

process, the brittleness continued, and there is no doubt that it is not superficial, but penetrates to the center of the wire.

I was anxious also to find out other evidences of change produced by similar immersion, but was unable to find anything beyond the brittleness.

At the suggestion of our worthy President, I tried experiments to ascertain the tensile strength of wire after immersion in acidulated water, and I found that in this respect wire remained unchanged as compared with its natural condition. I have tried iron, steel, hard-drawn wire, and annealed wire, but in all cases the ordinary tensile strength remained unaltered.

Heating restores the flexibility of wire so made brittle; but, of course, in the case of hard-drawn wire, the hardness is destroyed also.

Mr. ANDERSON: When the President invited me to attend this meeting and hear what Professor Hughes was going to say on the occlusion of hydrogen in iron and steel wire, I at once thought that I might hear something to my advantage, because I have the honor of presiding over the Committee of Research of the Institution of Mechanical Engineers on the hardening and tempering of steel. The question at present before that committee is the explanation of the hardening and tempering of steel; and the theory, for which I believe I am responsible, but which I think also finds favor with my colleagues, is that it is due to the greater or less quantity of hydrogen contained in the steel tending either to separate the particles or to allow them to approach more closely together. The experiments Professor Hughes has described confirm this theory, and show that the excessive occlusion of gases tends to separate the particles to such an extent as to make the steel more brittle. The application of heat again expels the gases and allows the metal to return to its normal state; but if suddenly quenched, so as to prevent the gases re-entering to some extent before the steel is cool, its particles are able to approach closer together, and therefore become more compact and render the steel harder. I should like to ask Professor Hughes whether, in the brittleness he has noticed in steel, he has found its hardness increased or otherwise?

Professor HUGHES: I have not been able to verify that fact.

The PRESIDENT: Mr. Stroh will perhaps answer.

Mr. STROH: I think I can safely say that the hardness of the steel is not affected in the least degree.

Mr. ANDERSON (continuing): When a theory has been started it is important to become acquainted with everything in the way of trustworthy experiment that may throw light on the subject. Professor Hughes's experiments show that the occlusion of hydrogen gas tends to separate the particles of steel, and this separation tends to reduce their cohesive power, according to the well-known laws of attraction. It is also known that hard steel carries a greater strain per square inch than the same steel softened, simply because the particles are very close together, and their cohesive force thereby increased. The first idea of this theory was suggested to my mind by Edison's experiments in searching for a permanent substance for his electric light produced by the incandescence of wire. He found that platinum fell to pieces, and propounded the theory that this was caused by the escape of occluded gases, causing cracks in the platinum and gradually destroying it. Edison argued that if, by repeatedly heating the platinum in vacuo, he could get all the gases out, he would then obtain a permanent material. He appears to have performed the experiment, and arrived at the result that an exceedingly hard and permanent substance was capable of being produced.

Professor ABEL: I think the case alluded to by Mr. Anderson does not quite correspond with that established by Professor Hughes's experiments, which show that iron wires may be made brittle and restored any number of times; whereas Edison's experiments appear to have soon brought platinum to a state of rest as regards the alteration of its molecular structure.

Professor HUGHES: Yes.

Professor ABEL (continuing): Another important point to bear in mind is that the increase in volume of gas absorbed is only about double that originally existing in the metal. Mr. Roberts finds about ten volumes of gas existing ordinarily in malleable iron; the maximum volume of hydrogen absorbed, and by which that gas is displaced, is therefore only double that original volume. Is it possible that the absorption of this additional volume of gas is sufficient to account for the supposed separation of the molecules and great change in the physical qualities of the metal?

It appears to me that there is still something wanting to explain the great change which the metal undergoes, and the restoration of its original properties for an indefinite number of times.

Professor HUGHES: During my experiments I continually kept in mind the theory advanced by Mr. Anderson, and as yet have found no experimental fact which could support it; on the contrary, I have remarked that tempered steel occludes hydrogen with as much, if not more, facility than soft steel, and does not become soft after having done so. I have not, however, made a special study of this point, but will do so at the earliest opportunity.

