat is equally confirmatory. Raising sewage to the boiling-point entirely prevents its undergoing nitrification. The heating of soil to the same temperature effectually destroys its nitrifying power. Finally, nitrification can be started in boiled sewage, or in other sterilized liquid of suitable composition, by the addition of a few particles of fresh surface soil or a few drops of a solution which has already nitrified; though without such addition these liquids may be freely exposed to filtered air without nitrification taking place.

The nitrifying organism has been submitted as yet to but little microscopical study; it is apparently a micrococcus.

It is difficult to conceive how the evidence for the ferment theory of nitrification could be further strengthened; it is apparently complete in every part. Although, however, nearly the whole of this evidence has been before the scientific public for more than seven years, the ferment theory of nitrification can hardly be said to have obtained any general acceptance; it has not indeed been seriously controverted, but neither has it been embraced. In hardly a single manual of chemistry is the production of saltpeter attributed to the action of a living ferment existing in the soil. Still more striking is the absence of any recognition of the evidence just mentioned when we turn to the literature and to the public discussions on the subjects of sewage, the pollution of river water, and other sanitary questions. The oxidation of the nitrogenous organic matter of river water is still spoken of by some as determined by mere contact with atmospheric oxygen, and the agitation of the water with air as a certain means of effecting oxidation; while by others the oxidation of nitrogenous organic matter in a river is denied, simply because free contact with air is not alone sufficient to produce oxidation. How much light would immediately be thrown on such questions if it were recognized that the oxidation of organic matter in our rivers is determined solely by the agency of life, is strictly limited to those conditions within which life is possible, and is most active in those circumstances in which life is most vigorous. It is surely most important that scientific men should make up their minds as to the real nature of those processes of oxidation of which nitrification is an example. If the ferment theory be doubted, let further experiments be made to test it, but let chemists no longer go on ignoring the weighty evidence which has been laid before them. It is partly with the view of calling the attention of English and American chemists to the importance of a decision on this question that I have been induced to bring this subject before them on the present occasion. I need hardly add that such results as the nitrification of sewage by passing it through sand, or the nitrification of dilute solutions of blood prepared without special precaution, are no evidence whatever against the ferment theory of nitrification. If it is to be shown that nitrification will occur in the absence of any ferment, it is clear that all ferments must be rigidly excluded during the experiments; the solutions must be sterilized by heat, the apparatus purified in a similar manner, and all subsequent access of organisms carefully guarded against. It is only experiments made in

* A paper by R. Warington, read before the Chemical Section of the British Association at Montreal.