SCIENCE:

A WEEKLY NEWSPAPER OF ALL THE ARTS AND SCIENCES.

PUBLISHED BY

N. D. C. HODGES,

874 BROADWAY, NEW YORK.

SUBSCRIPTIONS.—United States and Canada................$3.50 a year.
Great Britain and Europe................. 4.50 a year.

Communications will be welcomed from any quarter. Abstracts of scientific papers are solicited, and one hundred copies of the issue containing such will be mailed the author on request in advance. Rejected manuscripts will be returned to the authors only when the requisite amount of postage accompanies the manuscript. Whatever is intended for insertion must be authenticated by the name and address of the writer; not necessarily for publication, but as a guaranty of good faith. We do not hold ourselves responsible for any view or opinions expressed in the communications of our correspondents.

Attention is called to the "Wants" column. All are invited to use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The Exchange" column is likewise open.

ARSENICAL POISONING FROM DOMESTIC FABRICS.

PHYSICIANS long ago associated a certain class of symptoms with the presence of arsenic in the wall papers of the rooms inhabited by their patients. Of course, so long as the question was in this condition there was abundant room for mistake, and all that had been observed might be explained by some chance coincidence. It now appears that whenever the class of symptoms referred to are well marked there is arsenic present in the urine. It further has been shown in a number of cases that when the suspected wall paper was removed the arsenic disappeared from the urine of the patient, and the symptoms disappeared as well. The number of cases is large in which these points have been made: a certain class of symptoms, arsenic in the wall paper, arsenic in the urine of patients, wall paper removed, arsenic disappears from the urine, symptoms disappear in proportion.

Of course this is not absolute proof that the arsenic came from the wall paper, but, after a large number of cases of the same sort, the evidence amounts to moral proof, and it is rare in medicine to obtain evidence that is more conclusive.

How the arsenic gets from the wall paper to the patient is another question; but, although it would be satisfactory to establish this point, the proof of the *modus operandi* is not essential so far as the legal aspects of the case are concerned. Without this last proof it is easy to throw dust in the eyes of those not versed in such inquiries, but protective legislation has been taken again and again in cases where the risk is far less than here.

"The question how the injurious effects are produced by arsenical colors in our domestic fabrics is a moot point, some thinking it arises from arsenical dust, others holding to the gaseous theory."[1]

[1] Lecture on our Domestic Poisons, by Henry Carr, London, Health Exhibition Literature of 1884, Vol. IX., p. 189.

A New York chemist testified in a hearing on the subject in Boston, "I found that a botanist named Selmi, in experimenting on mould, found it produced a little hydrogen, and he invented the suggestion that the mould on the back of wall paper might produce a little hydrogen, which might unite with the arsenic on the front of the paper, and produce arseniuretted hydrogen, which might account for the popular idea that arsenical wall paper was dangerous."

This "botanist named Selmi," who may have the advantage of a knowledge of that science also, is an Italian chemist of first-class reputation, who has been publishing his work for at least eighteen years since 1874, and has devoted himself lately more especially to physiological chemistry. He is mentioned in Henry Watts's "Dictionary of Chemistry," Third Supplement, p. 122 (1879), by this reference, "On the detection of Arsenic in Toxicological Investigations, see Selmi (Gazz. Chim. Ital., II. 544)." An interesting paper has lately been issued by the Italian Ministry of the Interior from the scientific laboratories of the Bureau of Health, under the direction of Professors A. Monari and A. Di Vestea, prepared by one of Selmi's countrymen, Dr. B. Gosio, assistant in these laboratories, the following translation of which I am sure will interest your readers and assist in the solution of this problem. GEORGE S. HALE.

Action of Microphytes on Solid Compounds of Arsenic: A Recapitulation, by Dr. B. Gosio.[1]

It is well known that, under certain conditions, poisonous products may be developed from wall papers and tapestries colored with arsenical colors (Scheele's green, Schweinfurth's green), and experience has repeatedly demonstrated the serious evils that may arise from their use.