The action discovered is very peculiar in its behavior. There is no doubt that steel or iron becomes excessively brittle, and breaks off like glass and appears rotten, that it can be restored by heat and rendered brittle by acid many times. But the most curious fact is that if we take a wire, when brittle, and strain it, expecting it to break to pieces, it is almost as strong as before. This surprised me, and I tested the strained wire for brittleness, but found it had disappeared, and I concluded that the mere strain had had the effect of excluding the hydrogen. The departure of the hydrogen, of course, cannot be witnessed, but in practice the strain seems to rearrange the particles, and this molecular rearrangement seems to allow the hydrogen to escape.

Mr. VON TREUENFELD: The subject of the discussion is of very great importance in telegraphy, especially with regard to the question, "Is galvanizing injurious to telegraph wire or not?" I had an occasion of learning the injurious effect of galvanizing iron wire as far back as ten years ago, when constructing telegraph lines in South America. A large quantity of iron wire was ordered from England, which was tested before being galvanized by a competent inspector, and proved satisfactory in accordance with the government specification. It was afterward galvanized and shipped; but on its arrival in South America it was again tested, and failed to reach the given standard; after an investigation it was proved that it had suffered from exposure to acidulated water during the galvanizing process.

Professor HUGHES: I have listened with very great pleasure to the discussion on my paper, but I do not feel very clear yet as to the theory on the point. There is the certain fact that the iron wire absorbs hydrogen, and that the wire

becomes brittle, but the reason why has not yet been given. But one very extraordinary fact has come out. I have always understood, and have been taught to believe by most electrical works, that iron is one of the worst negatives that could be employed. Authors record iron as polarizing rapidly, and this error has been copied and repeated in electrical works until the statement has become one of common belief. Now, if iron absorbs hydrogen, and hydrogen is the cause of polarization, one would expect to find greater polarization in iron than in any other substance; but, to my surprise, when tested (as also stated by Mr. Kempe), it was less than any known substance, and after short-circuiting an iron and zinc element for hours it rises extremely rapidly again. Its constancy is higher than any other single liquid cell. In fact its constancy is so remarkable that it can only be compared with a Daniell. The resistance of this cell is also less than any other, exposing the same amount of surface—in a similar acid. When we measure the resistance of a single liquid cell we are really not measuring the resistance of the liquid at all; for every negative we put in, all being of similar dimensions, there is a different resistance to that liquid, and it is really the resistance of the negative element and its polarization that varies. My view of the matter at present is that iron absorbs hydrogen, and that being full of hydrogen, when hydrogen comes against it in the cell, it is repelled and given off in the streaming torrents of bubbles that are so astounding. Really the iron cell is worthy of being studied and investigated—as where large and constant currents are required, as in the case of electro-metallurgy, electric light, etc., it is the most simple and economical battery that we possess.

#### HYGROMETRIC PROPERTIES OF CHARCOAL.

We have perused recently an interesting paper by a French chemist, M. Jaillard, on the power of wood charcoal to absorb moisture, the results of which may be some day turned to practical account in many ways, and are at present interesting to manufacturers of gunpowder and firework compositions.

The author found that if wood charcoal were sprinkled with water and exposed to the air for three days, it underwent the same loss of weight by exposure to a temperature of 110° C. for two hours, as another sample did which had not been wetted.

It was next found that charcoals prepared from different kinds of wood, if exposed for a sufficient length of time to the same atmosphere, lost, when heated to 110° C., for two hours, from 9 to 10 per cent. of their weight, the humidity of the atmosphere at the time being found to be 80 per cent.

The practical conclusion drawn from these experiments is that different kinds of wood charcoal have nearly identical absorption power for moisture in the atmosphere. But the quantity of moisture thus absorbed varies with the degree of humidity of the atmosphere at the time of the experiment. According to M. Jaillard, this quantity is invariably from 9 to 10 per cent., when the quantity of moisture in the air is 80 per cent. of the total amount which the air can hold in solution or suspension. We trust that these results may be confirmed by future experiments; if so, they will throw considerable light upon many phenomena with which we are at present only very imperfectly acquainted.—*Monthly Magazine*.

#### ANALYSIS OF CHIAN TURPENTINE.\*

By G. W. WIGNER, F.C.S.

