III. Observations on the acetic and acetous acids

C. Darracq

To cite this article: C. Darracq (1802) III. Observations on the acetic and acetous acids , Philosophical Magazine Series 1, 13:49, 12-20, DOI: [10.1080/14786440208676082](https://doi.org/10.1080/14786440208676082)

To link to this article: <http://dx.doi.org/10.1080/14786440208676082>

Published online: 18 May 2009.

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rate the paste under a stream of water, and to prepare a gluten entirely freed from any amylaceous principle. But before this object can be carried into execution, it will be necessary that various trials should be made by artists to ascertain whether the glutinous solution has the properties requisite for painting, or at least for varnishing.

Chemists may, however, employ it in the mean time as luting. I mixed with fermented gluten diluted in alcohol a certain quantity of quicklime: ammonia and caloric were disengaged, and the mixture assumed a soft consistence. With this paste I luted several vessels, placed over them bands of linen dipped in the solution of gluten, and besprinkled them with quicklime: they soon adhered with such force that I think this luting preferable to that prepared with the whites of eggs.

It appears to me to result from these experiments:

1st, That fresh gluten is insoluble in alcohol.

2d, That it becomes soluble when it has undergone acid fermentation.

3d, That the alcoholic solution of gluten is precipitated by water.

4th, That this solution, evaporated to the consistence of syrup, furnishes a varnish which may be employed in the arts.

5th, That fermented gluten diluted in alcohol becomes an excipient of colouring matters, and makes them adhere to the smoothest bodies.

6th, That vegetable colouring substances combine with gluten better than others.

7th, That painting where gluten has been used dries very soon, has no noxious odour, and may be washed.

8th, That a very strong and tenacious luting may be made with gluten and lime.

III. *Observations on the Acetic and Acetous Acids.* By C. DARRACQ*.

THE numerous opinions of chemists on the difference between the acetic and acetous acids have induced me to make a few researches on the subject. But before I give an account of my experiments I shall mention a few of those of the modern chemists, and also the conclusions which they have drawn from them.

C. Adet read a memoir in the Institute, on the 11th Ther-

* From the *Annales de Chimie*, No. 122.

midor, year 6, on the acetic acid, published in the *Annales de Chimie* of the same year. By very interesting and well-performed experiments C. Adet endeavoured to prove that there did not exist two acids of vinegar, and he concluded from them, that the acetous acid is constantly at the highest degree of oxygenation; that it is consequently in the state of acetic acid; and, in the last place, that the difference which exists between the acetous and acetic acid seems to depend on the less quantity of water contained in the latter, and not on a greater proportion of oxygen, as before believed. I shall not describe his experiments, as they are too numerous; I shall only observe that they are very exact.

In the month of Brumaire, year 7, C. Chaptal published his observations on the same subject *. By new experiments he combats not those of C. Adet, but the consequences he has deduced from them. He however does not consider the acetic acid as more oxygenated than the acetous acid, but only as containing less carbon; he mentions several experiments which seem to support his theory, and particularly the distillation of acetite of copper. This celebrated chemist says, that when this salt is distilled the acetous acid is decarbonized; a part of this carbon combines itself with the oxygen of the oxide of the copper, and escapes in carbonic acid; while the other remains in its natural state in the retort with the metallic oxide, and while the acetous acid, thus deprived of a portion of its carbon, passes into the receiver in the state of acetic acid. A number of other experiments are quoted in support of this reasoning; but I shall not describe them as they are printed, I shall only describe those made by myself.

To ascertain whether the acetic acid was more oxygenated than the acetous acid, I repeated some of C. Adet's experiments, which I found to be perfectly correct; but it would be useless to describe them, since all those chemists acquainted with them are agreed in regard to the oxygenation of the acetic and acetous acids; but it will not be improper, perhaps, to mention some new ones which seem to support this theory.

As the two acids in question differ a great deal by their concentration, I brought them before I employed them to the same degree; and for this purpose employed the specific gravity of pure acetous acid, which was 1.007: on the other hand, I mixed with the acetic acid a sufficient quantity of distilled water until its density was equal to that of 1.007, or of acetous acid. In this state there is very little difference

* *Annales de Chimie*, vol. xxviii. p. 113.

between

between these two acids, only that the acetic acid emits a very slight empyreumatic odour.

I took 2000 parts of acetous acid, which I put into a retort with 500 parts of nitric acid. The apparatus being properly disposed for collecting the liquid and gaseous products, the mixture was carried to the state of ebullition, and continued till the 2000 parts nearly of the acetous acid employed were distilled. No gas passed during the operation, and the product, when examined, showed no mark of alteration, its specific gravity being equal to that of the acid employed, that is to say, 1.007. When combined with oxide of lead, I obtained, by proper evaporation, a crystallized salt, which was found to be common acetite of lead: it appears to be nearly certain that the acetous acid experienced no change during this operation; its odour was neither stronger nor more penetrating than that of the acetous acid employed, and its flavour was neither stronger nor more pungent.