But as to the internal mechanism by which the said coloring-matters become hurtful, many doubts remained, and on certain points perfect obscurity. The idea advanced by Selmi met with favor, viz., that poisonous gases may in such cases be produced by the vital processes of microphytes; but in view of the small range of his experiments (some of which gave results adverse to his theory although tried on a large scale) the preference is given, on the whole, to the theory of William Forster. He says that wall-hangings and tapestries containing arsenical colors are poisonous by reason of the solid particles that are mechanically set free from them and penetrate the organism when inhaled in the form of fine dust. The same conclusion was reached by Giglioli of Naples after eight months of experiment on mould-cultures in earths (both solid and broken up in water), mixed with arsenious anhydride; and he explained his ill success by saying that probably arsenic is not compatible with the life of those germs that would be capable of developing hydrogen, and, therefore, the reducing mechanism was wanting.

On the other hand, the partisans of the parasite theory, while they draw from their observations only general criteria, have not been able, thus far, to point out what micro-organisms are peculiarly suited to bring about the modifications of substance to which they refer; nor have they determined whether all the compounds of arsenic, or, if not all, which of them are most susceptible of these modifications. Thus, Bischoff relates that it was noticed that from a mixture of flour and common white arsenic (which had been used to poison a horse for purposes of revenge) a gas was developed which had the smell of garlic and the characteristics of arseniuretted hydrogen. But he neither states how it was found

[1] This study was communicated in advance to the last Congress of Hygiene, held in London, where the preparations were also exhibited.

possible to verify this phenomenon, nor could this fact serve to establish our proposition; for in his case the substance in question was arsenious acid, while the colors used in dyeing are salts of this acid, generally with a cupric base (Scheele's and Schweinfurth's greens), or sulphids of arsenic (realgar, orpiment). And it is obvious that this circumstance is not irrelevant; for arsenic or arsenious acid may be compatible with the life of certain germs while arsenite of copper may be incompatible, and, indeed, would at first sight appear to be so, if we consider the well-known antiseptic action of the salts of this metal.

Hence, in order to prove that tapestries which contain arsenical colors can become poisonous by reason of the transformation of the coloring-matter itself into volatile poisons as a result of the biological activity of the micro-organisms that vegetate in contact with it, it is necessary to prove that these micro organisms can exist with and do transform precisely those colors which are used in tapestries.

My experiments bring a contribution to this interesting question of hygiene and toxicology. The results obtained allow us to determine not merely whether from solid compounds of arsenic and from which of them (arsenious acid, arsenic acid, arsenites, arseniates) it is possible, through the action of microphytes, to develop arseniuretted hydrogen gas or volatile arseno-organic products, but also to determine what species are pre-eminently suited to produce this transformation. In the first place I prepared some potato pulps containing from 0.05 to 0.1 of arsenious anhydride to 1,000 of pulp. These, distributed in several broad Petri capsules, were kept for some days uncovered in a cellar. Soon the growth of moulds and of the common bacteria of the air was very abundant, and at the end of one week a strong smell of garlic began to be perceived, showing that gaseous arsenical emanations were taking place. The cultures were then placed in a large damp chamber, from which, by means of an automatic pump, a continuous current of air was drawn, and this was made to bubble up during about two weeks through a solution of nitrate of silver. A strong reduction of this salt, together with the formation, in Marsh's apparatus, of arsenical rings and spots obtained from the liquid after the elimination of the silver, were the indisputable proofs that the cultures had developed a reducing arsenical gas.

While this was a positive indication of great value in reaching a conclusion, other arsenical pulps in which also germs of many species had been developed gave no evidence of having undergone a similar decomposition. This disparity of results, if on the one hand it justifies the discordant conclusions reached by the various investigators, must, on the other hand, necessarily be accounted for by the generic diversity of the germs developed in the two cultures, since all the other conditions of temperature, humidity, atmosphere, nutrition, etc., had remained unchanged. And here began the work of separating the germs and the series of experiments on pure cultures, of which I will treat in detail in my larger work. Of the germs thus isolated some belong to the moulds, others to the schizomycetes; among the former I note penicillium glaucum, aspergillum glaucum, and, above all, as greatly preponderating in the mother culture, mucor mucedo. I would also have endeavored to ascertain exactly the species of other moulds and of the other saprophytes, if I had found them capable of bringing about the important transformations to which I refer, which was not the case.