CHIAN turpentine has for many years past been almost unknown in this country, and its use in pharmacy had almost entirely ceased, but special interest has been directed to it of late by the statement of the success with which Professor Clay has been using it in the treatment of cases of cancer. Great difficulty was experienced in obtaining even a small supply of the genuine article, and Professor Clay stated that more than 95 per cent. of the samples which he had seen were spurious. I have examined some samples of undoubted genuineness in order to obtain a standard for future comparison.

The description given in Flückiger and Hanbury's *Pharmacographia* is as follows: "A soft solid becoming brittle by exposure to the air; viewed in mass it appears opaque, and of a dull brown hue. If pressed, while warm, between two slips of glass, it is seen to be transparent, of a yellowish brown, and much contaminated by various impurities in a state of fine division. It has an agreeable, mild terebinthinous odor and very little taste."

As to its chemical composition, *Pharmacographia* says that it consists of resin and essential oil; the former, i.e., the resin, being probably identical with the alpha resin of mastic.

The first sample which I examined (obtained from Messrs. Allen & Hanbury's) was very probably a portion of the sample referred to in *Pharmacographia*, and would, in all probability, be 10 or 15 years old. It was of an opaque yellow brown color, rather too soft to make a good pill mass alone, very slightly sticky, and covered on the surface with a whitish powder, which appeared to consist of parts of the resin itself, acted on by the atmosphere. A small portion was melted and dropped into cold water so as to form tears, and the sp. gr. of these tears was found to be 1,050 at 60° F. If one of these fragments is gradually heated in water to the boiling point, it melts and expands rapidly, becoming lighter than the water, and floating as a film on the surface.

With the exception of a small amount of mineral impurities, consisting chiefly of sand, it dissolves readily in boiling alcohol 60 o.p., which becomes slightly milky when cold. The resin is precipitated as a white powder on dilution with water.

Absolute alcohol dissolves it readily even in the cold; so also do ether, chloroform, and bisulphide of carbon. Petroleum spirit and turpentine dissolve it readily on warming, and wood naphtha dissolves it slowly on warming, the solution becoming slightly milky on cooling.

A portion was distilled with water for the volatile oil, which was found to amount to a little over 9 per cent.

A 20 per cent. solution of the turpentine itself was examined in the polariscope, and gave a right-handed rotation of 9° 12' in a tube 200 mm. long for the sodium ray.

The essential oil from the same solution gave a rotation of 1° 54' for the sodium ray, leaving 7° 18' as the rotation due to the resinous constituents.

The sample contained two different resins, one of which saponifies readily with carbonate of soda, and the other saponifies with somewhat more difficulty, but forms a far less soluble soap. This latter is present in by far the larger

quantity, and it appears likely from its appearance and character that it corresponds pretty closely with the alpha resin of mastic.

Dividing these resins as far as possible by solubility, the sample appeared to contain as follows:

Volatile oil.....	9.2 per cent.
Alpha resin.....	79 "
Gamma resin.....	4 "
Benzoic acid.....	traces.
Impurities, chiefly sand.....	7.3 "
	99.5 "

The second sample I examined was a portion of a new supply just received in this country, but coming through almost the same channel as the first one. It was, of course, newer, and probably from that cause somewhat softer; the brittle characteristic of the original sample was, however, strongly marked, and the tears, which had been produced by letting a few drops fall into the water, were sufficiently brittle to break when allowed to fall on to the table.

A 20 per cent. solution, examined in the polariscope with a sodium flame gave a rotation of 7° 46', of which 1° 54' was due to the volatile oil, and the difference 5° 52' to the resin. Apparently, therefore, there was some slight difference in the optical rotatory power of the resin in this sample.

Saponified and treated in the same way, it gave the following results:

Volatile oil.....	9.2 per cent.
Alpha resin.....	81 "
Gamma resin.....	6 "
Benzoic acid.....	traces.
Impurities—ash.....	1.4 "
Woody fiber.....	2 "
	99.6 "

The sp. gr. of this sample was 1,052, or rather higher than the old one.