By a second experiment I again endeavoured to oxygenate, if possible, the acetous acid. For this purpose I prepared what was necessary to obtain oxygenated muriatic acid gas, and the apparatus being arranged the gas was received in a flask containing acetous acid: after more than a sufficient quantity had passed over, the apparatus was unluted, and the acid collected was carefully examined.

As the odour of the oxygenated muriatic acid conceals that of the acetous acid, the product was placed in a capsule, and exposed to a gentle heat. When the greater part of the oxygenated muriatic acid had been destroyed, the acetous acid exhibited no sensible change; its flavour only appeared to be stronger; on account, no doubt, of the muriatic acid which it contained. When combined, like that of the first experiment, with oxide of lead, I obtained a needle-formed crystallization of muriate of lead; and then a second salt, perfectly similar to common acetite of lead, and to that of the first experiment. I am therefore of opinion, especially when I call to mind the numerous experiments of C. Adet, that we may assert that there do not exist two degrees of oxygenation in vinegar. Other experiments would be superfluous, since, as I have already said, chemists are agreed on this point; but that on which opinions differ, is in regard to the different proportions of the carbon: the experiments I made on this subject I shall here describe.

Exp. I. After having brought the acetic and acetous acids, as in the former case, to the same degree of specific gravity, I saturated a given quantity of pure and crystallized carbonate

of potash with the two acids. As it is very difficult to ascertain with certainty the true point of saturation, I shall not speak of the respective quantities employed; they appeared to me to be nearly equal. When I had evaporated these two salts to dryness, they both presented themselves under the form of laminæ, of a flavour equally pungent and hot, and, at last, urinous and alkaline; in a word, no difference could be observed between them.

For the present, I shall call the one *acetite of potash*, and that made with acetic acid *acetate*. I took 576 parts of each of these salts, which had been evaporated and brought as far as possible to the same degree of desiccation, and put them into two glass retorts. They were exposed in a furnace to an equal heat, and during the distillation the same phenomena were observed. Some drops of a liquor slightly coloured, and of an acrid flavour, passed over into the two receivers. This liquor increased in a sensible manner till the end of the distillation; and after exposure to a strong heat there remained in the two retorts a carbonaceous matter, which was treated with boiling water in order to remove by washing all the alkaline part mixed with the carbon. Being separately filtered, there remained in the filters two light residuums known to be charcoal. The two filters which contained them were placed in a stove for 24 hours, and consequently were dried nearly in an equal degree. In this state the two portions of charcoal being weighed, that of the acetite weighed $22\frac{1}{2}$ parts, while that of the acetate weighed only 22. It is seen by this result that the difference is exceedingly small, and that there is reason to believe that the degree of the desiccation of the residuums, or of the salts employed, may be the cause of the small quantity of charcoal which the acetite furnished more than the acetate.

Exp. II. I saturated crystallized carbonate of soda with equal quantities of the acetous and acetic acid, and, by repeated evaporations, I obtained the two salts crystallized in the same manner: they exhibited striated prisms, among which I remarked some hexadral; their flavour was equally pungent and bitter, and their colour white and brilliant.

These two salts, well crystallized, were dried in an equal degree between filtering paper in a place slightly heated, where they were left till the paper was no longer moistened. I then took, as in the former experiment, 576 parts of acetite and acetate of soda, and put them into two retorts: these two salts, exposed to a graduated heat, first became liquid: in proportion as I augmented the heat they became black, and there passed over into the two receivers liquors slightly coloured,

loured, in which floated a kind of oil, the quantities of which were apparently equal. After strongly urging the fire, there remained in the two retorts black residuums, which I treated with boiling water as in the first experiment. The charcoals remaining in the filters were dried in a stove, and then weighed: that produced by the acetate of soda weighed eight parts, and that produced by the acetite eight parts and a half. These two residuums when examined were found to be charcoal, similar to that obtained by the acetite and acetate of potash. This experiment, which on account of the crystallization of the salts ought to be considered as correct, since the proportions of the constituent parts of the salts were found to be the same, explains what we have already announced, why the acetite of potash gave a little more charcoal than the acetate. There is reason, no doubt, to believe that it arose from the greater or less desiccation of the salts, and the manner in which they were burnt, since in the second experiment I found not only the same quantity of charcoal in the residuum of the acetate of soda, but even a small quantity more than in the acetite.