Nevertheless, each of the germs obtained in pure culture and others also which are most commonly kept in the laboraties (B. radiciforme, B. prodigiosum, B. subtile, yellow sarcina, etc.) were cultivated separately in sterilized potato pulp rendered arsenical by 0.05 grams per 1,000 of arsenic acid. The cultures were kept at the temperature of the surrounding air (20°–27° C.), and in diffused light. After one month of observation I was able to ascertain that the production of arsenical gas (indicated by the characteristic garlic smell) had taken place only in the cultures of mucor mucedo and (in a far less degree) in that of aspergillum glaucum. It was not perceived in any of the other cultures.

In view of these facts, special importance attaches to mucor mucedo, a mould very widely diffused in our atmosphere and capable of reducing remarkable quantities of arsenic acid, as I have been able to make sure by strict chemical researches on the abundant cultures carried on in presence of arsenic anhydride and of alkaline arseniates.

In another series of experiments, intending to follow out the practical direction that I had adopted, I inquired whether this activity of the mucor could he extended to those preparations of arsenic which the art of dyeing utilizes in the coloring of papers and hangings in general. To this end the cultures were carried on in the presence of Scheele's green, Schweinfurth's green, realgar and orpiment.

Without here dilating on the course of each separate experiment and on the method of chemical investigation pursued (a thing which I will do in my forthcoming publication) I will sum up my matter in the following corollaries: —

1. Mucor mucedo tolerates remarkable quantities of arsenic not only without injury, but with advantage to its nutrition, for it grows more vigorously.

2. Many solid compounds of arsenic are, through the biological activity of the fungus that vegetates in contact with them, transformed into gaseous combinations, of which arseniuretted hydrogen is certainly one.

3. This transformation is brought about more or less rapidly, but is constant and lasting in the case of all the oxygen compounds of arsenic, including arsenite of copper, which is the basis of the green arsenical colors used in dyeing. It does not appear to take place in the case of the sulphids of arsenic (realgar, orpiment) although the presence of these in the cultures is not at all detrimental.

4. In given conditions of humidity, temperature and light, arsenical gases may be given off from hangings colored with Scheele's and Schweinfurth's greens, through the vegetation of the mucor (I cannot say yet whether of all the mucorini): hence the danger to those who live in such an atmosphere.

This statement of mine does not, of course, exclude the possibility that poisoning may be caused through inhaling the fine dust, as William Forster thinks. But it is evident that this could only happen as an exception, inasmuch as one essential condition of the production of the fine dust is a certain degree of dryness of the walls to which the papers adhere, whereas we have seen that the poisonous character of arsenical hangings is generally favored by a certain degree of humidity and can be suspected from a more or less intense smell of garlic in atmospheres which answer to the above-mentioned conditions.

I cannot yet say whether the product of the action of mucor mucedo on the oxygen compounds of arsenic is entirely arseniuretted hydrogen. I have reason to think that it is not. By the action of alkalies I have, in fact, constantly succeeded in setting free a volative substance smelling strongly of garlic from the silver solutions employed to oxidize the assumed AsH_3 developed by the cultures. The gas so obtained, when burned by oxide of copper, furnishes an abundance of CO_2; but it is not possible, thus far, to reach any positive conclusions on this point, nor even to exclude the suspicion that the formation of the CO_2 may depend on the admixture of some other hydrocarbon gas. This point will be made clearer by the special studies that I have undertaken together with Dr. Gorini, for which I am making use of a large culture material.

September, 1891.

A PROBLEM IN PHYSICS.

IN *Science* for Nov. 28, 1890, there was a short note on the experiment conducted by Joule, in which air compressed in one cylinder was allowed to expand into an exhausted cylinder. It was shown that the only work done by the compressed air was that of imparting a velocity to its own particles, i.e., it did not expand against a resistance, and hence the chilling produced was slight. This experiment has not received the attention it deserves, and, moreover, it seems to have been entirely misinterpreted. It has been suggested that, while at the first instant on opening communication between the two vessels, there is an expansion into a vacuum and no work done, yet at the very next instant there is air in the previously exhausted cylinder, and there is work done in compressing that. This is a serious fallacy, and lies at the bottom of the misinterpretation. It is very certain that no work against a resistance is done at any moment during the expansion This experiment is so far-reaching in its application, and is so extremely important, that I desire to discuss it a little farther, and I sincerely trust that some one in a suitably-equipped laboratory may be induced to try a few simple experiments in this line.

