

NOTES AND CORRESPONDENCE

EFFICIENCY OF AIR DRYERS

Editor of the Journal of Industrial and Engineering Chemistry:

In his letter of Aug. 17th [THIS JOURNAL, 8 (1916), 853], Prof. Lewis takes exception to my statement that the consumption of heat in an air dryer is definite and that practically 1120 B. t. u. are required to evaporate 1 lb. of water. "In order to convince me of my error" he proceeds to describe an ideal dryer capable of evaporating 1 lb. of water with a considerably smaller expenditure of heat than I postulated.

Before examining the workings of this machine, let me say that as far as convincing me of error is concerned, the whole matter is quite beside the mark. I did *not* say this was the requirement "under even the most ideal conditions," the phrase in quotation marks being Prof. Lewis' and not mine. I believe that a careful perusal of my letter of June 24th will make it perfectly clear that in stating the heat consumption at 1120 B. t. u. I referred solely to single effect apparatus. Moreover, I specifically stated that it requires "practically" this amount and that I spoke in round (approximate) numbers.

I cannot understand how the performance of an ideal, multiple-effect apparatus as described, has any relation to my figures as to the requirements of a practical single-effect machine and I am somewhat surprised that such an illustration should have been presented to convict me of error, the more so as Prof. Lewis himself appeared to recognize in a later paragraph of his letter that I was discussing solely single-effect apparatus and admits that he has to invoke the aid of multiple-effect to show that I was mistaken. Certainly, I have not at any time questioned or failed to recognize the larger possibilities of heat utilization in multiple-effect apparatus.

It will be seen, therefore, that I was not "mistaken" in my original statement, which is substantially correct in the form and in the relation in which it was made.

But while Prof. Lewis' ideal double-effect dryer quite fails in its purpose of either convicting or convincing me of error, it is intensely interesting in other ways and well worthy of a little further analysis.

The expenditure of heat for evaporation in this ideal dryer is calculated by Mr. Lewis as 1104 B. t. u. per lb. of water as regards the first part of the machine—that is to say, as long as it is operating single-effect. This figure, correct for the temperature limits assumed, is the only one that can properly be compared with my approximate figure, and, of course, is in substantial agreement with it. It is 1.5 per cent less than the quantity I stated as a "round number" and without defining the temperature limits.

The first assumption regarding the ideal dryer is "a material that may be dried at temperature of 160° without injury." It is then specified that the air *leaving* the dryer has a temperature of 160°, a dew point of 140° and carries 0.1514 lb. water per lb. of dry air. In order to accomplish this result each pound of air passing through the dryer has to evaporate 0.1462 lbs. water (0.1514 - 0.0052). To do this the air must give up $0.1462 \times 1102 = 161.2$ B. t. u. This quantity of heat is capable of heating the 1 lb. of air $162/0.2373 = 683^\circ$, and as the air is to leave the dryer at a temperature of 160°, it must enter at a temperature of $160 + 683 = 843^\circ$. This temperature is well above the melting point of zinc.

It is proper to say that while the required amount of heat would result in this high initial temperature, if supplied at one time, it is nevertheless conceivable that no such high temperature need obtain if the heat is supplied in small increments. Indeed, in order to comply with the stipulated requirements of our ideal machine (not to exceed 160°), it would be abso-

lutely necessary to supply the heat in differential increments. The method of supplying heat in increments is indeed practiced in certain forms of drying apparatus, usually by re-circulation of the air, and to some extent makes possible the use of a high discharge temperature, with a corresponding economy in heat, without necessitating an inconvenient, or in many cases impossible, initial temperature.

It is stated that "it is theoretically possible to reduce the heat consumption * * * to 1102 B. t. u. per lb. of water evaporated." This is literally correct as expressing the heat used in evaporation, but it must not be overlooked that a further expenditure of heat is necessary to raise the temperature of the air from 50° to 60°. As 1 lb. of air is to carry off 0.1462 lb. water, each pound of water requires 6.8 lbs. of air, which must be heated from the inlet temperature of 50° to the discharge temperature of 160° and requires $110 \times 6.8 \times 0.2373 = 177$ B. t. u.