Exp. III Being persuaded, by the results of these two experiments, that the acetous was not more carbonized than the acetic acid, I made as a new proof the following experiment: I put equal parts of sulphate of copper and acetite of lead, both crystallized, pulverized, and mixed, into a retort, which was placed in a furnace over an open fire, joined to the retort an adapter and a receiver, and to the latter a bent tube to collect the elastic fluids. At a very gentle heat there passed over a white liquid, which increased without changing its colour: the fire being continued a considerable time that I might obtain all the acetic acid, no other gas passed over but a part of the air of the vessels; the heat was continued till the glass retort began to enter into fusion, upon which it was taken from the fire. There remained a residuum of a reddish yellow colour, the weight of which was equal to two-thirds that of the mixture employed: the liquid obtained had a perfect resemblance to pure acetic acid, its weight was equal to a third of the quantities employed. A similar experiment is described in the *Annales de Chimie* by C. Badolier; but its object, indeed, was different from that of the present one, it was published merely for the purpose of giving a simple and economical process for obtaining radical vinegar. The author, however, observes very justly that no carbonic acid was disengaged, as was the case in my operation; nor did I obtain a single atom of acid gas. I endeavoured also to discover whether any carbon remained in the residuum; a rigorous examination

examination showed that none existed. This experiment with that above described seems evidently to prove that acetous acid is not more carbonized than acetic acid; for it is certain that, if it contained more, the excess above that necessary to constitute the acetic acid produced would have remained in the residuum.

However conclusive my experiments might be in my own opinion, I ought not to lose sight of that of Chaptal, since it is described in the observations I have quoted. Having distilled then, with a proper apparatus, 576 parts crystallized acetite of copper, there passed over into the receiver a white liquor, which was coloured by the progress of the distillation, and which became green by the copper: this liquid was acetic acid. Before the distillation there were disengaged 65 inches of a gaseous fluid, which, when examined, was found to be a mixture of carbonic acid gas and hydrogen gas: the residuum, of a blackish brown colour, being treated with muriatic acid, the latter dissolved the oxide of copper, and left a powder slightly black, which was collected on a filter: this black matter, when edulcorated, dried, and then subjected to chemical examination, was found to be charcoal, the weight of which was 22 parts. As this result was perfectly similar to that described by the chemists, it was necessary to examine whether the consequences they deduced from it were very exact; for it may be asked, why the acetous acid was decarbonized under these circumstances alone, and not in that of which I here give an account. Presuming that I should find a solution of this question by means of comparative experiments, I made the following:

I prepared acetate of copper by dissolving oxide of copper in acetic acid diluted with water, and by proper evaporation I obtained a salt crystallized in octaedra. Of this salt, when dried to the same degree as the acetite of copper, I introduced 576 parts in a retort, and exposed it to heat: it exhibited exactly the same phenomena as the acetite. I obtained nearly equal quantities of hydrogen gas and carbonic acid. The liquid, when collected in the receiver, had the same colour; when examined comparatively with that produced by the acetite, it was impossible to find any difference between them. The residuum, of a blackish brown colour, was treated with muriatic acid, which dissolved the oxide of copper, and left a black matter, which floated on the liquid. This matter, separated by means of the filter, and dried on a stove, being subjected to analysis, was found to be charcoal perfectly similar to that of the preceding experiment: it had abso-

lutely the same weight as that produced by the distillation of the acetite of copper; that is to say, it weighed 22 parts.

From this result it is evident that the charcoal obtained by distilling acetite of copper does not arise from the acetous acid containing more than the acetic acid, since the latter, when subjected to the same operation, gives an equal quantity. In my opinion, that produced in these operations does not arise from a portion of the acid decomposed, the quantities of which ought to vary according to the progress of the decomposition. Experience authorizes me to believe that the difference which may exist between the two acids in question does not depend either on the proportions of the oxygen or of the charcoal. I superintended some trials, not on the constituent principles of vinegar, but on vinegar itself, and the kind of mucilage with which it is accompanied. Before I describe this labour I must not forget to observe that C. Chaptal furnished me with the idea by his observations on the acetic and acetous acids. He says on this subject, that the acid of vinegar may be considered as existing primitively in a state almost saponaceous; which diminishes its action, and weakens its properties.

After having ascertained, by some experiments, the existence of this extractive or mucilaginous principle, I endeavoured to separate it. Distillation as well as filtration were found to be imperfect means: my vinegar always contained mucilage: it remained nearly the same. However, by combining it, after distillation, with alkaline bases, it deposited very little of that flaky matter called *extractive matter*. Having not forgot, during these researches, the opinion of C. Adet, who says that the quantity of water might occasion the difference between the acetous and acetic acids, comparing the observations of these two chemists, I made some experiments, which appear to me to be interesting; but I shall describe that only which appeared to me to be most conclusive.