A third sample of very similar appearance was procured in London from another source. This was probably old, although, perhaps, not so old as the first sample above referred to.

The sp. gr. was 1,048. The rotation in the polariscope was practically identical with the first sample. The analysis showed, however, a larger percentage of volatile oil, viz., 12.1 per cent., and it contained a mere trace of ash and a smaller proportion of impurities. This, which was to all appearance a genuine sample, was evidently more carefully collected.

A fourth sample, also purchased from a London wholesale house, was obviously a spurious one, and I record its characteristics here simply to enable such samples to be distinguished. In color, appearance, and smell it closely resembled Canada balsam; it was softer and far more tenacious than genuine Chian turpentine, so sticky, in fact, that it was with difficulty it could be removed from the fingers. When a portion was rubbed on the hand the smell was extremely pungent and persistent. The sp. gr. was 1,000, or exactly identical with water at 60° F. In connection with this, it should be borne in mind that the sp. gr. of Canada balsam is less than water, some samples being as low as 970. Its rotatory power in the polariscope was 6° 15' for a 20 per cent. solution in a tube 200 m.m. long; that is, it was only about two-thirds of the rotation of the genuine samples. It yielded about 26 per cent. of volatile oil, or more than twice that contained in the genuine samples, and the rotation due to the volatile oil was 3° 36', leaving only 2° 39' due to the rotation of the resin, or less than half that of the genuine samples. The solubility in alcohol, ether, chloroform, petroleum spirit, naphtha, bisulphide of carbon, and turpentine showed no difference from the genuine samples which was capable of being used for discrimination. It appeared to contain about 70 per cent. of a resin which corresponded in some respects to the alpha resin found in the genuine samples, but was of a darker color, and formed a much harder and more brittle soap, while I could not detect the slightest trace of benzoic acid. My opinion is that this sample was mainly a mixture of colophony and Canada balsam.

It appears most probable that the Chian turpentine, described as Cyprian or Syrian, or Chio, in *Watts' Dictionary*, p. 920, was a sample of this kind, for the description there states, among other things, that it was viscid, and had an aromatic taste like that of mastic, both of which statements correspond exactly with this spurious sample, but are incorrect as regards the genuine samples first referred to.

#### NOTES ON CREAM OF TARTAR.\*

By ALFRED H. ALLEN.

CRUDE tartar or argol is well known to be a crystalline crust deposited during the fermentation of grape juice. It consists largely of acid potassium tartrate, but if plaster be used in the manufacture of the wine, the tartar contains a large proportion of calcium tartrate.

Cream of tartar is generally admitted to be a preparation obtained by boiling crude tartar or argol with water, filtering, and crystallizing the salt from the clear liquid. The term "cream" of tartar is derived from the fact that during the evaporation of the liquid the salt is deposited in white crystalline crusts on the surface of the solution.

Cream of tartar thus obtained consists chiefly of potassium hydrogen tartrate, KHC<sub>4</sub>H<sub>4</sub>O<sub>6</sub>. All commercial samples contain more or less tartrate of calcium, which, though nearly insoluble in pure water, dissolves with moderate facility in a hot solution of acid tartrate of potassium.

According to the *British Pharmacopœia*, cream of tartar is a synonym for the acid tartrate of potash, but the solution in hydrochloric acid is admitted to be "rendered slightly turbid by oxalic acid" after neutralization by ammonia.

According to Pereira, cream of tartar "contains from two to five per cent." of calcium tartrate. All the specimens which Thomson examined contained "rather more than five per cent." Stillé and Maisch say "the amount of tartrate of calcium contained in crude tartar varies between 5 and 15 per cent."

According to R. Warrington—a high authority on this subject—the proportion of tartaric acid existing as neutral tartrates in refined tartars varies from one and a half to seven per cent. Taking these amounts as calcium tartrate, we may say that the proportion of that salt existing in cream of tartar is, according to Warrington, from 2 to 8.8 per cent.

In my capacity of public analyst, I have recently received from inspectors 14 samples of cream of tartar which have been considered genuine. These were obtained at various

\*Read before the Society of Public Analysts, on 2d June, 1880.

\*Read before the Society of Public Analysts, on 14th April, 1880.