Considering the second dryer, I think we shall find that Prof. Lewis' figures on this are not entirely correct. He states: "It is therefore possible in this dryer to evaporate 0.1223 lb. of water per lb. of dry air." This statement is at the least elliptical, for under the specified conditions 1 lb. of the air supplied to the second dryer (at 90°) could not possibly evaporate this much water. To do so the air must give up 132 B. t. u., or would have to cool to absolute zero. Doubtless what was intended to be conveyed was that the heat recovered from 1 lb. of the discharge of the first dryer would evaporate this much water. This heat (132.3 B. t. u.) would, by assumption, be distributed in 13.8 lbs. of air; that is to say, the 132.3 units recovered from the first dryer would heat 13.8 lbs. air from 50° to 90°, or, in other words, 13.8 lbs. air in the second dryer is the equivalent of 1 lb. discharged from the first.

Now in order to perform the evaporation postulated by Prof. Lewis, it is manifest that this air must be cooled to 50°; *i. e.*, it must give up all the heat supplied to it. But long before this occurs the air would be completely saturated, and it is therefore impossible that the entire 132 units recovered in the first effect could be utilized in evaporation in the second effect as assumed by Prof. Lewis. To give the complete figures, in order to give up 132.3 B. t. u., 13.8 lbs. of air must cool at 40° or to a temperature of 50°. If completely saturated at 50° the 13.8 lbs. of air can contain $13.8 \times 0.0075697 = 0.10446$ lb. of water. But by hypothesis, the 13.8 lbs. of air start with a content of $13.8 \times 0.0052 = 0.07176$ lb. water and consequently can absorb only $0.10446 - 0.07176 = 0.0327$ lb. instead of the 0.1223 lb. predicted by Prof. Lewis.

The maximum possible result would be obtained by a discharge temperature for the second dryer of approximately 63°, that is cooling the air 27°, under which conditions it would give up for evaporation 89.3 B. t. u., theoretically capable of evaporating 0.0844 lb. instead of Prof. Lewis' 0.1223 lb. The discharged air would then leave the machine completely saturated.

In his reference to the discussion of 1910, Prof. Lewis makes some remarks that I believe are not warranted by the record. In that discussion I did not at all question the published figures, but only the use of the word "efficiency" as deduced from those figures. In the original report Mr. Ruggles quite correctly stated the heat necessary to evaporate the water, using, by the way, the figure of 1122 B. t. u. per lb., which is practically the same which I am so grievously mistaken in using.

The thing I questioned was the propriety of *crediting* the machine with the large amount of heat carried off in the dried material. In the report in question this entirely wasted heat

is made to contribute to the "efficiency" of the dryer. My sole contention was that the efficiency of a drying machine was in no way a function of its capacity for heating sand. I did indeed point out that the published figures required that about 33 per cent of the discharged water must be in the form of mist or liquid water, and I said, "if this is possible it opens up a way to extraordinary possibilities in drying economies." This does not appear to me to differ from Prof. Lewis' attitude of "unlikely but not impossible."

The theory of re-condensation and fog formation has been advanced a number of times. I agree absolutely with Prof. Lewis about it, that it is by no means impossible, but is unlikely, and that the conditions under which it can occur are unusual. Personally, in a limited experience, I have not come across an instance where any large supersaturation could be positively demonstrated, even under conditions favorable to it.

In conclusion as to the dryer question, I hold no special brief for the retention of the word efficiency in connection with the performance of these machines although to me personally it is a very useful tool. It is very much easier for me, for instance, to think of a turbine as a more efficient machine than a water wheel, than it is to remember their respective outputs of power per unit of energy supplied. Very likely this is largely a mental habit only, but the consideration of machines on their efficiency expressed as a percentage of an ideal duty is pretty strongly rooted as a custom.

Neither am I deeply concerned as to whether air drying and evaporation are treated separately or as one subject. Prof. Lewis feels very strongly about this, so by all means let us treat them as one. Frankly it appears to me quite too trivial to develop any strength of feeling about. My suggestion to treat them separately was largely in the interests of convenience and expediency. Take only the matter of indexing. Prof. Lewis' original letter setting forth the theoretical possibilities of multiple evaporation contains very much of interest, but the student of the subject will seek it in vain under the heading of "evaporation" and will be indeed fortunate if he searches under the caption of "air dryers" for matter relating to vacuum, multiple-effect evaporators.