Entertaining the idea that there was no difference between the acetous acid and the acetic, but a larger quantity of water, and an extractive or mucilaginous matter, I endeavoured to find the proper means of separating the former from it, and bringing it to the state of acetic acid. For this purpose, I prepared muriate of lime, which I strongly calcined. After reducing it to powder, I put it into a tubulated retort, and poured over it common acetous acid with an apparatus proper for receiving the products. The retort, which was on a sand-bath, being exposed to a gentle heat, the matter immediately
entered

entered into ebullition, on account of the heat produced by the calcareous salt in solidifying the major part of the water contained in the vinegar. During this distillation there passed over a clear liquor, which fell in striae on the sides of the adaptor, and which was collected in a receiver. This liquor totally changed in appearance; had a stronger and more pungent flavour than the acetous acid employed, and its odour was sharper and more penetrating: one might have taken it for diluted radical vinegar. This product, when subjected to a second distillation with dried muriate of lime, acquired a little more strength: after a third and a fourth rectification, a transparent liquor was obtained, of an odour and flavour very similar to that of common acetic acid. During these rectifications, not an atom of charcoal was deposited. To ascertain whether the acid obtained, in losing a great quantity of its water, had lost also the mucilaginous matter which accompanied it, I saturated a portion with crystallized carbonate of potash: in vain did I add an excess of this salt, for nothing was precipitated; whereas common acetous acid, by a similar saturation, precipitates a flaky matter, which is collected in the bottom of the vessel employed.

Though the strong and penetrating odour of the acetic acid obtained by the simple means above described was sufficient to induce me to conclude that it was really similar to that obtained from metallic acetites, I endeavoured to ascertain this fact by a new experiment. For that purpose I took equal parts of this radical vinegar and of alcohol to try to make acetic ether: the attempt was not unsuccessful; and, by Pelletier's process, I obtained an ether perfectly agreeable, and having an exact resemblance to the acetic ether found in laboratories. I must here observe, in speaking of the acetic acid obtained by rectification alone with muriate of lime, that it is far more agreeable than the acetic acid produced, for example, by the distillation of metallic acetites; and especially when they are diluted with water. The latter then assumes a disagreeable empyreumatic odour, and an oily flavour, arising from a portion of acetic acid which has been decomposed by the heat, and which has given birth to a kind of oil which the acid retains in combination. The other, on the contrary, passes only to the state of common acid of vinegar by preserving an agreeable pungency. Before any conclusion can be drawn from the preceding results, I must not forget to mention an interesting fact remarked by C. Pontier. This able chemist and mineralogist, who discovered the chromate of iron in France, sent, about eight

months ago, to C. Vauquelin a flask containing a liquor obtained by the distillation of vinegar on a large scale. As C. Pontier was not ignorant that the first product of the distillation of the acetous acid is aqueous, he separated it from that which followed. After uniting several phlegms of the same kind, he was much astonished to find that these phlegms had a very agreeable aromatic odour: having discovered, by preliminary trials, the properties which gave this product a similarity to ethereal liquors, he rectified all the supposed phlegms, and by these means obtained a peculiar ether, which Vauquelin found to be acetic ether completely rectified.

From an identity so striking in the experiments above described, it is evident that there exists no difference between the constituent parts of the acetic and acetous acids; that the water, and mucilaginous or extractive matter, are the only two apparent differences observed in these two acids; and, consequently, that there exists only one acid of vinegar, which being at its maximum of oxygenation, ought, according to the chemical nomenclature, to be distinguished by the name of *acetic acid*. In future, therefore, there will not exist two different kinds of salts, either by the combination of distilled vinegar or of radical vinegar; they will hereafter be so many products, the names of which must be the same; and hence *acetates* will exist, and not *acetites*.

IV. *Description of a new Implement called a Cultivator.* By
Mr. WILLIAM LESTER, of Northampton*.

SIR,

AS the health and luxuriance of corn depend, in a great measure, on the pulverization of the soil previous to the seed being sown, the Society of Arts will, I am persuaded, give every encouragement in their power to the introduction of any implement that promises an abridgement of labour; and as all tenacious soils are pulverized in the best manner in dry weather, when their particles are the most disjointed, and their contact broken, the propriety of taking the advantage of working them in that state will be obvious: and at the same time it follows, that an improved implement for the abridgement of labour would be a desirable thing in a climate like England, where the seasons are so uncertain.

* From the *Transactions of the Society for the Encouragement of Arts*, &c. vol. xix.—The Society's silver medal was awarded to Mr. Lester for this invention.