Tyndall has shown that mere rarefaction is not a source of cold, though this is somewhat of a popular fallacy. Let us take a cylinder with a piston fitted air-tight and moving without friction. Let us consider that there is no loss of heat from the interior nor accession from the outside. Suppose the piston is raised suddenly from bottom to top. A perfect vacuum will be formed; but, as no work has been done below the piston, there will be no cooling effect; all the work and consequent heating would be at the engine, which may communicate with the cylinder, though a hundred feet away. Now, suppose a very thin film of air .001 of an inch thick were at the bottom of the cylinder. When the vacuum was formed this thin film would impart a velocity to its particles in order that they might follow the piston, but this air certainly would not expand against a resistance, and hence the chilling would be exceedingly slight. Suppose the piston should be at a point half-way from top to bottom; when it was raised the air beneath would impart a certain velocity to its particles in following the piston, but here again there would be no expansion against a resistance, and hence the chilling would be slight.

Let us change the conditions slightly. Instead of having the air at atmospheric pressure beneath the piston, as in the last case, let it be at double that pressure. On lifting the piston as before we have taken off the pressure and the air beneath imparts a certain velocity to its particles in following the piston. At the first instant that the piston starts there may be a very slight expansion against a resistance, but that would be momentary. The bulk of the cooling would, as before, be due to the fact that a velocity is imparted to the particles beneath the piston, and, in this case, this velocity would be given to a greater number of particles than before. The cooling would be slightly greater, also, but it would not be due to the loss of heat consequent upon the work of expanding against a resistance.

In order to compute the cooling in such cases as these, a formula has been used which will be found in the *American Meteorological Journal* for November, 1890, p. 339, as follows:

$$\frac{T}{T'} = \left(\frac{p}{p'}\right)^{.291}$$

In this T and T' are the absolute temperatures corresponding to p and p'. It seems to me, however, that this formula is not applicable in this case; for it gives a greater cooling, the less the work that is done. Suppose $\frac{p}{p'} = \frac{3}{4}$, the cooling by the formula would be 38°; if $\frac{p}{p'} = \frac{1}{3}$ the cooling would be 134°; and if $\frac{p}{p'} = 0$, or the expansion was in a vacuum, the cooling would be 490°. Now, by the principles already enunciated, if the expansion took place in a vacuum there would be no expansion against a resistance, and hence there would be no work done except in imparting a certain velocity to the particles. If the formula fails in the last case, it must also fail in all the others. It seems to me that the formula is only intended to be used in cases where there is an expansion against a resistance, and not in the cases here given.

A question has come up recently which may be partly answered by this discussion. It is this: What will be the cooling due to the expansion of gas in a balloon if it should ascend very suddenly to several thousand feet above the earth? Suppose the balloon were instantly put into a perfect vacuum, and the envelope had no resistance; there would be no expansion whatever against a resistance, as we have just seen, and the only work performed would be that of imparting a certain velocity to the particles of gas. As a result the gas would be slightly chilled, but vastly less than if it had expanded against a resistance. Now, if the balloon had been suddenly placed at a point where the pressure was ten inches, or one-third that at the earth, the same principles would apply; the only work done would be in imparting a certain velocity to the particles of gas, and in consequence there would be only a slight chilling.

I should be very glad if some physicist would kindly solve the following problems.

1. Given an exhausted cylinder of certain dimensions, how much would the air be heated if allowed to enter without noise, and until the pressure was the same as that outside?

2. What would be the cooling of a perfect gas in a balloon one-third full, if the pressure on the outside were suddenly reduced from thirty inches to ten inches, the temperature of the outside air remaining constant, the envelope of the balloon being without weight and infinitely flexible?

H. A. HAZEN.

THOMAS WHITTAKER announces a volume by Frederick Saunders (of the Astor Library), entitled "The Story of the Discovery of the New World by Columbus," the same being an abridgment from the latest authorities. It will be an illustrated quarto.