I appreciate Prof. Lewis' elucidation of the point that I was unable to understand about the evaporation of saline solutions. Of course, if the vapor temperature is the same as that of the boiling solution the difficulty disappears. As Prof. Lewis says, my understanding was that the temperature of the vapor is that of the pure solvent. This is stated to be the case by very many authorities, including Rankine, Box, and many others, and I supposed was an accepted and established law. I have happened on only one authority to the contrary, namely Mendeléeff, who states that the temperature of the vapor is the same as that of the solution from which it is generated. He proceeds to give the boiling temperatures of several solutions of salts and rather curiously adds, "if the temperature of ebullition be determined by immersing the thermometer in the liquid itself," * * * This qualifying phrase appears to indicate that a correct thermometric indication cannot be obtained at all in the vapor.

I was so much interested in the divergent statements of authorities on this point that I endeavored to satisfy myself by a very crude experiment. I took a solution of caustic soda, boiling approximately at 120° C., in a large flask, starting with the thermometer bulb immersed in the liquid. When very actively boiling and with the thermometer practically stationary, I raised the bulb just clear of the liquid, when the temperature instantly dropped about 12°, and then remained fairly steady. In this case the thermometer bulb was at a temperature of 120° and the drop could hardly be due to condensation, and was in the center of the flask having a surrounding jacket of vapor of about 3 in. on all sides.

I then withdrew the thermometer and wiped the bulb clean of caustic soda solution and immediately replaced it in the vapor, when the indication was 100°, very slowly rising to about 110°. The 100° mark was reached almost instantly, while the subsequent rise to 110° was very slow, requiring many minutes. The same result was obtained by rapidly washing off the thermometer with boiling water and plunging into the vapor while still hot, so as to eliminate as far as possible condensation on a cold bulb. Again the temperature of 100° was reached, almost instantly followed by a very slow rise as before.

It will be observed: (1) that the clean bulb (free from the solution) in all cases indicated 100° in the vapor at first and that the subsequent very slow rise was coincident with the deposition of the boiling solution on the bulb, from the bombardment from the actively boiling surface; (2) in no case could a reading be obtained anywhere near the boiling point of the liquid, but a condition of equilibrium occurs about midway between the temperature of ebullition and 100°.

BAYONNE, NEW JERSEY
September 10, 1916

W. E. WADMAN

ANALYSIS AND MANUFACTURE OF RED LEAD

Editor of the Journal of Industrial and Engineering Chemistry:

It may be of value to those interested in the analysis and manufacture of Red Lead to know that a mixture of dilute nitric and tartaric acids is an excellent solvent for that substance. The hot solution of tartaric and nitric acids dissolves Red Lead almost instantly. The proportion of tartaric to nitric acid is very small. A convenient dilute solvent for Red Lead is prepared by dissolving 5 g. of crystallized tartaric acid in some water, adding to it 200 cc. concentrated nitric acid and the whole diluted to 1 liter. Stronger solutions for larger samples are made by increasing the nitric and tartaric acid contents; thus, 100 cc. of a solution containing 300 cc. concentrated nitric acid and 15 g. of crystallized tartaric acid per liter will dissolve 20 g. of Red Lead. Too strong nitric acid must not be kept with tartaric acid as the latter will decompose on standing.

The solvent should be used hot, pouring it over the sample and stirring; it is very much more efficient and also cheaper than hydrogen peroxide which is in general use for Red Lead analysis.

CLEVELAND, OHIO
October 6, 1916

GREGORY TOROSSIAN

DETERMINATION OF SMALL AMOUNTS OF ALCOHOL AND WATER IN ETHER FOR ANAESTHESIA—A CORRECTION

In our article printed under the above title in *THIS JOURNAL*, 8 (1916), 807, an error occurs in Table I. The fraction

$$\frac{\text{Wt. ether} + 1.040533 + 0.9970795}{49.8444 + 1.040550 + 0.9970770}$$

should read:

$$\frac{\text{Wt. ether} + 1.040533 \times 0.9970795}{49.8444 + 1.040550 \times 0.9970770}$$

Also, Heft VIII in footnote 2, page 807, should read Heft VII.

ST. LOUIS
September 18, 1916

E. MALLINCKRODT, JR., AND A. D. ALT

A STUDY OF THE EFFECT OF STORAGE ON MIXED PAINTS—CORRECTION

In the paper by Mr. Ware and myself as printed in *THIS JOURNAL*, 8 (1916), 879, the last line, second column, Table I, should read " Na_2CO_3 , . . ." instead of " Na_2CO_3 , 3 g."

UNIVERSITY OF MICHIGAN
ANN ARBOR, October 19, 1916

R. E. CHRISTMAN